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# TRANSACTIONS

OF THE

# AMERICAN

# PHILOLOGICAL ASSOCIATION.

## 1874.

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# CONTENTS.

I. On the Prepositions in the Homeric Poems.
By Professor WILLIAM S. TYLER. . . . . 5

II. On the Formation of the Tenses for Completed Action in the Latin Finite Verb.
By Professor ALBERT HARKNESS. . . . . 14

III. On an English Vowel-Mutation, present in CAG, KEG.
By Professor S. S. HALDEMAN. . . . . 26

IV. On a Passage in Homer's Odyssey (x. 81–86).
By Professor LEWIS R. PACKARD. . . . . 31

V. On Numerals in American Indian Languages, and the Indian Mode of Counting.
By Dr. J. HAMMOND TRUMBULL. . . . . 41

VI. On the Distinction between the Subjunctive and Optative Modes in Greek Conditional Sentences.
By Professor J. B. SEWALL. . . . . 77

VII. On the Age of Xenophon at the Time of the Anabasis.
By Mr. CHARLES D. MORRIS. . . . . 82

VIII. Φύσει or Θέσει—Natural or Conventional?
By Professor WILLIAM D. WHITNEY. . . . 95

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TRANSACTIONS

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## I. — *On the Prepositions in the Homeric Poems.*

BY WILLIAM S. TYLER,

WILLISTON PROFESSOR OF THE GREEK LANGUAGE AND LITERATURE IN AMHERST COLLEGE.

THE parts of speech, as they are called by grammarians, are a classification, founded in the nature of language, but at the same time inevitably more or less artificial and imperfect, of the different kinds of words in their relations to each other and to the sentence. No one has ever been able to give a perfect definition of any one of these parts such as would bear the questionings of a Socrates, or such as to include every thing that belongs to it and exclude every thing else. The *number* of classes or parts of speech which grammarians have made has varied widely at different times, and has not yet been settled beyond dispute. Two different classifications have come down to us bearing the name and clothed with the authority of the great Greek philosopher who was the founder of the science of classification — both marked by his fondness for simplicity and excessive generalization — one of which makes but three parts of speech (grammatically), viz.: verbs, nouns, and connectives, and the other makes four, viz.: verbs, nouns, articles, and connectives (*ῥήματα, ὀνόματα, ἄρθρα, σύνδεσμα*). Both these are natural classifications founded in the nature of the sentence, and answering to the logical



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distinctions of the subject, the predicate, and modifiers, of which distinctions there will, of course, be either three or four according as we include *all* modifiers in one class, or distinguish modifiers of the verb from modifiers of the noun.

But these classifications are too general to satisfy the demands of most grammarians; and they easily admit of further division and subdivision. Hence the number of parts of speech was gradually increased by the Greek philosophers, particularly the Stoics, who were especially given to grammatical studies, till nine became with them, as it has usually been with modern grammarians, the accepted number.

Some Roman grammarians in the time of Quintilian, as we learn from that judicious scholar, went on still further dividing and subdividing till they made ten, eleven, or twelve parts of speech in the Latin language, without the article which is wanting in that tongue. Quintilian himself disapproved of these later and subtle distinctions, leaving undecided however the question whether all *names* should be classed together, or whether they should be distinguished into substantive and adjective nouns.

Whatever may be the classification adopted, and however many or few the classes may be, there will always be words which cannot be referred absolutely or exclusively to any one class; either because they do not answer exactly to the definition of any one, or because they perform the office now of one part of speech, and now of another, and now they subserve the uses of more than one at one and the same time. Thus words which are usually parsed as adverbs, in all languages, often perform the office of conjunctions also, since they not only modify the verb of the clause in which they stand, but also connect the clause with some other part of the sentence.

The process by which the same words, or even whole classes of words, so change their use and office in course of time as to become different parts of speech from what they once were, is one of the familiar and one of the most interesting and instructive phenomena in the history of language. Thus substantives easily become adjectives and

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adverbs by a mere change of relation to the principal words of the sentence, while verbs, sometimes a source of supply for adverbs and prepositions, are recognized among the principal fountains from which conjunctions are derived. In other words, and as a matter of course, nouns become adjectives or adverbs whenever, ceasing to be themselves the principal subject or object of a proposition, they attach themselves as mere modifiers to other principal words; and verbs become adverbs, prepositions, or conjunctions, whenever, ceasing to be themselves the predicate of a proposition, they only modify or connect it. So that this process of transformation resolves itself into little more than a change of emphasis — at any rate it involves a change of emphasis, not less than a change of relation. Again, that is a most curious process, by which, simply by growing less and less emphatic, the demonstratives of so many languages have become first relatives and then articles or conjunctions; as, for instance, the English *that* (and so the Greek ὅτι and the Latin *quod*) was first a demonstrative, e. g.: "I knew THAT (*viz.* which) he said"; then, by a little falling off of the emphasis, a compound or simple relative: "I knew *that* he said"; and then, by losing all emphasis, a conjunction merely connecting the two clauses: "I *knew* that he said." By a similar process the definite article in English, as also in Greek, in German, in Italian, in French, and in the modern languages generally, was made from the demonstrative growing gradually less emphatic; and then, to supply its place in each of the languages, a lengthened and strengthened form was taken up for the demonstrative, in which the demonstrative element (*t* or *d*) was repeated at the end as well as the beginning of the root (compare the English *the* with *that*, the Greek ὁ, ὅς, or τός with οὗτος, the German *der* with *dieser*, etc.).

Of all the parts of speech, the preposition has been the most unfortunate in its nomenclature, being the only part of speech whose name expresses nothing of its nature or office, but merely its position with reference to the verb of which it is a prefix or the noun which it precedes; and that position, usual indeed, but by no means universal, still less essential

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or founded in the nature of things. If it must be named from an accidental circumstance instead of an essential characteristic, its position is indeed so generally a *pre-*position as perhaps to justify the name on the principle of logicians: *a potiori nomen fit.* And usage has so sanctioned the name that it cannot now be easily changed; for not only did the Greeks originate the name πρόθεσις, and the Romans translate it into *praepositio*, and the English into *preposition*, but even the Germans, whose grammatical nomenclature is usually so significant and so just, call this part of speech *die Präposition* and *das Vorwort*, although they sometimes also call it *das Verhältnisswort*, and thereby express its most essential characteristic.

The proper prepositions are not numerous in any language, scarcely a score in Greek, about the same in Sanskrit, and but little more than that number in Latin and the modern European languages. They are primitive words with monosyllabic roots, which reappear, with only accidental and euphonic variations, in all the branches of the Indo-European family. In Greek, however, the majority of them have been made dissyllabic by the addition of a vowel, which vowel receives the accent except when the preposition becomes a post-positive, in which case it suffers anastrophe. This annexation of a vowel illustrates the musical superiority of the Greek over other languages, as for instance the Latin, the vowels being the musical and the consonants the significant elements in language; and the fact that the accent regularly rests on this comparatively unsignificant syllable of the preposition, a syllable which disappears in the Latin and English equivalents, is itself sufficient to show that the Greek accent was not mere stress, but rather tone or inflection. For the most part the prepositions seem originally to have expressed such essential and fundamental relations of place and of motion as *up* and *down*, *over* and *under*, *to* and *from*, *in* and *out*, *on* and *off*, *before* and *after*, *at* or *near*, *through* or *amid*, *about* or *around*. From these space-relations they were easily transferred by analogy to express the relations of time, and then, by metaphor or other figure of

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speech founded on some nearer or more remote resemblance, they came gradually to denote all the varied relations of human action and thought. Of course no class of words can be more interesting, none more instructive to the philologist or the metaphysician, shedding so much light as they do and must, not only on the origin and progress of language, but on the fundamental laws of thought, and illustrating our intuitive conceptions even of the material universe.

It has been the almost unanimous opinion of philologists that the class of words which are commonly called prepositions were originally and properly adverbs. A class of words which originally signified *action* and *motion* would naturally be followed or accompanied by a class of words denoting the *direction* of motion and the *relations* of actions; in other words, verbs would not long exist without adverbs. But inasmuch as motion naturally ends in some place, and action terminates on some object, or tends to some result, when thought came to be more fully expressed, the same words which denoted the direction of motion and the tendency of action would naturally, not to say necessarily, denote also the *relations* between such motions or actions and the places, persons, or things affected by them — in other words, verbs and nouns could not be used to any great extent without adverbs being gradually converted more or less into prepositions to show the relations between them.

It becomes then an interesting question whether this theory of the normal rise and growth of prepositions is confirmed by facts. Are there traces of the process still remaining in the early literature of nations, or does it go back to a period antecedent to all extant literature — a period of which we have no other record but language itself? Do the earliest extant productions of Greek literature — for example, the Homeric Poems — exhibit to us the class of words of which we speak as fully adverbs, or fully prepositions, or in a transition state between adverbs and prepositions? All the authorities on Greek grammar, American, English, and German, agree in recognizing a marked peculiarity in Homer touching the use of this class of words, and differ only as to

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the extent in which they acknowledge it and the interpretation which they put upon it. Some regard them as already in reality and in the main prepositions, although used as adverbs more frequently by Homer than by later authors; and they treat what is called *tmesis* as a real separation of the preposition from the verb. Such was the view generally taught in the grammars of the last generation and still accepted by some grammarians of the old schools. The more recent authors on Greek grammar, however, generally recognize this class of words in Homer as partly adverbs and partly prepositions, partaking more or less of the properties of both, and, as some distinctly affirm, in the stage of transition from the one to the other; and generally, although not unanimously, they explain what is called *tmesis*, not as an actual separation of the preposition from the verb in a proper compound, but rather as an antecedent and more primitive stage of the language, in which the preposition was an adverb, although on its way towards composition with the verb.

In order to come at a more accurate knowledge of the facts in the *usus loquendi* of this class of words, I recently struck a trench through several successive strata of Greek literature somewhat as Dr. Schliemann has cut through the successive strata on the site of ancient Ilium, although my researches have not been as thorough as his, nor did I find or expect to find any such magnificent results. But I submit a brief report of my observations, or excavations if any choose to call them so. I went through first with the third book of the Iliad — a book which I am accustomed to read with almost every class, because it is a favorite book with me and generally proves equally interesting to my pupils — taking up each of the proper prepositions and noting its various uses, as a preposition preceding the substantive, as a prefix to the verb, as a post-position following the substantive, and as a separate word not connected with either the substantive or the verb. I noted also the comparative frequency of the occurrence of the simple verb and the verb compounded with a preposition. I then went through the seventh book of the Odyssey in the same way, examining and recording the same particulars. I

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then proceeded to examine in the same way a specimen portion successively of Sophocles, Herodotus, and Xenophon. I had thus gathered up statistics of the remains, so to speak, of successive strata of Greek literature, which I could compare numerically with each other. I shall not trouble the Society with all the details of these statistics. But I may say in brief, that they showed clearly enough a general and constant *diminution* of the separate and unprepositional use of this class of words from the earliest extant specimens of the language in the Homeric poems to the perfection of the Attic form and style in Xenophon's Anabasis, and a corresponding relative *increase* of their use both as prefixes to verbs and as prepositions governing substantives. Of the 251 instances in all in which words of the class called prepositions occur in the third book of the Iliad, 10 per cent. occur separate from either substantives or verbs, and 9 per cent. more come after their substantives, thus making 19 per cent. that are not strictly prepositions; while 81 per cent. occur in the normal state of prepositions, 47 per cent. before substantives and 34 per cent. in composition with verbs. In the seventh book of the Odyssey, there is the same percentage (10) of separate occurrence, and the only change (and that *perhaps* accidental) is that there are only 6 per cent. of post-positives, while there are 84 per cent. of normal prepositions, of which, however, a larger proportion, namely, 53 per cent., precede substantives and 31 per cent. are prefixed to verbs. In Sophocles, there are only 3 per cent. of separate words of this class (and these more manifestly cases of *tmesis*) and 3 per cent. also of post-positives, making only 6 per cent. in all of unprepositional use, while 94 per cent. are prepositionally used. A much larger proportion, however, 59 per cent., have now entered into composition with the verb, leaving 35 per cent. standing before substantives. In Herodotus, the unprepositional use has disappeared,* while 53 per cent. are in composition with the verb and 47 per cent. stand before substantives. In

* That is, in the passage of several pages which I used as a specimen. There are not wanting sporadic instances of *tmesis* and adverbial use, e. g.: ἀπὸ δ' ἔθανε, vi. 114; μετὰ δέ, vi. 120.

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Xenophon, the only change from the usage of Herodotus is that a still larger — a *considerably* larger — proportion of this class of words have entered into composition with the verb, namely, 59 per cent., while the remaining 41 per cent. stand before substantives.

A comparison of the compound with the simple verbs in this series of writers shows a corresponding change keeping regular pace with the progress of the language. In the Iliad and Odyssey only 14 per cent. of all the verbs are compounded with prepositions; in Sophocles, 26 per cent.; in Herodotus, 32 per cent.; and in Xenophon, 36 per cent. An examination of the first chapter of the Acts of the Apostles showed a still greater proportion of compound verbs, namely, 40 per cent.* Moreover, there is in Luke a marked increase of the disposition to *repeat* the *same* preposition, using it *both* before the substantive and also in composition with the verb, which usage is not unfrequent in Xenophon,† but is rare in Herodotus, while there is scarcely a trace of it in Homer or Sophocles.

It should also be observed that in Homer, where the preposition *does* enter into composition with the verb, it seems to retain more of its original adverbial force, whereas in the later Greek it perhaps gradually changes the meaning of the word, or perhaps loses its force so that the compound differs less and less from the simple verb; hence the naturalness, not to say the necessity, of sometimes reinforcing it by the *repetition* before the substantive of the same preposition which appears in composition with the verb. A good illustration of this peculiarity of Homeric usage in the verb compounded with a preposition may be seen in the 12th verse of the third book:

Τόσσον τίς τ' ἐπιλεύσσει ὅσον τ' ἐπὶ λᾶαν ἵησιν:

"And one sees on (sees over, sees ahead) only as far as on (over, ahead) he throws a stone;"

---

* A subsequent examination of specimen passages in Plutarch and in Tricoupes, the modern Greek historian, discovered a farther increase, namely, 41 per cent. of compound verbs in the former and 43 per cent. in the latter.

† In such constructions as εἰσ-(or ἐμ-)βάλλειν (or βαίνειν) εἰς, ἐκβάλλειν ἐκ, συστρατοπεδεύεσθαι σύν, etc., etc.

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where ἐπιλεύσσει does not mean "to oversee," "to overlook," or "to live to see," as such compounds do in later Greek, but "to see over," and the ἐπί in composition has just the same adverbial force which the same preposition has in the last clause of the verse, where it stands by itself, being separated, as some would say, from the verb ἵησιν by *tmesis*, but, to speak more properly, and as grammarians would now generally say, used as an adverb.

We have a similar use of the same preposition in the 277th verse of the same book:

> Ἠέλιος Θ', ὃς πάντ' ἐφορᾷς, καὶ πάντ' ἐπακούεις:
> "And thou, O Sun, who seest over all and hearest over all;"

where the ἐπί has essentially or very nearly the force of an adverb of place, whereas in later Greek it gives to the same composite verbs the modified meaning "to oversee," and "to overhear" or "to listen to." The difference is analogous to that between *dúrchreisen* and *durchreísen* in German, of which the former means "to travel *through*" literally and emphatically, while the latter means only "to travel *over*" or "to traverse"; and in the former of which the preposition is separable from the verb, while in the latter it is inseparable. This is another illustration of the power of emphasis or accent to modify the meaning and use of words.

As a counterpart to the Homeric preference of the simple over the composite verb, Homer uses also the *noun* without a preposition — without any governing word — more frequently than it is used in later Greek, and that (as we might expect) the genitive or dative to denote primarily place, or secondarily some other relation which can easily be conceived as analogous to the space-relation. And in many instances where the preposition does precede the substantive or stands between it and the verb, it seems to hover between the office of a preposition and that of an adverb.

Facts then seem to justify the theory which is accepted by most modern writers on Greek Grammar, and to show not only that prepositions were originally adverbs, but that in the Homeric poems we see them in a transition state corresponding with the transition state of the pronouns and the generally



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flexible and formative condition which characterized the language at that early and formative period of Greek history. Homer is peculiarly worthy of the study of the philologist not less than the general scholar, as a faithful voucher and true witness, not merely of the state of society, government, morals, and manners of the heroic age (whence Frederic Schlegel fancies he received the name of Ὅμηρος, a pledge or voucher), but also for his unconscious testimony to, or representation of, the phenomena of language in that primitive period, when it was still flexible in its form and changeful in its features, but surpassingly rich in material and expression, and as far from being barbarous or savage, as were those elegant works of art which Dr. Schliemann found in the lowest strata of his excavations. The stone age in the language and literature, as in the art and civilization, of Greece was not before the golden age, but long after; it was not before, but long after, the poems of Homer.

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## II. — *On the Formation of the Tenses for Completed Action in the Latin Finite Verb.*

By ALBERT HARKNESS,
PROFESSOR OF GREEK IN BROWN UNIVERSITY.

In investigating the system of verbal inflections, as found in the Indo-European family of languages, the science of Comparative Philology has encountered few problems which have hitherto proved more difficult of solution than that presented in the origin and formation of the Latin perfect. Most of the prominent leaders in the new school of Philology — Bopp, Curtius, Schleicher, Corssen, and, more recently, Westphal and Merguet — have given it careful thought, but the problem still remains unsolved. Many valuable facts have indeed been collected by these eminent scholars, and much light has been thrown upon many obscure points; but no explanation has yet been proposed which can be said to account fully for all the facts in the case; no theory devised which has met with general recognition among the scholars

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of the world. The question therefore of the origin and formation of the Latin perfect is unfortunately still an open one; and though its difficulty might well deter us from entering upon so unpromising a discussion, its great importance, from its vital connection with the whole subject of Comparative Philology, imperatively demands that we should still continue to investigate it. Each discussion may in its turn throw some new ray of light upon it, until at length its secret, we may hope, shall be exposed to the full light of day. If therefore this paper should succeed, even in the smallest measure, in preparing the way for the final solution of this difficult question, my attempt will not have been made in vain.

The Latin, Greek, and Sanskrit undoubtedly inherited from the mother tongue of the Indo-European family the power to express completed action by means of *reduplication*, and to create new tense-forms through the help of *auxiliary verbs*. The Latin, in its treatment of tenses for completed action, is distinguished from the Greek and the Sanskrit by a freer use of compound tense-forms to supply the place of the reduplication. While in the Greek λέλυκα, πέποιθα, and in the Sanskrit *babhúva*, *ćakára*, the reduplication is indispensable, in the Latin *amavi*, *audivi*, *monui*, *rexi*, no trace of it appears. The Latin has indeed retained a few reduplicated perfects, as *cecĭdi*, *cecĭni*, *pepĕri*, but in comparison with the vast number of compound forms, these appear but exceptions to the general rule.

But before we enter upon the discussion of the Latin perfect, it seems desirable to examine some of the compound forms in the other tenses for completed action, the pluperfect and future-perfect indicative and the perfect and pluperfect subjunctive. In these forms the auxiliary does not necessarily supply the place of the reduplication. In analyzing *cecineram*, as an illustration, we at once recognize the modified root *cin* with its reduplication *ce*, and the auxiliary verb *eram*. The root *cin* gives the general meaning of the verb, the reduplication *ce* denotes completed action, while *eram* adds the idea of past time. Hence we have an

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expression for completed action in past time. The Greek ἐπεποίθειν is a precisely analogous formation, consisting of the modified root ποιθ with its reduplication πε and the auxiliary ἦα, ἦαμ = ἦσαμ = *eram*. If now we compare the other tenses for completed action — *cecinero*, *cecinerim*, *cecinissem* — with *cecineram*, which we have just analyzed, we shall find that they differ from it only in the form of the auxiliary. Let us notice this point of difference more carefully.

*Eram* and *ero* in *cecineram* and *cecinero* appear to be the unchanged forms of the imperfect and future of the auxiliary *sum*, and indeed, if our analysis of *cecineram* is correct, they are such. It then seems to follow as a matter of course that in *cecinerim*, *erim* is the present subjunctive of the auxiliary *sum*, and that it is for *esim*, the full form for *sim*, which drops the initial vowel *e*, as it is also dropped in *sum* for *esum*, though retained in the Greek forms ἐστί, ἐσμέν, ἐστέ. The change of *s* in *esim* to *r* in *erim* is in accordance with a well-known law of the language which usually changes *s* to *r* between two vowels.

We have thus explained all the forms of the auxiliary in these compounds, except *issem* in *cecinissem*. That alone presents some difficulty. Whence comes the vowel *i?* What is the full form of each of the elements in *cecinissem* and how do they unite to form the compound? Are the elements *cecini-essem*, *cecini-ssem*, or *cecin-issem* with *issem* for *essem?* The second combination is the one generally adopted. Curtius says of the first (*cecini-essem*) that it would give *cecinessem* rather than *cecinissem*. This we freely admit. But what is the objection to the third (*cecin-issem*)? This alone would be in harmony with our explanation of the other compound tenses. The *i* in *issem* is undoubtedly of the same origin as the *e* in *eram*, *ero*, *erim*. May it not then come from *e* in *essem?* According to Corssen and others of undoubted authority, *essem* was originally *esem*, which would be the regular subjunctive — originally optative — formation from the indicative *eram* = *esam*. But *cecin-ĕsem* would become *cecin-ĭsem*, according to a well recognized principle that in reduplicated and compound verbal forms *a* and *e* are weakened:

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as *cado*, *cecidi; cano*, *cecini*, where *a* is weakened to *e* in the reduplication and to *i* in the stem-syllable; also *emo*, *adĭmo; lego*, *collĭgo*, where *e* is weakened to *i* in the compounds. Thus *esem* became *isem* in *cecin-ĭsem;* but when it became a recognized rule that *s* between two vowels should be changed to *r*, the alternative was presented by which *isem* would become *irem*, *erem*, as in the indicative *esam* became *eram*, or *s* would be doubled and thus protected against change. In the same manner the imperfect *esem* would become either *erem* or *essem*. The latter seems to have been the course actually pursued both in the simple *esem* and in its compounds; and hence we have *essem* and *cecinissem*. This is moreover confirmed by the fact that compound forms have been found with these different endings—*eset*, *esset*, and *isset;* as, *adi-ĕset*, *adi-esset*, and *adi-isset*.

But the explanation just given for the *i* in the ending *issem* is at variance with the generally received opinion upon the subject. Corssen, seeing that the *i* in *issem* is undoubtedly of the same origin as the *e* in *eram*, *ero*, *erim*, and the final *i* in *cecĭni*, identifies all these vowels with the long *i* in the perfect. In his view *cecin-eram* is a compound of *cecini* and *eram*. But, if that is the correct analysis of the form, how does the long *i* in *cecinī-ram* become short *e* in *cecin-ĕram?* Do we find any general analogy for this change? Indeed, would not such an analogy change the imperfect subjunctive, *audī-rem*, to *audĕrem?* The cases are entirely parallel.

But how shall we explain the endings of the perfect—*i*, *isti*, *it*, *imus*, *istis*, *erunt* or *ere?* This inquiry brings us to the vital point in our whole investigation—the formation of the Latin perfect.

The Latin perfects naturally divide themselves, in respect to formation, into three classes:

I. Perfects in *ui* and *vi: alo*, *alui; colo*, *colui; amo*, *amavi; audio*, *audivi*.

II. Perfects in *si: carpo*, *carpsi; dico*, *dixi; rego*, *rexi*.

III. Perfects in *i: cado*, *cecidi; tango*, *tetigi; capio*, *cepi; ico*, *ici*.

If now we inquire what is the tense-sign in each of these

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three classes, what characterizes these forms as perfect tenses, we shall find that perfects in *ui*, *vi*, and *si* are compounded with perfect tenses of auxiliary verbs. The tense-sign of the compound must therefore be sought in the auxiliary. All other perfects, i. e. simple perfects in *i*, according to Bopp, Curtius, Schleicher, Westphal, and others, were originally reduplicated and the reduplication constituted the tense-sign. As this conclusion, whose correctness I think there can be little reason to doubt, has been accepted with so little dissent, it will be sufficient simply to indicate in brief the general character of the argument by which it has been reached.

1. The simple perfects in the Greek and in the Sanskrit are reduplicated.

2. In Latin some of the perfects in *i* retain the reduplication in full, while most of the others lengthen the stem-vowel, thereby showing traces of a lost reduplication.

3. With four exceptions, the few perfects which retain the unchanged stem have the stem-vowel already long either by nature or by position. Moreover, of these four exceptions—*scidi*, *tuli*, *bibi*, and *fidi*—the first and second have archaic forms with reduplication, the third is in fact a reduplicated perfect, while all analogy shows that the fourth (*fidi*) must have been originally *fifidi*.

4. In some verbs there are found side by side reduplicated simple perfects and compound perfects without reduplication, showing that the tense-sign in the simple form resides in the reduplication and in the compound in the auxiliary, as *teneo*, *tetĭni*, *tenui*; *pango*, *pepĭgi*, *panxi*.

But we must hasten to consider the compound perfects. These end in *ui*, *vi*, and *si*. Those in *ui* and *vi* were explained by Bopp as compounds of *fui*, those in *si* as compounds of *esi*, a perfect formed from *es*, the root of *sum*, and corresponding to the Sanskrit *āsa* = *fui*. This explanation has been generally accepted by philologists, but has of late been called in question by Westphal and Merguet, the latter of whom denies its application even to *potui* for *pot-fui*, as the perfect of *possum*. He explains *potui* as formed from *potīvi* from *potio*, and thus, as he conceives, deprives Bopp's theory of its very strongest support.

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At first Westphal and Merguet both took the position that compound tenses may be produced by the union of inflected forms with each other, but not by the union of such forms with naked stems. They recognized such compounds as appear in the Sanskrit periphrastic perfect, *ćorayám-ása*, and in the French future, *j'aimer-ai*, but not such as Bopp found in the Sanskrit *á-dik-sham*, in the Greek ἔδειξα, and in the Latin *alui*. Such was Westphal's position when his Greek Grammar appeared; but in a special work on the Latin verbal inflections, published last year, he recedes from this position and expresses, in a most unqualified manner, the conviction that the Latin perfects in *ui* and *vi* are compounds of *fui*, and those in *si* compounds of *esi*.

But Merguet in his work entitled *Die Entwickelung der lateinischen Formenbildung*, published in 1870, claims that the union of inflected forms with naked stems is in itself a contradiction, inasmuch as, in his judgment, the two elements of the compound belong, as independent words, to different ages in the development of language.

To this sweeping criticism, Curtius in his recent work on the Greek verb replies:

1. That the assumption, that there could have been no transition period in which naked stems and inflected forms may have existed side by side as independent words, is utterly without foundation.

2. That compounds of inflected forms with naked stems do undoubtedly exist; that indeed no other reasonable explanation can be given of such forms as λογο-ποιός, πυρ-φόρος, and the like.

Thus the objection to Bopp's theory that the Latin perfects in *ui*, *vi*, and *si* are compound forms has, in my judgment, been fairly met. We proceed to examine the compounds themselves.

*Alo*, *al-fui*, *alui* (*f* dropped); *amo*, *ama-fui*, *ama-ui*, *amavi* (*f* dropped and *u* changed to its corresponding *v* between two vowels); *carpo*, *carp-ĭsi*, *carpsi* (*ĭ*, for *ĕ*, dropped); *dico*, *dic-ĭsi*, *dixi* (*ĭ* dropped and *c-s* united in *x*). Now all these perfects are such only by virtue of the auxiliary *fui* and *esi*

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contained in them. But what imparts to *fui* and *esi* their character as perfect tenses? That they are such, there can be no doubt, but what makes them so is not equally clear. Let us, however, compare these forms with the Sanskrit and Greek perfects from the same roots. The Latin *fui* and the corresponding Sanskrit *ba-bhúv-a* are inflected as follows:

| | |
|---|---|
| fu-i, | ba-bhúv-a, |
| fu-isti, | ba-bhúv-itha, |
| fu-it, | ba-bhúv-a, |
| fu-imus, | ba-bhúv-ima, |
| fu-istis, | ba-bhúv-a, |
| fu-erunt *or* -ere. | ba-bhúv-us. |

The contrast is scarcely less remarkable than the resemblance. They are undoubtedly corresponding forms, but they seem to have received very different treatment. The Sanskrit retains the reduplication; the Latin, apparently, no trace of it. The endings of *ba-bhúv-a* are not peculiar, those of *fui* are without a parallel, or even an analogy, in any other tense in the Latin verb. The Greek πέφυκα throws no light upon *fui;* we compare *esi* with the Sanskrit *āsa*, and we encounter the same contrast as before; *āsa* is inflected precisely like *ba-bhúv-a; esi*, precisely like *fui*.

Let us now note the points of difference and set distinctly before us the peculiarities of the Latin perfect, as seen in *fui*, *esi*, and their compounds.

1. The reduplication appears in full in the Sanskrit and in the Greek, but not in the Latin; though we should indeed have its equivalent in *esi*, if we could prove that the initial *e* is long, as is generally assumed without proof from its connection with the Sanskrit *āsa;* but as this vowel uniformly disappears from the Latin paradigm without leaving any trace behind, there is, I think, good reason to question the assumption that it is long.

2. The *i* in *fui* is peculiar and requires explanation.

3. So also are the endings *isti*, *istis*, *erunt*, and *ere*.

In this list there are at least three or four points, which by general consent have never been satisfactorily explained. In regard to Bopp's labored effort to bring the Latin perfect into some sort of harmony with Sanskrit aorist forms, Corssen

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remarks that in the midst of all these varying and at times contradictory statements, he has endeavored in vain to find a consistent explanation of the Latin perfect in harmony with the facts in the case. His own words are: "Ich bin vergebens bemüht gewesen, in diesen und anderen schwankenden und sich zum Theil widersprechenden Angaben, eine feste und consequente mit den Thatsachen der lateinischen Sprache in Einklang stehende Erklärung des lateinischen Perfectum zu finden." With the same emphasis he also rejects the suggestion of Curtius, that the long *i* of the Latin perfect may be identical with the short *a* in the Sanskrit perfect. Schleicher recognizes in a verb like *facio* three distinct stems for the perfect: *fac* in *fac-sim*, *feci* in *fecit*, and *fecis* in *fecis-tis*. The first and third of these Corssen discards utterly; in regard to the second (*feci*), Schleicher himself admits that the *i* is added to the perfect stem, and is moreover of uncertain origin. His words are: "Ausserdem tritt ein in seinem Ursprunge dunkeles *i* an den Ausslaut des Perfect-stammes." No explanation is attempted of this troublesome *i*. Corssen calls it a vowel of formation (*Bildungsvocal*), and with Aufrecht identifies it with the *i* in the Sanskrit aorist in *isham*, as *ávēdīsham*, but attempts no explanation. He derives *s* in the first syllable of *isti* and *istis* and *r* in *erunt* from the stem *es*, and in this view is supported by Curtius; though, so far as I see, neither of these eminent linguists makes any use of the fact.

Such, if I understand it aright, is the present state of the question involved in the formation of the Latin perfect. In view therefore of the great uncertainty which still hangs over several important points connected with it, I venture, with unfeigned diffidence, to submit to the thoughtful consideration of my fellow-laborers in this field a few suggestions, in the hope that they may at least aid us in our subsequent investigations.

A word upon the manner in which the Latin auxiliaries *fui* and *esi* are used in forming compound tenses, in distinction from the corresponding use of auxiliaries in the Greek and Sanskrit, may not be entirely useless at this point. We



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notice first that when the Sanskrit *āsa* and *ba-bhúv-a* are used in the formation of the periphrastic perfect, the auxiliary is retained in full with reduplication — *ćorayāmāsa;* and secondly, that in such compounds as the Sanskrit *á-dik-sham* (= *dik* and *āsam*) or the Greek ἔδειξα (= δεικ and ἔσα or ἧσα), the augment is uniformly retained. In Latin, on the contrary, though all the compounds of *fui* and *esi* uniformly retain *ui*, *vi*, and *si*, the *e* in *esi* entirely disappears in every instance, and with it all trace of that which makes it a perfect tense, if it is formed like the Sanskrit *āsa*. These facts suggest the inquiry whether *esi* may not be a slightly different formation from *āsa*, though an entirely analogous one; whether indeed we may not find here in the treatment of the auxiliary itself, the key to the explanation of some peculiarities of the Latin perfect.

The Sanskrit *āsa* is, I think, admitted to represent an earlier form *asasa* or *asasma*, with the root repeated in accordance with the original idea of the reduplication. Moreover, it will be observed that we have here only the repetition of a single syllable *as*, like that of οπ in ὄπωπα, οδ in ὄδωδα, ακ in ἀκήκοα, and like the corresponding reduplication in the Zend. After the analogy of *asasma*, the Latin *es* would give *esismi* inflected thus:

| | | |
|---|---|---|
| esismi | = esīmi = | esī, |
| esisti | | esisti, |
| esisti | = esist = | esīt, |
| esismus | | esimus, |
| esistis | | esistis, |
| esīsunt | | esīsunt. |

That *es* reduplicated produces *esis*, instead of *eses*, is in accordance with the well-known principle, already mentioned, by which *a* and *e* are often weakened in the reduplicated and compound forms: *cado*, *cecĭdi; emo*, *adĭmo; dedi*, *condidi*. Moreover, that the *i* before *t* in *dixit* may be identical with *e* seems to be supported by the fact that the form in *et* actually occurs in early inscriptions. Again, *i* is the favorite vowel before *s*, as is abundantly shown by Latin forms, such as *cinis*, *cineris; pulvis*, *pulveris;* so also before *st* in the middle of a word: as in *antisto*, *antistes*, etc.

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But let us now examine the changes which take place in our inflection of *esismi*. For the dropping of *s* before *m* in *mi* and *mus* in the first person singular and plural, we may adduce not only the corresponding treatment of the Greek *c* in εἰμί for ἐσμί and ἦμεν for ἦσμεν, but also the well-known usage of the Latin which often drops *s* before *m* in similar cases, as in *rēmus* for *resmus*, *ōmen* for *osmen; Cămēna* for *Casmēna*. If now we drop the personal ending *mi*, in accordance with the general usage in the first person singular of all leading tenses of the indicative, and then lengthen the preceding vowel in compensation, we shall have *esī* and *esīmus*. In the latter the *i* in the penult may be either long or short — short, if it follows the analogy of *Cămēna* for *Casmēna*, long, if it follows the more common analogy of *ōmen* and *rēmus*. Upon the latter supposition, it must have been subsequently shortened — a treatment by no means uncommon in vowels which have been lengthened by the principle of compensation, as in *pedēts*, *pedēs*, *pedĕs*.

The dropping of *i* final in the personal ending of *esisti* in the third personal singular requires no explanation, as it is in accordance with the general usage. In the same form the significance of the *s* before *t*, as a part of the stem, was in process of time practically lost, and finally the letter itself disappeared under the influence of the endings *at*, *et*, *it*, which regularly represent the third person singular in the Latin indicative and subjunctive. Thus *esist* became *esit* by a process which finds its complete analogy in the Greek ἦτον for ἦστον, and in the Sanskrit imperfect *a-śat* for *a-śast*, and in the aorist *á-bod-it* for *a-bod-ist*.

*Ti* in *esisti* of the second person singular is a recognized ending for that person and corresponds to the Sanskrit *tha*, the Greek θι in ἴσθι. It forms also the first element in *tis* of the second person plural. The quantity of the final *ī*, which Westphal pronounces a still greater problem than the preceding *st*, illustrates a treatment of this vowel by no means uncommon in the Latin. Short final *i*, it is well known, is generally lengthened, or changed to *e*. The Latin *istī*, as compared with the Greek ἴσθι, shows precisely the

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same change in quantity, as actually appears in the Latin *sināpī* as compared with the Greek σίναπι.

The *i* in the penult of *isti*, which is long by position, seems to have been at times treated as long by nature, perhaps after the analogy of long *i* in *esī* and *esīt*. The subsequent shortening of ī in this last form *esīt* before final *t*, requires no explanation, as it follows the general usage.

In the third person plural, *ĭsunt* became first *ĭrunt*, a form which actually occurs in inscriptions, and then *ĕrunt*. But in the classical period the penult of *erunt* was generally long, a fact which may be best explained in connection with the shorter ending *ēre*, as seen in *dixerunt* or *dixere*. This ending is generally explained as formed from *erunt* by dropping *nt* and weakening *u* to *e*. Westphal objects to this view, on the ground that the Latin nowhere else drops the plural ending *nt*, and that, if it did so here, we should probably have *ēro* and not *ēre*. He does not recognize the auxiliary *sunt* either in *erunt* or *ere*. I do not regard these objections as at all decisive against the common explanation, but I venture to suggest another, that in making our selection we may at least have a little wider choice. The Latin treatment of the root *es*, as seen in the verb itself, gives, in the third plural, *esunt*, which becomes *sunt* or *erunt;* but as *erunt* points back to *esunt*, so *ēre* seems to suggest an earlier form *ēsĕ* or *ēsĭ*, the latter of which finds an exact parallel in the Greek εἰσί from ἐσντί, and is formed in strict accordance with principles of general application, alike in Latin, Greek, and Sanskrit. In this form, *si* represents the personal ending *ti;* but *t* before *i* is often thus changed to *s*, as in *consensio*, *dissensio*, precisely as τ before ι is changed to σ in εἰσί. But in Latin *ēsĭ* becomes first *ērĭ* — a form actually found in inscriptions — and then *ērĕ* as in *carpsēre*, *dixēre*. Thus there may have existed, in the infancy of the language, two distinct forms side by side — a shorter form in *ēre*, with the penult uniformly and necessarily long, and a fuller form in *erunt*, with a short penult which may have become gradually lengthened by the analogy of its associate *ēre*. As a matter of fact, this penult was generally long, though by

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no means uniformly so; and if the more common derivation of *ēre* from *erunt* be preferred to the one here proposed, we may, I think, without impropriety assume that *e* in *erunt* was lengthened to bring it into harmony with the other long vowels in the endings of this tense.

If now we form perfects by appending the auxiliary *esi* to the roots *carp* and *dic*, we shall have *carp-ĭsi* and *dic-ĭsi* and, dropping *ĭ* (*ĕ*), *carpsi* and *dixi*, inflected thus:

| | |
|---|---|
| carpsi, | dixi, |
| carpsisti, | dixisti, |
| carpsit, | dixit, |
| carpsimus, | diximus, |
| carpsistis, | dixistis, |
| carpsērunt (*or* -ēre). | dixērunt (*or* -ēre). |

From *esi*, or its stem *esis*, may now be formed the other tenses for completed action, *esisam*, *esiso*, *esisim*, *esissem*, precisely as *esam* (= *eram*), *eso* (= *ero*), *esim* (= *sim*), and *essem* are formed from the root *es*. If now we append these tenses of the auxiliary to *carp* and *dic*, dropping the initial *e* and observing the ordinary euphonic changes, we obtain the regular classical forms.

| | |
|---|---|
| carp-sisam = carpseram, | dic-sisam = dixeram, |
| carp-siso = carpsero, | dic-siso = dixero, |
| carp-sisim = carpserim, | dic-sisim = dixerim, |
| carp-sissem = carpsissem. | dic-sissem = dixissem. |

Our discussion seems to warrant the conclusion that in the class of verbs which we have been examining, the peculiarities of the Latin perfect — the final *ī*, *s* in the first syllable of *isti* and *istis*, and the peculiar endings *erunt* and *ere* — may all be the direct result of the reduplication of the root *es* in the auxiliary. They are all readily explained in this manner without doing violence to any known law of the language, and without requiring the insertion of a single letter, even of a connecting vowel.

The examination of *fui* and of perfects in *ui*, *vi*, and *i* is reserved for a future paper.

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## III. — *On an English Vowel-Mutation, present in CAG, KEG.*

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THE somewhat rare Celtic vowel of 'fat' occurs in Irish and Welsh,* and when present in European dialects, it is probably due to Celtic influence, as in Suabian, and in its French nasal form in 'vin.' It does not necessarily occur in the same word in languages where it is present, for the Irish word 'cat' (a *cat*) and 'capull' (Lat. 'caballus,' a *horse*) have the vowel of English *cot*. It is well established in English, where, from its affinity with *ĕ* of 'ebb,' the two present many interchanges, as in 'cag' and 'keg,' 'mash' and 'mesh' (of a net) where 'mash' is the vulgar and etymologic form, and Dr. Johnson says it is "better written, as it is commonly pronounced, *mash*." In certain localities we find forms like 'merry,' 'scelp,' and 'hev,' for 'marry,' 'scalp,' and 'have,' forms which are apt to disgust people who are not accustomed to hear them.

This mutation appears in English 'fat' as compared with German 'fett;' 'have,' old Saxon 'hebbjan,' low Saxon (or Plattdeutsch) 'hebben;' and as both vowels are present in low Saxon, this English interchange may be heard, as in the word for *six*, which, according to the locality, is 'sas'ə' (sʌs'ə, strictly with the vowel of 'fat') and 'sɛs'ə,' with the vowel of 'met.'

Mutations with the Latin or true *a* of 'arm' (as in 'barberry' and 'berberry,' Anglosaxon 'arc' and 'erc') and that of 'wash' and '+wesch,' are excluded from the following list, but as the vowel of 'fat' is often used in words with the allied â of 'âfter,' these pairs have been retained. Proper names, archaisms (marked with +), Scotch, and local English forms are included. The abbreviations

* As in Irish 'că'hăr,' *four;* Welsh 'băch,' *a hook*, the same vowel being lengthened in Welsh 'bäch,' *little*. Hald., Analytic Orthography, p. 85.

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used are: *Ch.*, Chaucer; *Hlw.*, Halliwell; *Str.*, Stratmann; *Sc.*, Scotch. Scotch examples must be cited with caution except by natives, as the spelling is deceptive. According to Mr. James A. H. Murray (Philol. Soc. London, 1873, pp. 109, 145), this dialect gives 'man' with the German and French *a*, and 'men,' 'pet,' 'led,' with the vowel of *add*.

The following lists contain about three hundred and twenty pairs of examples:

abbas *Hlw.*, abbess
abele, ebble *Hlw.*
ac *Sc.* (but, and), ec
ac-ute, edge
addabarân, aldebaran
adder, edder *Hlw.*
admiral, amrell *Hlw.*
after, efter *Hlw.*
agg* *Hlw.*, to egg *v.*
ak-yn *Sc.* (oaken), yek (oak) *Hlw.*
al-chemy, el-ixer
al-ligator, El-dorâdo
alf *Hlw.*, elf
+alfisch, elvish
ăl-ibi, ăl-ias, el-se
Alic, Elick
Alice, Elsie
alder, eller *Hlw.*
alum †, *for* elm
ambassador ‡, embassy
amîr §, emîr
among, +emong
Amphipolis, Emboli
amty *Hlw.*, empty
+angel ‖, +engel
annual, perennial
+anoug, enough
ant, emmet
antique, ancient(= en-)
+any, eny *Ch.*
appear-ance, appar-ent
apt, adapt, inept, adept
+arran (spider), nerane *Hlw.*
arrand, errand
arrant, errant
+asaumple *Str.*, example
+ascape, escape
ascry *Hlw.*, eskrie *Hlw.*
ash (ask) *Hlw.*, esh *Hlw.*
ash (stubble) *Hlw.*, esh *Hlw.*
ash (tree), esh *Hlw.*
ashes, ess *Hlw.*
ascend-ant *a.*, descend-ent *a.*
ascend-ancy, ascend-ency
ask, esse *Hlw.*
aspen, espin *Hlw.*
assay, essay
astonish, +estonen *Str.*
at, +et
attend-ance, tend-ency
attend-ant, intend-ent
+atter (poison), etter *Hlw.*
+attercop (spider), eddercop *Hlw.*
atwiten *Str.*, edwyte *Hlw.*

* Agg (to incite) — the popular and the preferable form. Pennsylvania.

† It is uncertain whether a western stream called Alum Creek took its name from the mineral or from the tree.

‡ The erroneous em- for am- in 'embassy' is due to Spanish, and is retained by the accent. See Hald., Eng. Affixes, p. 263, and compare 'empire' and 'imperial.'

§ This is the better form.

‖ Although Anglish 'eng-el' (not eng-gel) corresponds with German 'eng-el,' the dialectic form 'enc-gel' seems to indicate the *gay* in 'anger.' Similarly, Anglish 'angel' or 'angl' (a *hook*) is probably like German 'ang-el' rather than the English 'angle' = ang-gl.

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avery *Hlw.*, every
avoid, evitable
axe, exe *Hlw.*
axle, eẋle *Hlw.*
back, beck
baff *Sc.*, beff *Sc.*
bag, begg-ar
ballys *Hlw.*, bellows
band, bendy
bank, bench
Bâyou Tash, *for* — Tèche
+birafte, bereft
blacken, bleck *Sc.*
blanch, blench
bland *Sc.*, blend
bran, bren *Hlw.*
brand, brenne *v. Hlw.*
brant (goose), brent
+brant (steep), brent
cadlock, kedlock
cąg, keg*
calash, caleche
calf, kelf (?) *Hlw.*
can *v.*, ken *n.*
canal, kennel
cand-ent, *etc.*, ac-cend, *etc.*
canine, kennel
cannel-coal, kennel-coal
canto, accent
canvas, hemp
capital, cephalic
capt-ure, inter-cept
carn-al, charn-el
cast, +cest†
castanet, chestnut
castrel, kestrel
catch, kecche‡
catsup, ketchup
chack *Sc.*, check *v.*
chack *Sc.* (a bird), check *Sc.*
chack *Sc.* (a meal), check
champion, kemp *Sc.*
channel, kennel
charity, cherish
chăstity, incest
claft *Sc.*, cleft
command, commend
cour-ant, curr-ent
crang, kreng
c-rank, wrench
daddy, deddy *Sc.*
Dak'han, Deccan
damn, condemn
dĭabolism §, devil
donat, donet
+drad, dread
drag, dredge
e-jac-ulate, e-jec-t
example, exemplary
+facche *Hlw.*, fetch
faction, defection
faloun *Hlw.*, felon
fan-cricket, fen-cricket
farrier, ferrier
farrow *a.*, ferow *Sc.*
fash *Sc.*, fesh *Sc.*
fasten, +festen
fatch *Hlw.*, vetches
fealty, fidelity
foc-al, fu-el
Frank, French
frantic, frenetic‖
Gallic, C^k^eltic
gang *n.*, genge *Hlw.*

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* Compare the same sounds in Anglosaxon 'cæg,' 'ceg' (a *key*), etc.

† Pronounced *kest*, and used by Wyatt (1503–1542) as a rhyme to *best*.

‡ About 1275, according to Stratmann.

"To ketch him at a vauntage in his snares." *Spenser.*

"That, as pursued appearing at full stretch,
This, barking after, and at point to catch." *Tate.*

§ The ĭ is marked as short, because English accent generally obscures the adjoining syllables.

‖ "An irous man is lik a frentik best."

*Chaucer*, l. 7631, where 'best' = 'baist,' a *beast*.

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+gather*, to-gether
ginsang †, ginseng
glabbet *Sc.*, glebber *Sc.*
glance, glent *Sc.*
granadier, grenadier
granite, grenade
Guiana, Cayenne ‡
hack *Hlw.*, hedge
hackle, heckle
hadder *Hlw.*, hĕather
hag, hegge *Hlw.*
han *Hlw.*, hence
hand, hend *Hlw.*
Handel, Hendel
hang, +heng *Ch.*
harry *v.*, herry *Hlw.*
Harry, Henry
has, hes *Hlw.*
hasp, hesp *Hlw.*
have, hebben *Hlw.*
hospit-al, hot-el
intend-ant, *etc.*, -ent, *etc.*
jasmin, jessamin
kavel *Sc.*, kevel *Sc.*
lactic, lettuce
+lad *Ch.* l. 7260, led.
+lasse *Ch.*, less
lat *Sc.* (to reckon), let *Sc.*
lather, lether *v.*
Maggy, Meg, Peg
malte *Hlw.*, melted
mameluke, memlook
man, men
+manace, menace
manage, menagerie
manifold (?), many §
maret *Hlw.*, merit
Mariatta, *for* Marietta
mash, mesh
maslin, meslin
mass, +messe
Massurâda, Mesurâdo
Matamoras, Metamoras
obeis-ance, obedi-ence
pall-mall, pell-mell
panel, penelle *Hlw.*
pansy, pensive, pensy *Hlw.*
pantile, pentile
pantograph, pentagraph
pector-al, poitr-el
pend-ant, pend-ent
penit-ence, pen-ance
pinchback, pinchbeck
placid, pleasant
radish, *as if* reddish
radly *Hlw.*, readily
rakene *Hlw*, reckon
ransom, redemption

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* Wiclif uses 'gethere' (Daniel iii. 2) as a transitive verb; 'geder-en' occurs about the year 1200 (Stratmann); 'gĕdher' is much used in speech, and Wordsworth rhymes it with 'hĕather':

"The wild-woods fruits to gather, . . .
A crest of blooming heather."

† Thus pronounced in the United States by those who know and collect the plant. When the root is forked it is likened to a man, and is probably named in Chinese from 'dzhin,' *man*, and 'seáng,' *form*, but in the literary language the second part is 'sēng,' and seems to be without a special meaning. Span. 'jinsing,' Port. 'ginsáō.' Webster (1828), quoting Grosier's China, gives the word as meaning "the resemblance of a man, or man's thigh." Worcester quotes Palmer for "*gen-seng*, first of plants," which is added as a second etymology in Mahn's Webster, and is an error due to a mixing of authorities. The Rev. Dr. S. F. Jarvis (Trans. Nat. Hist. Soc. of Hartford, 1836) quotes Jartoux for the proper meaning, "the representation of a man," for what reason he cannot tell. The Tartars call it *Orhota*, which means "the first of plants."

‡ Both of these appear in the name of the condiment 'ky-an'.' The 'cay' of 'Cayenne' and 'cayman' should be read *ki* (in *kind*) as intended by the writers.

§ 'Many,' a town in Texas, is called 'mann-y.'



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ranch, *for* wrench
rax, ratch *v. Hlw.*, stretch
redan, indent
raplock *Sc.*, reploch *Sc.*
lang-saddle *Sc.*, -settle *Sc.*
sack, seck *Hlw.*
sag *v.*, seg *Sc.*
salify, (salt)cell-ar
sally, salmon (summer)set
sampler, exemplar
satis-fy, as-sets
sattle *Hlw.*, settle *v.*
+sax *Str.*, +sex (an ax)
+Saxlond, +Scxlond
scad *Hlw.*, shed *v.*
scal-p, shell
scand-ent, ascend-ant
scarlat-inous, scarlet
serv-ant, subservi-ent
shadow, shed *n.*
slack, sleck *
slack, sleck †
slant, slent *Hlw.*
smack, smecen *Hlw.*
sparage, sperage
spat-ula, pĕt-al
stag *Hlw.*, steg (a gander)
strand, trend
+tache (a spot), tetch *Hlw.*
tamper, temper
Tamsford, Thames
tanrec, tenrec
tendrac, tenrec
tarantula, tarentula
tarrapin ‡, terrapin
tarras §, terrace
+tarre, +terry
tarrier, terrier
tassel (hawk), tercel
ten-ant, *for* ten-ent
+than ‖, then
thatch, deck
+thrad, thread
thrash ¶, thresh
track, treche *Hlw.*
track-pot *Sc.*, treck-pot *Sc.*
tractable, tretable *Hlw.*
trans(fer), tres(pass)
travesse *Sc.*, treviss *Sc.*
vacche *Hlw.*, fetch
Vandal, Wendish
vandue, *for* vendue
vanquished, venqueste *Hlw.*
vascul-ar, vessel
vi-and, viv-ency
wăme (venter), wem *Hlw.*
wax *v.*, wexe *Hlw.*
whammel, whemmel
+whan, when
wrack, wreck
wrastle **, wrestle
wratch *Sc.*, wretch *n.*
wrath *n.*, wreth *Sc.*
wrath, breth *Hlw.*
yalloch *Sc.*, yell *n.*
yaldran *Sc.*, yeldrin *Sc.*
yalowe *Hlw.*, yellow

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Alsace, Elsess
affi-ance, diffid-ence
af-, de-fi-ant, diffid-ent
ambush, +embush
amend, emendation
+asoine, +essoin *Str.*
astray, estray
assist-ance, consist-ence

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* Small pit-coal. *Ray.* † To quench, as a fire; to thirst. *Ray.*

‡ This is the popular speech-form in the United States. Browne (Hist. of Jamaica, 1756, p. 465) and Schöpf (Hist. Testud., 1792, p. 64) call it 'terrapin,' and Lacepède (1788) calls it 'terrapène.' Compare Lenâpe 'tul-pe,' a *tortoise.*

§ "As in the Tarras heere this other day." *Drayton*, ed. 1613.

‖ "I heard a man, | That now and than." *Wyatt.*

¶ "Corn must be *thrash'd*, and ground for *food:*" *Pettus*, 1683.

** "VVhich wrastleth with the water," *Drayton*, ed. 1613.

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assist-ant, consist-ent
Aurangabad, Aurengabad
Aurangzib, Aurengzib
Balize, Belize
blab *Sc.*, bleb
bladoch *Sc.*, bledoch *Sc.*
blanch *Sc.* (a ray), blenk *Sc.*
blancher, blencher
blather, blether
Brackenridge, Breckenridge
Bustamante, -mente
chance, cadence
chavender, cheven
confid-ant, -ent, -ence
conniv-ance, -ent
counten-ance, contin-ence
crann-y, cren-ulate
cross-jack, crojeck
Damiata, Damietta
dan-delion, den-tal
dual, duel
eleg-ance, intellig-ence
eleg-ant, neglig-ent
en-amor, en-emy
fatten, fetten *Str.*
fasten, fest *Hlw.*
f-lag(stone), (crom)lech
flat *Sc.* (floor), flet *Sc.*
gag *Sc.*, geg
glanders, glen
gradual, ingress
Granada, Grenada
+jalous, jealous
labber *Sc.*, lebber *Sc.*
Lan-caster, Chester
+lassen, lessen
laverok *Sc.*, lerrik
manhaden, menhaden
miscre-ant, cred-ent, -ce
Navesink (N. Jersey), Nev-
nuis-ance, noc-ent
parrakeet, perroquet
persist-ance, -ent
+provand, provender
puiss-ant, pot-ent
rabbet, reb'ate
rab-id, rev-ery
remn-ant, reman-ent
resist-ance, exist-ence
resist-ant, exist-ent
snag (a cut), sneg *Sc.*
staddle, steddle
+stam, stem
that, +thet
thous-and, +thus-end
+thratte *Hlw.*, threaten
wax *n.*, +wex
Yeman, Yemen

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## IV. — *On a Passage in Homer's Odyssey* (x. 81–86).

By LEWIS R. PACKARD,

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*ἑβδομάτῃ δ' ἱκόμεσθα Λάμου αἰπὺ πτολίεθρον,*
*τηλέπυλον Λαιστρυγονίην, ὅθι ποιμένα ποιμὴν*
*ἠπύει εἰσελάων, ὁ δέ τ' ἐξελάων ὑπακούει.*
*ἐνθα κ' ἄυπνος ἀνὴρ δοιοὺς ἐξήρατο μισθούς,*
*τὸν μὲν βουκολέων, τὸν δ' ἀργυφα μῆλα νομεύων·*
*ἐγγὺς γὰρ νυκτός τε καὶ ἠματός εἰσι κέλευθοι.*

Od. x. 81–86.

THAT this passage was obscure and difficult to the early students of Homer appears from the number of conflicting explanations and conjectures suggested in the Scholia; and among modern commentators there is scarcely more agreement,

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though the conjectures are less wild. I propose to state the difficulties and briefly describe the different theories of explanation, and to indicate in what direction the true explanation of part of the passage appears to me to lie.

The first question arises as to Λάμου — is it the name of a city or of a person? It may very well be the name of a city; there is in Strabo (xiv. p. 671) mention of a river and a village, both bearing the name Lamos, in Kilikia, and the construction, a genitive of designation or apposition, occurs elsewhere in Homer as well as frequently in later Greek. An example is Il. ii. 538, Δίου τ' αἰπὺ πτολίεθρον. If we take it as the name of the city, the two words in the next line, τηλέπυλον Λαιστρυγονίην, may be regarded as adjectives agreeing with πόλιν, which is perhaps implied in πτολίεθρον. Others understand Λαιστρυγονίην as a substantive in apposition to πτολίεθρον, translating "to the city Lamus, to long-streeted Laestrygonia," in which phrase two names for the city are given. These are awkward, but perhaps not impossible constructions.

On the other hand, may not Λάμου be the name of a person, a former or the then reigning king of this land? This view likewise was taken in ancient times, and the Scholia even tell us that he was a son of Poseidon. It is difficult to find the origin of that statement, as this seems to be the only passage of Greek literature before the Christian era in which the name occurs. Probably the guess was suggested by the similarity in character of these Laestrygonians to the Kyklopes, who are some of them described in Homer as descendants of Poseidon. If Λάμου is the name of a person, it would apparently be some former king or eponymous hero of this tribe, as lines 106–111 below refer to one Antiphates as king at the time of this visit.

Then, if Λάμου is the name of a person, the next point of variation in the Scholia is as to τηλέπυλον — is it an adjective, or a substantive, the name of the city, and so to be printed, as Dindorf prints it, with a capital T? They differ also as to the meaning of the word as a compound (whether substantively or adjectively used in this place); is it "having

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gates far apart, distant from one another," or is it "having large gates, wide and high"? This latter view is taken by two modern editors, Ameis and Hayman, on the ground that such gates belong to a city of giants and where two flocks at once pass through the gates — an idea which is by no means clearly expressed in the passage. But they give no example to support this meaning, and I find no other compound of τῆλε having such a sense. It always has the meaning "distant, far" and never that of "large." For "wide-gated" we have εὐρυπυλής (Od. xi. 571); for "high-gated" the Scholia on this passage use μακρόπυλος. It seems then that τηλέπυλος should mean "having gates far apart," a description of a city either, as Nitzsch understands it, "with long streets" and gates at both ends, a length measured on a diameter, or, as perhaps is more natural, measuring the length on the circumference, with a long stretch of wall between its gates and so "large in circuit." The word occurs, I believe, nowhere except here and in Od. xxiii. 318, in a reference to this same city which occurs in a summary of the wanderings of Ulysses, but that summary, though a part of the poem in the time of Aristotle, for he (Rhet. iii. 16) refers to it as an example of successful condensation, is of doubtful genuineness in the view of modern critics. At any rate, it gives no real help to the understanding of this passage. On the whole, it seems that there are no sufficient data for a positive opinion on the questions raised in regard to this first line and a half.

We come now to the rest of the passage, which is evidently all one thought. The different items are parts of one fact in regard to this city, one distinguishing peculiarity, which the poet labors to make clear to us. The translation seems easy — somewhat as follows: "(a city) where one shepherd coming in hails another, and he going forth answers; there a man who needed not sleep could have earned double wages, one by herding cattle, another by tending sheep, for near are the paths of night and day."

The ancient comments upon these lines hardly deserve mention — certainly not the labor of refutation. One refers the description to the neighborhood of Leontini in Sicily,

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where, he says, the flies were so troublesome that the cattle could not be pastured in the daytime, whereas the sheep being defended by thick fleeces could be; and so the line referring to the different flocks is explained. Another supposes that the day and night pastures were different ones, but near to each other, so explaining the last line. Another, that the suburbs of the city were uncultivated, and so used for pasture land; and thus that a herdsman, not being obliged, as in other cities, to go to distant hills, might be able to go out twice a day with different flocks. It is plain that all these are mere conjectures, and some of them very unsuitable ones. There is a nearer approach to the probable truth in a suggestion attributed to Krates, the grammarian of Pergamos in the 2d century B. C., who thought that the whole account referred to some region of short nights. He is quoted as saying that they lived "about the head of the dragon," that is, in the region apparently under the constellation so named, "of which," Krates continues, "Aratus says 'that head will move there where risings and settings are closely joined together.'" It will be observed that this statement of Aratus has no reference to the passage in Homer. It is only the authority of Krates therefore, and not that of Aratus besides, that we have for this interpretation. He understood the phrase of Aratus as applying to the Laestrygonian country. Krates goes on to explain that since the outgoings of day and night were so near each other, the night must be very short and so a man who could dispense with sleep could earn double the pay of him who must spend a part of every day (of twenty-four hours) in sleeping.

We now turn to the opinions of modern commentators, for the fullest account of which I depend upon a young German scholar, now dead, J. F. Lauer, the first volume of whose literary remains (Berlin, 1851) is occupied with Homer. It is not however worth while to enter into all the conflicting and in many cases obsolete explanations which he discusses. I refer to his essay only as containing the best *resumé* that I have found of the various opinions; but I shall confine

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myself here to the views of recent scholars, mentioning enough to show the differences among men who all have the same general principles of criticism. Völcker (in his Homeric Geography. Hannover, 1830) perhaps hardly comes under this category, but his idea may begin the list. He supposes that the Laestrygonian city lay near sunset, or the entrance to Hades, and on a high mountain (αἰπύ); that the Greeks had noticed that sunrise came earlier and sunset later upon such mountain tops, as for instance upon Athos; and so that this city had a longer day than any other place and of course a shorter night. This idea that the city was on a high mountain is plainly inconsistent with the subsequent story in the Odyssey, and as to the rest of the theory the prolongation of day on a mountain top is hardly sufficient to suggest this exaggeration of it. Another idea is that of Klausen (die Abenteuer des Odysseus aus Hesiod erklärt. Bonn, 1834), that the day and night are spoken of as beings, not periods of time, that the western home of day was close by the Laestrygonian land, and that where the day was, it must be always light. This seems to be a step in the right direction, but does not cover the whole ground.

Nitzsch (Commentary on Odyssey i.–xii. Hannover, 1826–40) seems to have been the first to discuss the meaning of the passage in a simple and thorough way, introducing hardly any conjectures and explaining the whole as a whole. He assumes simply these two points, that the herds are driven forth in the morning and home at evening, and that the cattle are driven out earliest in the morning, the sheep come home latest at night. Where he gets this last idea I do not know; it may be so in fact in Germany or elsewhere, but I do not think there is any trace of it in Homer, nor does Nitzsch himself support it by any passage or speak of it as anything but an assumption. The passage then means in his view that the interval is so short as practically to disappear; the sheep-herd coming in at the end of his day meets and hails the cow-herd going out at the beginning of his, so near to one another are the goings forth of day and night. Thus a man who could dispense with sleep might go right out

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again with the other herd and so earn double pay. In support of this view he thinks it necessary to argue at some length that the word κέλευθος means in Homer not *way*, *road*, but the *act of going*, or, as he translates it, *Lauf*, *Fahrt*, *Fortgang*. His translation of the last line he defends by the authority of Eustathius (who however is not earlier than the twelfth century of our era), quoting from him as follows: ὡς ταχὺ μετὰ νύκτα τῆς ἡμέρας διαφαινούσης — ὃ παραφράζων Ἄρατος ἔφη τό· μίσγονται δύσιες καὶ ἀνατολαί. But, as we have already said, there is no indication in the poem of Aratus that this Homeric passage was in his mind. The connection is due to Krates only. The order of the words in the fifth line, Nitzsch adds, is to be explained by the fact that he would naturally mention first the herd that went out first in a given day.

With this explanation Faesi, the most judicious recent editor, in the main agrees, adding only the unimportant and apparently groundless conjecture, that the cow-herd would go out by the eastern gate, as the sheep-herd came in by the western.

The only other view that seems to deserve mention is that of Lauer in the book already referred to. He explains the third line as meaning that the sheep-herd coming in greets the cow-herd going out, and that this act of meeting occurs at evening. The reason for these opinions he finds in ἄυπνος ἀνήρ (for the time) and (for the order of meeting) the correlation of clauses in the third and fifth lines (a sort of *chiasmus*) — which are plainly inadequate proofs. The last line he translates as others have: "near to one another are the goings forth of day and night." Now he denies that the whole passage has any reference to the short nights of high latitudes; for his whole treatment of the subject is designed to combat the idea of any knowledge in the genuine Homer of the north of Europe. He supposes the poet to imagine this people as living very far from Greece, near the place to which the sun makes his daily journeys — so near that the day lasts much longer for them than for other people — but to imagine also, half unconsciously, that the sunrise occurs to them at the same time that it does to all the rest of the

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world, so that, the day being prolonged indefinitely towards its close, but not cut off equally at its opening, sunrise follows almost immediately upon sunset. They have indeed a night (νύξ, line 86), but the word means only the interval, however short it may be, between sunset and sunrise. As to the question how the sun gets back in time to rise in the east, Lauer says, as others have often said, that in such popular fictions we ought not to demand logical consistency or the carrying out of an idea through its results (weder Consequenz noch Durchführung). The imagination does not act logically; it views one thing at a time, and catches an idea without troubling itself as to contradictions between different partial representations. There is nothing in Homer as to the journey back of the sun from west to east; the myth of the voyage on the Ocean-stream in a golden boat is of later date.

Now I wish to present a view of the passage which differs in one or two points from any of these mentioned, and which seems to me to involve less of assumption and to agree better in one respect with the use of language elsewhere than any of them. I say nothing about the first line and a half, because, as already suggested, there seem to be no sufficient data for a positive opinion as to the precise meaning.

Let us assume, as naturally taken for granted in the mind of both poet and hearers, only this one thing — that herdsmen and flocks usually spend the night, that is, generally speaking, half of each twenty-four hours, in the fold, and the day only in the pasture. This is the representation in Homer in other cases. In that of the Cyclops for instance, in the ninth book, it appears as his habit to spend the night with his flocks about him in his cave, and to drive them out to pasture every morning (Od. ix. 216 f., 233 f., 307–15, 336–9, 405, 437 ff.). So too in that of Eumaeus, the swine-herd of Ulysses (Od. xiv. 13–22, xvi. 3). An apparent exception, in the famous simile at the end of Il. viii. (555–61), where the shepherd is spoken of as rejoicing in heart at the sight of the stars, ceases to be an exception when we remember that the shepherd's hut about which the flock would be folded might often be in the open country, perhaps on a hillside, where a wide view of the stars would be had in the evening.



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Now to apply the assumption based on these passages to the case in hand. Neither man nor animal spends all the time in the field, because of the need of sleep on the part of the man, and of protection from wild beasts or from wandering astray on the part of the animal. In a country not fully cleared of wild beasts nor fenced off into pasture fields, as Greece was not in the Homeric period, such is the necessary custom. So in the mind of the poet the idea of bringing in the flocks to the hut or to the town is naturally, we may say unconsciously, applied to Laestrygonia as it would be in any other case, from the usage with which he was familiar. Still if a man could dispense with all sleep he could there be out all the time, on account of the extreme shortness of the interval between sunset and sunrise. Yet it would not necessarily follow in the poet's mind, that a flock or herd could do the same thing, and so the double pay would have to be earned by bringing in, say the cattle for their milking and indoor time, and immediately taking out a flock of sheep for the rest of the long day. This explains the mention in the fifth line of the two kinds of animals to be tended by such a sleepless man. Thus too we understand the meeting at the gates mentioned in the second and third lines, of the outgoing and incoming droves. Nothing indicates whether cattle were going out and sheep coming in or *vice versa*, because each of these suppositions would be true at different times in the day and the description here takes the most general form. Nothing is said of its occurring at morning or at evening, for it might not be exactly at either. At certain intervals in the day of nearly twenty-four hours of light, without definite fixing of the intervals, without anything more scientific or positive than a play of the imagination, such a meeting, in whatever order, would happen.

The explanation of this strange phenomenon is in the last line, and the one point in it which gives room for uncertainty seems to be the first word, ἐγγύς. This word is generally understood, as we have seen, according to the idea of Krates (2d cent. B. C.) and Eustathius (12th cent. A. C.), as meaning "near to one another." Now ἐγγύς is used some forty-five

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times in the Iliad and Odyssey, and generally with reference to a subject in the singular (e. g. Il. iv. 496), often with also a dependent genitive expressing that to which the subject is near (e. g. Il. vii. 225, *στῆ ῥα μάλ' Ἕκτορος ἐγγύς*). In eleven cases of these forty-five (not counting the one under discussion) it refers to a dual or plural subject and has no dependent genitive (Il. iii. 344, x. 113 *τῶν γὰρ νῆες ἔασιν ἑκαστάτω, οὐδὲ μάλ' ἐγγύς*, 221, xi. 340, xviii. 586, xxi. 285, xxiii. 378, xxiv. 365, Od. ix. 166, x. 30, xxiv. 494); that is, it appears in the same situation as in x. 86. These cases then are the only ones which can illustrate the use of the word there, and in all of the eleven except one (Il. iii. 344 *καί ῥ' ἐγγὺς στήτην διαμετρητῷ ἐνὶ χώρῳ*) it must mean "near to something" mentioned in the context, not "near to one another." It thus appears that the usual sense of the word *ἐγγύς*, without *ἀλλήλων*, is that of simple, not reciprocal, nearness to something expressed in an adjoining clause and so easily supplied. So the less frequent collateral form *ἐγγύθι* is never used in Homer of a plural subject and without a dependent genitive, and never in a reciprocal sense. The true word for reciprocal nearness is *πλησίος* in the dual or plural, and sometimes in the singular as an adverb, with or without *ἀλλήλων*. It occurs some twenty-seven times, of which eight are in the plural or dual with the reciprocal and two with the single sense, four in the singular with reciprocal and thirteen with single sense; but of these thirteen, nine are repetitions of the line,

*ὧδε δέ τις εἰπεσκεν ἰδὼν ἐς πλησίον ἄλλον,*

so that throwing out this line, we have twelve cases in all numbers of the reciprocal and six of the single sense. Now with this preference of *πλησίος* in the sense of reciprocal nearness and *ἐγγύς* in the sense of simple or single nearness, it seems that we ought to prefer for *ἐγγύς* in this passage the usual sense, in which some of the Scholia take it; and then what is the implied object to which the subject is near? Plainly we should translate "near to the home of the Laestrygonians are the paths of day and of night." When we follow the narrative on, we find that this idea accords entirely with the subsequent representations. The next

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place to which they come, without any mention of time taken for the passage, is the Aeaean island, home of Kirke, a daughter of Helios. It appears that this island was not more than a day's sail from Laestrygonia in the poet's imagined geography, for in line 116 there is mention of a δεῖπνον, the morning meal, their flight is immediate, and no mark of time intervenes before their arrival at the island. Now at this island, as we learn from xii. 3 f., are "the home and dance rings of Eos, the dawn, and the risings of Helios." Less than a day's sail from there but on the farther side of Oceanos is the land of the Kimmerii (xi. 11–19) where perpetual night prevails, for the daily journeys of Helios are bounded by the Ocean stream. When the wanderers after returning from there leave Kirke's island they come speedily (xii. 166) to the island of the Seirens, and on the way the sun is so hot as to melt wax (xii. 175 f.), then immediately (xii. 201) to the abode of Skylla, then again immediately (xii. 261) to the island of Thrinakie, where are kept the cattle and sheep of Helios, guarded by his two daughters, Phaëthousa and Lampetie. All these wonders come in one day's voyage (xii. 284–93) from the island of Kirke. This whole account bears upon the line we are discussing. It represents this part of the journey, separated by six days' sail on the one side from the island of Aeolus and by nine days' sail on the other from the island of Kalypso, as spent in a region of marvels which is so because of its nearness to one of the abodes of Helios, or because, in other words, it is on the confines of the known world, at one end of the day. It is impossible to make out a consistent system from the fictions of the story-teller's imagination. He seems to have a dim idea that if one should travel west far enough he would come to a world of wonders, to the place of sunset itself, and that somehow he would find there sunset and sunrise not as far apart as they are in the ordinary experience of men. Certainly it would seem natural that to one travelling so far west the day would be indefinitely lengthened at the latter end, and the logical consequence, that it would be shortened at the beginning, might easily not have been thought of.

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Yet, in spite of Lauer's arguments, we can hardly think it impossible that the idea of such short nights was suggested by the stories of wandering Phenician or Greek navigators. Some may have gone far enough north in the Euxine or outside Gibraltar to have observed the shortening of the nights, and these stories may easily have been exaggerated by the popular imagination into such a form as this—just the form into which such exaggeration would naturally fall without knowledge of the facts which we know of the polar regions. They did not think of the night or day as lasting continuously for months, but only of the indefinite extension of what they had observed, the lengthening of the day to the extreme limit of the twenty-four hours.

The points in which this explanation differs from most other recent ones are two: 1st. The accounting for the mention of both sheep and cattle in the fifth line by the general habit of having each kind of animal at home half the time; 2d. The translation of ἐγγύς in the sixth line "near (to Laestrygonia)" instead of "near to one another."

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## V.—*On Numerals in American Indian Languages, and the Indian Mode of Counting.*

By J. HAMMOND TRUMBULL,
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THAT "all numerals are derived from the fingers"[1] is as generally true for languages of the new world as for those of the old. The North American Indians have, with comparatively few exceptions, adopted *decimal* systems, reckoning the fingers of *both* hands. Some South American tribes have not advanced beyond a *quinary;* and a few are said to be poorer even than this. The Brazilian Tupis had, at one time, no names for numbers higher than 3,[2] and the

[1] "Alle Zahlwörter gehn aus von den Fingern der Hände."—Grimm's Gesch. der deutschen Sprache, i. 167.

[2] The fact that the Tupis *lost* their names for 4 and 5, after the coming of Europeans, is worth noting. J. de Lery, who was in Brazil in 1557, writes that the "Tououpenambaults . . . . . usque ad numerum *quinque* verbis notare, hoc

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Abipones of Paraguay, as Dobrizhoffer states,[3] could not count beyond 4, giving to that number the name of 'the ostrich's toes,' *geyènknute.* Some nations, particularly those of Mexico and Central America, and the Eskimos, have reckoned by twenties instead of tens or fives, counting toes with fingers for the base of their numeral system. The Tule Indians of Darien reckon in this way: 20 is 'a man,' i. e. all his fingers and toes, 100 is '5 men,' and so on.[4] Gallatin has given a good account of these vigesimal systems in his "Notes on the semi-civilized Nations of Mexico," etc.,[5] the substance of which was incorporated by Pott in his *Zählmethode* (Halle, 1847). Mr. Gallatin had previously observed, in a note to his Comparative Vocabulary of fifty-three North American nations, "that all these had resorted to a decimal numeration." More recently, Buschmann has shown[6] that the system of the Athapascan family is clearly decimal, exhibiting traces of the vigesimal in two languages only — the Umpqua of Oregon and the Kinai; while of the languages of his Sonora group (including the Comanche, Paiute, Pima, and Shoshoni), seven have the decimal and five the vigesimal system, one (the Tarahumara) possessing both.[7] In some dialects, indications of a former vigesimal system, abandoned for or in progress of change to a decimal, may be observed.

The derivation of numerals from the fingers admitted, an answer to the question, *In what order* are the fingers counted? becomes a necessary preliminary to the investigation of any table of numerals. Which finger marks 'one'? Is it the

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modo: *augepé* 1, *mocouein* 2, *mossaput* 3, *oioicoudic* 4, *ecoinbo* 5."—Hist. Navig. in Brasiliam, 1586, p. 272. (In the 5, we recognize *po* 'hand.') Jos. de Anchieta, in his Tupi Grammar, 1595, says: "Os numeraes não chegao mais que até numero de *quatro:* ut *oiepé* 1, *mocóiⁿ* 2, *moçapîr* 3, *oyoirundic* 4." Eckart, a Jesuit missionary in Brazil, 1753–57, gives the same names for 1, 2, and 3, adding: "Non plus ultra Brasili *hodie* numerant," though he had seen names for 4 and 5 *(monkerondyc, ambó)* in 'an ancient grammar by Father Anchieta'; "sed uterque hic numerus modo jam exolevit."—Specimen Ling. Brasilicæ, 1778.

[3] Dobrizhoffer's account of the Abipones, ii. 168.

[4] See Lull's Darien Vocabulary, in the Am. Philol. Association's Transactions for 1873, p. 103.

[5] Transactions of the American Ethnological Society, vol. i. (1845).

[6] Worttafel des Athapask. Sprachstamms, §§ 114, 115, 157.

[7] Grammatik der Sonorischen Sprachen, Abth. 3, p. 141.

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*little* finger — or, as in the designation of numbers by educated deaf-mutes, the *thumb?* And, in passing from 5 to 6, i. e. from one hand to the other, is the sequence from finger to finger — thumb to thumb, like the Zulu[8] — or thumb to little finger, like the Veis?

Nearly all the information given by Gallatin and Pott on these points relates to the Eskimo numerals. In the language of "the Eskimos of Hudson's Bay, the names of the numerals 8, 9, 10, mean respectively, the middle, the fourth, and the little finger."[9] Pott, transferring this from Gallatin, infers (Zählmethode, 301) that the thumb of the second hand designates 6, i. e. 1 + 5 of the first hand. The account given by Cranz,[10] of the Eskimo mode of counting, is quoted by Pott as the starting point of his work: "Their numerals fall very short. However, they can with difficulty make a shift to mount as high as 20, by counting the fingers of both hands and the toes of both feet. But their proper numeration is five: *attausek*, 1 — *arlæk*, 2 — *pingajuak*, 3 — *sissamat*, 4 — *tellimat*, 5. If they must go further, they begin with the other hand, counting upon their fingers. The sixth [i. e. the thumb] they call *arbennek*, but the rest, till 10, have no other names but, again, 'two,' 'three,' 'four,' 'five.' They call 'eleven' *arkangat*, and 'sixteen' *arbarsanget*, and these *-teens* they count upon their toes. Thus they muster up 20. Sometimes they say instead of it, 'a man,' that is, as many fingers and toes as a man has;" etc.

That the fingers of the two hands were counted by other North American nations in the same order as by the Eskimos, several writers inform us:

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[8] "The Zulu, counting on his fingers, begins in general with the little finger of his left hand. When he comes to 5, this he may call *edesanta* 'finish hand'; then he goes on to the thumb of the right hand, and so the word *tatisitupa* 'taking the thumb' becomes a numeral for 6."—Tylor's *Primitive Culture*, i. 228. "The Vei people and many other African tribes first count the fingers of their left hand, beginning, be it remembered, from the little one, then in the same manner those of the right hand."—Id. 227.

[9] Gallatin's "Notes on the Semi-Civilized Nations of Mexico," etc. (*ut supra*), p. 49.

[10] History of Greenland (English translation, i. 225). The Greenland numeral system is more clearly and accurately exhibited by O. Fabricius, Grönlandsk Grammatik, 58–63.

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"The Dakotas, in counting, use their fingers, bending them down as they pass on, until they reach ten. Then they turn down a little finger, to remind them that one ten is laid away, and commence again. When the second ten is counted, another finger goes down, and so on."[1] "The Aubsároke or Crows [who are of the Dakota stock] like all the Indians with whom I am acquainted," says Dr. F. V. Hayden, "use their fingers in counting, bending them down temporarily against the inside of the hand as they proceed," etc.[2]

Mr. Say, describing the Indian sign-language, says: "To indicate the digits, they clench the hands and extend the *little finger of the left hand* for one, the ring finger for two," and so on to "the thumb for five, . . . the thumb of the right hand for six," etc. "When enumerating a small number, where a considerable exertion of the memory is requisite, the Indians extend the left hand with the palm upward, whilst, with the index of the right, the fingers are successively bent in to the palm, beginning as before with *the little finger*, and the greater difficulty in recalling to mind the numbers or events, the more apparent resistance is offered to the inflexion of the finger."[3] Prince Maximilian von Wied[4] gives a similar description, observing that "wenn man an den Fingern abzählt, so fängt man an der *linken Hand* an." Mr. Swan, in his account of the Makahs of Cape Flattery (Straits of Fuca), says of their mode of counting: "They commence with the little finger of the left hand, closing each finger as it is counted; then pass from the left thumb, which counts five, to the right thumb, which counts six, and so on to the little finger of the right hand, which counts ten."[5]

Whether an Indian marks 'one' by a thumb or a finger does not seem at first sight a question of much interest to students of language. It is, however, one of the thousand questions which every philologist must be prepared to answer

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[1] Riggs, Dakota Grammar, p. 36.

[2] Contributions to the Ethnography and Philology of the Indian Tribes of the Missouri Valley, p. 396.

[3] Long's Expedition to the Rocky Mountains (Philadelphia, 1823), I. 388.

[4] Reise in das Innere von Nord-America, Bd. II. 650.

[5] Indians of Cape Flattery (Smithsonian Contributions, vol. xvi.), p. 100, note.

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before he is fully competent to discuss the subject of Mr. Robert Ellis's lately published volume "On Numerals as Signs of Primeval Unity among Mankind" (London, 1873). Mr. Ellis thinks that he has detected "a great number of coincidences, affecting not only numerals, but also the names of the members of the body from which those numerals are derived, in languages far removed from each other in position," and he presents these coincidences as "the result of primeval affinity — indications of unity of origin in human speech and, probably, in the human race" (p. 4). He assumes that "the names of numerals commonly carry in themselves the proofs of their own great antiquity" (p. 2). For the Indo-European and Semitic languages this assumption is perhaps well grounded; for the American, it is untrustworthy and unsustained by evidence, except — for reasons to be mentioned presently — as regards names for the first three numerals in languages of the same linguistic group. Admitting the original unity of American speech, it is yet certain that its division into widely separated families must have preceded the origin not of numerals only, but of the verbal or nominal roots from which names of numerals in the several families were derived. Even in the same linguistic group these names, as compared with other portions of the vocabulary, carry no indications of high antiquity, but rather the contrary; and in dialects of the same language names for the same number are often radically unlike. Compare, for example, the Algonkin 'fives': Massachusetts *napanna tahshe*, Micmac *nân*, Chippeway *nánan*, Abnaki *barenesku*, Delaware *palenach*, Illinois *miaranui*, Blackfoot *nísito*. Such dissimilarity is more apparent and more general in numerals above 'five,' which are with few exceptions composite. The Arikaras or 'Riccarees' of the upper Missouri speak nearly the same language as the Pawnees and, probably at no very remote period, belonged to the same nation. Their numerals correspond with the Pawnee numerals, to 'five,' inclusive; but here the likeness ends, not merely the *names* but the *primary conceptions* of the higher numbers differing in the two dialects. One Yuma dialect of the Colorado, the Mojave, repeats 1, 2, 3, in the



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names for 6, 7, and 8, and marks 9 as 'next to ten'; another, the Cuchan, near akin, regards 6 and 9, respectively, as a pair and a triplet of 'threes,' and 8 as a doubled 4. All these in some sense "gehn aus von den Fingern," but the same finger of the same hand or the hand itself may be — and in fact very often is — differently named, or the number it marks is differently expressed, by tribes speaking dialects of the same language; nor may we expect always to find *names* either of 'hand' or 'finger' in the numeral.

In the investigation of the origin of American numerals and in inferences as to their antiquity, two facts must be borne in mind:

1. The primitive mode of indicating numbers by the *fingers* is still in use. The *name* is not completely independent of the *sign*, and, consequently, the constancy of the name in passing from one dialect to another is less assured. When an Indian marks 'five' by showing or bending down all the fingers of his left hand, the vocal utterance — whether *nánan* or *barenesku* — is of secondary importance. In the Indo-European languages the *vocal* was long ago substituted for the *digital* expression. "It was no easy task for the linguistic faculty to arrive at a suitable sign," as the exclusive designation of a number, "and when the sign was once found, it maintained itself thenceforth in use every where, without danger of replacement by any other, of later coinage."[6] But this is necessarily true only of languages in which the earlier sign — by show of fingers — is obsolete.

2. The origin of names for 'one,' 'two,' and probably 'three,' in all languages, preceded formal numeration. Pairs, couples, doubles, were known before 'two' was counted on or marked by the fingers. The conception of *duality* dates from the first conscious separation of the 'not-I' from the 'I': and, with the first perception of differences in the 'not-I' — as 'this' and 'that,' 'here' and 'yonder,' 'thou' and 'he,' 'before' and 'after,' came the notion and name of 'three,' as something 'beyond,' 'besides,' or 'above'

[6] Whitney, Language and the Study of Language, 195.

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(tar, tri, trans, tres, très) the primary distinction; and thereupon, the exclusive and inclusive dual, 'thou-he' (and not 'I'), 'I-thou' (and not 'he'); after this, the conception of *plurality*, and *numeration*. Some nations, as we have seen, never advanced beyond the 'three.' Others (to be mentioned hereafter) only found their way to 'ten' by help of 'pairs' and 'triplets.' Hence, as Mr. Gallatin observed of American languages generally, "there is much confusion and but little regularity in the formation of the names expressing the higher numbers," even in nearly related dialects.

Mr. Ellis's first group of coincidences, and the one he regards as most important of all, includes North American words "of which different names for 'finger' supply the elements." These words, he thinks, "sufficiently illustrate the manner in which names for 'finger' and 'hand' are employed to form numerals; and by showing, moreover, that *hand* may = *fingers* = *finger-finger* (which last would be the rude plural of *finger*), they explain how 'hand' and 'two' may be the same word, as in the Omaha *nomba* which has both these meanings" (p. 6). He goes on to detect in the Basque language terms for 'finger,' 'one, i. e. finger,' and 'five = hand = fingers = finger-finger,' that correspond nearly with terms derived from North American languages, and finds coincidences with one or another of these in European and Asiatic names for 'thumb,' 'finger,' 'palm,' 'five,' 'six,' 'arm,' 'ten,' etc. (pp. 13, 14). He suggests the probability that "the Aryan languages virtually contain the forms *svas* and *saz* for 'five,' as the Basque contains *zaz* and as the North American languages contain forms like *azbaz*, such as Natchez *ispeshe* 'hand.'" And he argues (p. 18) that "if the resemblances between all these s *fives*, as they may be called by way of definition, were sufficient to imply affinity wherever they were detected, such affinity could be no other than a primeval one," — an inference the justice of which no one is likely to question. Even those much-vexed Etruscan dice of Toscanella are made to testify to primeval unity; for why may not *mach* [conjectured by Mr. Isaac Taylor to stand for] 'one,' be

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connected with "California (Sekumne) *ma* 'hand'" and "Comanche *mowa* 'hand,' 'arm,'" as well as with Siamese *mee* 'hand,' Armenian *mi* and Greek *μία* 'one,' and African (Melon) *moe* 'finger'?

Rigidly examined, these and a host of other coincidences which Mr. Ellis with much ingenuity presents, would prove to be less remarkable than they seem to him. It is not my purpose, however, to discuss them in detail, or to seek for them, collectively, any other explanation than the one which I am assured in advance "is not satisfactory" — namely, that so far as they are not imaginary, they "are merely accidental." I propose instead to make some observations on the composition and primary meaning of Indian names for numbers, and first, to point out such relation as I can find between some of these and names for the hand and the fingers. The examples will be taken chiefly — but not exclusively — from two great families of North American speech, the Algonkin and the Dakota, because, in these, published grammars and dictionaries facilitate etymological research and afford means of noting differences, phonetic and radical, between names in one and other dialects of the same stock.

I. In some languages we find only one name for 'hand' and 'fingers' collectively; and generally, for designating the fingers individually, names are formed from the word for 'hand,' with a descriptive prefix, e. g. the third finger is 'middle of the hand.'

Pott (Zählmethode, 234 ff.) has given illustrations from American languages of the recognition of a likeness between *men* and *trees*, and of figures of speech drawn from it. The arms are 'limbs' or 'branches' of the human 'trunk'; the hands and fingers are 'branches' of the arms; the fingers 'sprouts' or 'leaves'; the thumb a 'spur' or 'off-shoot.' Sometimes the fingers, collectively, are a 'row of branches,' or a 'fence.' Compare

| | |
|---|---|
| Dakota | *nape* 'hand'; *napsukdza* ('small piece of hand') 'finger.' |
| Iowa | *náwe* 'hand'; *nawépa* ('hand point') 'finger.' |
| Chippeway | *-nindj* 'hand'; *biné* 'in a row'; *-ikwan* 'branch'; *binakwanindj* 'finger,' '(one of) a row of branches of the hand.' |

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Massachusetts *-nutch* 'hand;' *pochi* 'divided'; *pochatuk* 'a branch, or division'; *pochanutch* 'a finger.'[7]

Cree (Western) *-tchitchïy* 'hand'; *yiyiki* 'forked,' 'branching'; *yiyikitchitchân* 'finger.'[8]

In some of the Algonkin languages, the name for 'hand' seems to be formed from a verbal root meaning 'to seize,' 'to lay hold of': ANŬN 'he lays hold of, catches,' *anutch* 'the layer hold of, the seizer'; *-nutch* (with pronominal prefix) 'hand.' In the western Cree, *-tchitchïy* (in composition, *otchi*) 'hand' is from the same root as the Mass. *-tchan* 'nose' (Chip. *odjanj*), which is found again in the final *tchân* of Cree 'finger,' meaning 'projecting,' 'point,' 'vertex.' The names for 'nose,' 'head,' 'fore-arm,' 'hand,' in the Dakota are apparently related one to another, their common root denoting 'pointed,' 'a projection, vertex, or extremity.' Compare with Dakota *pe* 'pointed, sharp,' *pe* 'top of the head,' *pa* 'head,' *paha* 'hill,' *pa-sú* 'beak or bill,' 'snout of an animal,' *apá* 'a part,' *apé* 'a leaf,' 'a fin,' *etápa* 'the right hand,' *ishpá* 'the fore-arm'; and Iowa *náwe* 'hand,' *náwe* 'leaf,' *nawépa* 'finger,' *pa* 'nose,' *pa-thūkh* 'beak.'

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[7] Compare Hawaiian *lima* 'arm' and 'hand'; *manamána* 'branching,' 'a branch' (redupl. of *mana* 'to be divided,' 'to branch'); *manamana lima* 'fingers.'

[8] My principal authorities for ALGONKIN languages are: *Massachusetts*, Eliot's Indian Grammar and version of the Bible; *Chippeway*, Baraga's Otchipwé Dictionary and Grammar; *Cree*, Lacombe's Grammaire et Dictionnaire de la Langue des Cris, and (Hudson's Bay dialect) Howse's Cree Grammar; *Delaware*, Zeisberger's Grammar, and Vocabulary; *Abnaki*, Rasles's Dictionary, by Pickering; *Micmac*, Maillard's Grammar; Dr. Hayden's Vocabularies of the *Blackfoot*, *Shyenne*, *Arapoho*, and *Atsina*. For the DAKOTA, my chief reliance is, necessarily, the invaluable Dictionary compiled by the Rev. S. R. Riggs and his associates in the Dakota mission of the American Board; and for other dialects, Dr. W. Matthews's *Hidatsa* (Minitari) Dictionary, Dr. Hayden's *Assiniboin*, *Aubsaroke* (Crow), *Mandan*, *Omaha*, *Iowa*, and *Winnebago* Vocabularies, the Rev. Wm. Hamilton's *Iowa* Grammar; for the *Ponka* numerals, a primer, "Ponka ABC Wa-bá-ru" (prepared by the Rev. J. Owen Dorsey, of the Episcopal mission); and for the *Osage*, Prince Maximilian von Wied-Neuwied's Vocabulary, compared with Gallatin's (in his Comparative Vocabulary).

The vowels are to be sounded as in German, except *ŭ* which is the short English *ŭ* in *but*, or the neutral vowel, variously represented in vocabularies as *ă*, *ŭ*, ʊ, and v. For the *n* which marks a nasalized vowel, I have substituted a 'superior' (ⁿ), and for the gutturals—variously represented by ch, ḣ, ḥ, χ, etc.—I have used CH or *kh*. The italic *ch* has the English sound (as in *church*), and *th*, *sh*, and *zh* (used interchangeably with *j*) are as in English.

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II. Counting the fingers from left to right, the numerals are distributed thus: Little finger, 1 and 10; Fourth finger, 2 and 9; Middle finger, 3, 8; Fore finger, 4, 7; Thumb, 5, 6.

**1. 10.** The fifth or little finger is variously designated in American languages, as 'the last of the hand,' 'the least,' 'the youngest son,' 'the little daughter of the hand,' etc. From one or another of these names, that of the numeral 'one' has, in many languages, been taken; but in others we find another expression for *unity*, 'one by itself,' which is probably of earlier origin than finger-counting. A distinction corresponding to that which is marked by the Indo-European cardinal and ordinal, between one *single* and one *coming before* others, 'fore-est,' first of a numeral series, seems to be universal in language.

In the Algonkin, these two names are represented by

| | | | |
|---|---|---|---|
| Massachusetts | *pâsuk* | and | *n'qut, nequt.* |
| Chippeway | *payzhik* (*béjig*) | | *ningoto.* |
| Cree | *péïak, paťak,* | | *nikut* 'some one,' *nikuton* 'formerly.' |

A note in Cotton's vocabulary of the Massachusetts language distinguishes these names thus: "*Nequt*, a thing that is past. *Pasuk*, a thing in being." This note has puzzled more than one writer on the Algonkin languages.[9] Cotton himself had only half caught the true distinction between *pâsuk* 'one *only*,' literally, 'a small thing,' and *n'qut* 'first' or 'fore-est,' 'beginning.' The latter was used when speaking of a *one* which had been (or necessarily must be) followed by *another*, and in this way came its appropriation to "a thing that is past," i. e. a *former* thing. Hence, Mass. *nukkone* 'old,' i. e. passed by, and the ordinal *ne-gonne* 'first,' and *ne-kutche* 'the beginning,' 'it begins.' The prefixed *n'* in eastern Algonkin numerals is merely demonstrative.

*Pâsuk* is a contraction of *piasuk* (*peasik*, Eliot) 'very small,' the diminutive of *piak* 'small, little.'[10] Comp. Chip. *pangi* 'a little,' *pangishe* 'very little.' The root, *pi*, is seen

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[9] See Mr. Pickering's note, in his reprint of Eliot's Indian Grammar (2 Mass. Hist. Soc. Coll., ix.) p. xlv.; Duponceau's *Mémoire*, 389, 390.

[10] Abn. *bi*, plu. *bi-ak*, Mass. *piak*, a 'grain,' 'bit,' or 'bead' of shell money; whence the name adopted by the English for unstrung 'peag' = Abn. *wa*ⁿ*ban-biak* 'white beads,' Eng. 'wampompeag.'

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in Cree *peïak* 'one' and *api-s* 'small,' dimin. *apísis* 'very small.' The little finger being counted as 'one,' *pâsuk*, *bèzhik*, 'the very small' has, as a result of association, been substituted in several dialects for *n'qut*, *nikoto*, as the name for 'one,' but the latter reappears in the composition of the higher numerals: e. g. Cree *peïak* 1, *nikot-wassik* 6, i. e. 'one over'; Abnaki *pezuku* 1, *nekud-a"s* 6, *nekuda"nkáo* 11.

The following are some of the names of the little finger, in North American languages:—

| | |
|---|---|
| ALG. Cree | *iskwe tchitchanis* 'last little finger.' |
| Chippeway | *ishkwe' nindj* 'last of the hand.' |
| Abnaki | *askwanmi-retsî* 'youngest (last born) of the hand.' |
| Massachu. | *muttásonitch* 'youngest son (*muttásons*) of the hand.' |
| DAK. (Sioux) | *shashté* ? Comp. *chi' stin* 'little'; *chatan'* name of a fourth son. |
| Minnitari | (Hidatsa) *sháki-kazhi* diminutive of *sháki* 'hand.' |
| Mandan | *ungkní ingka* 'little finger.' Comp. Iowa *î-yangke* 'one.' |
| MUSKOKI (Creek) | *enke-echhuswuche* 'hand's little daughter.'[1] |
| Choctaw | *iblak-ūshi-ūhli* 'hand's little son.' |
| PAWNEE | *skēts-pit* 'finger little.' |

Gallatin's vocabulary (from Parry) of the Hudson's Bay Eskimo gives *eerkitkoka* (Greenl. *ekékkok*) 'little finger' as the name for 'ten.' The Algonkin 'tens' are related to—but not derived from—names of this finger. These will be noticed hereafter.

2. 9. The Fourth finger—second by Indian reckoning—is in some American languages, as it has been in many languages of the eastern world, 'the nameless' (Sansk. *anâman*, *anâmikâ*; Lithuan. *bewardis*; Tibet. *mingmed*). In others, it is designated only by its position 'next the little' or 'next the middle' finger. In mission-Indian it has received the name of 'ring finger.' Lacombe gives Western

[1] For translations of this and other Muskoki (or Creek) finger-names, I am indebted to Mrs. A. E. W. Robertson of the Tullahasse mission, and to Buckner and Herrod's Muskoki Grammar. For other languages of this group, I use the Rev. Cyrus Byington's "English and Choctaw Definer" (1852) and his Choctaw Grammar (posthumous) edited by Dr. D. G. Brinton (1870), and valuable vocabularies (MSS.) of the Muskoki, Hitchitee, Coassatti, and Alabama, collected by Gen. Albert Pike, in possession of the Smithsonian Institution, which I hope will soon be published, and with them, one of the Muskoki language, compiled by the Rev. W. S. Robertson and Mrs. Robertson. For the *Pawnee* and related *Arikara*, I rely on Dr. Hayden's vocabularies.

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Cree *atchani-tchitchân* (from *atchanis* 'a ring'), and so Von Tschudi in his Wörterbuch of the Kechua of Peru has *siui-rucanu* 'ring finger,' from *siui* 'ring.' In a few languages, its name denotes 'becoming smaller'—whether from its shape, more 'tapering' than other fingers, or from its size, as between the middle and little fingers, is not certain.

Dakota *shaste iyokihe* 'little-finger next-to.'
Minitari *shaki-kazi-utidu* 'that which the little finger joins,' or, as Dr. Matthews (Hidatsa Dictionary) translates, 'base of the little finger.'
Muskoki *enke-hochef kŭ sekŭ* 'hand's name-without,' 'the nameless.'

As a numeral I find the name of this finger only in the 'nines,' and here only in the

Eskimo, Hudson's Bay *mikkeelukkamoot* 'nine' = 'fourth finger' (Parry).
Greenland *mikkelerak*, 'fourth finger,' literally 'it becomes smaller.'
Algonkin, Shyenne *na-so'toyōs* 'my fourth finger'; *sohk'tu* 'nine.'

3. 8. The 'Middle' finger is so named in almost all languages, but it not unfrequently has the additional designation of 'the great' or 'chief.' It gives in many dialects a name—but not generally *its own* name—to the numerals 'three' and 'eight.' In the Algonkin languages, of two expressions for 'in the middle' or 'half-way between' (Mass. *noë'u* and *nashaüe*, Chip. *nawaii* and *nassawaii*), one is given to the *finger*, the other to the *numeral*.

| | | |
|---|---|---|
| Abnaki | *na"wi-retsi* 'middle of hand'; | *nass* 'three.' |
| Chippeway | *náwi-nindj* " " | *nisswi* 3; *nîjwassi* 8. |
| Cree | *tâwi-tchitchân* 'middle finger;' | *nistoo.* |
| Mass. | (*nashaue, 'shawe* 'half-way'); | *nish, nishwé, 'shwi-* 3; *shwosuk* 8. |
| Arapoho | (*naüthĭ* 'in the middle'); | *nais* 3; *naisa-toh'* 8. |
| Sauki | | *nissoa* 3; *shóashic* 8. |
| Shyenne | *no'toyōs* 'middle finger'; | *nā'a* 3; *na-nōhk'tu* 8. |
| ? Blackfoot | *nohkh, noho-ka* 'three.' | [Mass. *noeü* 'in the middle.'] |
| DAKOTA | *napéochókaya* 'middle finger' (*ochókaya* 'in the middle'). | |
| Minitari | *sháki-dumátadu* 'middle of the hand'; *dámi, náwi* 'three' (*dumáta* 'in the middle,' *nuwah'taru* 'between'). | |
| MUSKOKI (Creek) | *enke nŭrkŭphuerŭ* 'hand's middle-stander.' | |
| Choctaw | *ibbak ūshi iklănna* 'hand's middle son.' | |
| Pawnee | *skĕtsi-kadíku* 'half-way finger.' | |
| Navajo | *hullah náizi* " " (*hulah', elá* 'hand'). | |

In one dialect of the Eskimo (Hudson's Bay) the name, as in the Shyenne above-noted, appears only in 'eight:'

*kittuklimüt* 'the middle finger,' 'eight' (Parry).

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4. 7. The Fore finger has been, almost universally, the 'showing finger' or 'index.' Names for 4 and 7 are in Algonkin languages taken from it, or from the act of showing, or their connection with it is established through the demonstrative pronouns:

| | |
|---|---|
| Eskimo (Greenl.) | *tikek* 'the pointer.' |
| ALGONKIN, Chip. | *ino'i-nindj* 'showing finger'; *niwin* (= *niouin*) 'four.' |
| Cree | *itwahigani-tchitchiy* 'pointer finger'; *néwu* 'four.' Comp. *naah* 'that yonder!,' *awâh* 'this one.' [As was before remarked, the *n'* prefixed to the Algonkin numerals is a demonstrative particle, and does not belong to the root.] |
| Massachusetts | *yau* (Eliot; = *iéu*) 'four'; *yeu* 'this,' 'there.' |
| Narragansett | *yòh* " *yò* 'there,' 'that way!' |
| Illinois | *niwi, niui* " *newa, newe* "voilà, regarde là," *iwa, iiwe* "le voilà." |
| Shyenne | *na-ni'sotoyōs* 'my fore finger'; *ni'soto* 'seven';[2] compare *nísiwo* 'that.' But Shyenne *nipa* 'four' has a different origin. |
| Arapoho | *yen* 'four'; *ti'ENa* 'to touch one *to call his attention* to anything' (Hayden). |
| Blackfoot | *ni-su'i* 'four'; *súmis* 'look!' |
| DAKOTA | *napé tokaheya* 'hand's first' (modern?). |
| | *napé apazo* 'hand's pointer' (*pázo, apázo* 'to point to, to show by pointing' — √*pa* denoting action of the *hand*). |
| MUSKOKI (Creek) | *enke-esmelkŭ* 'hand's pointer.' |
| Choctaw | *ibbak-ūshi-tikba* 'foremost (or eldest) son of the hand.' |
| | (The name for 4 is not, in any language of the Chahta-Muskoki group, taken from this finger.) |
| Navajo | *tî*ⁿ 'four'; *tî* 'here,' 'this'; *n'la'-te* 'there' (*la* = hand). |
| Apache | *ti-i* " *ti* 'this,' 'who'; *ti-tchi* 'this day.' |

5. 6. The Thumbs mark 'five' and 'six,' but rarely, if ever, give a name to either number, in American languages. In Algonkin, and in many other American languages, the thumb is the 'big,' 'thick,' or 'stout' finger; sometimes, 'the chief.'[3]

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[2] The Zulu corresponds with the Shyenne in taking numeral names from the fingers of the *second* hand. "The Zulu verb *komba* 'to point,' indicating the fore finger or 'pointer,' makes the numeral 7. Thus, answering the question, 'How much did your master give you?' a Zulu would say, '*U kombile*' 'He pointed with his fore finger,' *i. e.* 'he gave me seven,' and this curious way of using the numeral verb is shown in such an example as 'amahasi *akombile*' 'the horses have pointed,' *i. e.* 'there were seven of them.'" — Tylor's Primitive Culture, i. 228.

[3] Compare Latin *pollex*, "vocatus quod inter cæteros *polleat* virtute et potestate." — Isidori *Origines*, quoted by Pott, 'Zählmethode,' 288.

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| | |
|---|---|
| ALG. Chippeway | *mitchitchi-nindj* 'great finger.' |
| Cree (Western) | *misi-tchitchân* " " |
| Abnaki | *aghitkwe-retsi* 'chief (greatest) finger.' |
| Massachusetts | *kettiquanitch*, Blackfoot *omaki'chis* (*omukh-u* 'great'), and Shyenne *nama-a-im'oïk*, have the same meaning. |
| DAKOTA | *napa'hunka* 'the hand's elder' (*hunka* 'parent, ancestor, elder brother.' Riggs). |
| MUSKOKI | *enke ichhki*, and Choctaw *ibbak ishke*, 'the hand's mother.'[4] |
| PAWNEE | *skēts'-skūts* 'large finger.' |
| NAVAJO | *hullah tsó* 'thick' or 'big finger.' |

III. Names of number that are not derived from the fingers individually. Names for 'one' and 'two,' as has been said (p. 46), must have preceded digital numeration.

1. There are, as we have seen, two expressions for the numeral 'one'; namely, '*only* one' and '*first* (fore-est) one.' In Algonkin languages these are represented in Mass. *pâsuk* and *n'qut*. The former expression is sometimes related to the pronoun of the first person singular and to the demonstrative 'this'; sometimes it has the meaning 'alone,' 'single,' or 'by itself.' Its root in Algonkin and Dakota languages denotes 'small.' The other expression for 'one' (=Mass. *n'qut*) is from a root denoting *priority* or *fore*-coming, in order or time, 'beginning;' and it has in many languages the secondary meanings, 'old,' 'aforetime,' etc.

In the Dakota family, one of these expressions is used for the cardinal, the other to form the ordinal: e. g. Dak. *wanzhi'*, *wa$^{n}$zhi'-da$^{n}$*,[5] *wa$^{n}$cha* 'one'; *toka'heya* 'first' (from *toka'* 'at the first'); Hidatsa (Minitari) *duétsa*, *luétsa* 'one,' *ítsika* 'first'; Iowa *īya$^{n}$gke* 'one,' *pakranaha* 'first.' Between phonetic decay and dialectic growth,[6] the Dakota 'ones'

[4] So in Malayan (Pott, 'Zählmethode,' 299), and in American Maya, Huasteca, Tamanaca, etc.; and in Botocudo *nipo-diik* 'hand's mother.'

[5] Ihankton *wa$^{n}$zhi-na*. The suffix, *dan*, Ihank. *na*, is restrictive; 'one *only*.' "The form in counting is *wancha*" (A. L. Riggs) or, as Dr. Hayden writes it, *wunch*. This is further contracted in the Ponka to *win*, and in the Omaha to *wi*.

[6] Or rather, between "laziness and emphasis," as Mr. A. H. Sayce (Principles of Comparative Philology, 16) prefers to call the two great causes of phonetic change. Compare Whitney, Language and the Study of Language, 70, 95. In no American family of language is the operation of these principles more apparent and more troublesome than in the Dakota. Not merely that *wa$^{n}$zhi'dan* is shortened to Omaha *wi* or changed to Mandan *makh'ana* and Iowa *iyangke*, but in the same dialect, and from the lips of the same speaker, a name

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have become so widely variant that they cannot all be confidently referred to a common root. In several dialects, if not in all, the numeral has lost all *consciousness* of its roots, becoming a mere phonetic mark. Compare

| | | | |
|---|---|---|---|
| Dakota | *wa$^{n}$zhi'da$^{n}$* | Ponka | *win* |
| Assiniboin | *washi'na* | Omaha | *wi* and *miaχtcheh* |
| Winnebago | *izhak'ida, hezunkera* | Mandan | *makh'ana* |
| Iowa | *īya$^{n}$gke* | Osage | *minche* |
| Hidatsa | *duetsa, luetsa* | | |

and — least conformable of all — Aubsaroke *hamat'*.

I was at first inclined to refer the Dakota *wa$^{n}$zhi* to the root *wi$^{n}$zh* 'to bend,' from the *bending down* of the little finger in counting. Comparison of ten dialects of the same family makes it more probable, if not absolutely certain, that it is the equivalent of Algonkin *pâsuk* 'the least' or 'very small': compare with *wa$^{n}$zhi*, *wa'nikhadan* 'very little' and *wa$^{n}$ícha-dan* 'very little, none'; *wá$^{n}$ske*, the name of the fourth child in a family, if a daughter (remembering that the *thumb* is 'parent' or 'elder' of the hand), and *wánka* 'soft, weak, tender.' With Iowa *īya$^{n}$gke*, comp. Mandan *ungkni-ingke* 'hand's little one'; and Winneb. *izháki-da*, with *wachek* 'young.'[7] In the Assiniboin, *nape* 'hand,' with

may vary as *nowassa, duetsa, luetsa* (Minitari) 2; *pitika, pirika*, 10; *nahwi, dami*, 2; *bira, mida* 'a tree,' etc. In this last-mentioned Dakota dialect, the Hidatsa (called Minitari and Gros Ventres), Dakota *y* becomes *d* (*ya* 'thou' and *ya* 'to go' = *de*), *b* and *w* are interchangeable with *m*, and *l, n*, and *r*, with *d* (Matthew's Hidatsa Dictionary and Grammar, p. 28).

[7] Since this paper was written, I have been favored by the Rev. A. L. Riggs of the Dakota mission (Santee Agency, Nebr.), with some notes on the Dakota numerals, to which his father, the Rev. Stephen L. Riggs, contributed some suggestions. For the grammar and vocabulary of the language, I could have no higher authority; and when I have ventured to differ from Mr. Riggs's conjectures as to the origin of the numerals, it has been only after thorough comparison of the names in eleven languages of this family, with whatever light was to be had from published and manuscript vocabularies. Of the names for the lower numbers, Mr. Riggs writes: "I have thought that, as high as 'three,' the names of numbers arose from sight of outward objects, as 'one' evidently does." "*Wanji*, root *wan*, interjectional, 'see!'; *ji* [*zhi*] is not necessary, as the form in counting is *wancha* (for *wan-e-cha*). *Ji* means 'separately'; *dan* added has something the force of 'only.' *Nonpa*, 2. Root, *onpa* 'to lay on,' 'to add.' The origin of the *n* will be sought in different directions, according to the theory of the numeral. . . . . It may be that it comes from *nape*. While *nape* is the whole hand, in composition it may stand for a 'finger,' which is *nape-sukaza* = 'a single hand'" [or, 'a portion, *particle* of hand'?].

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*nape-washi* 'finger,' i. e. 'hand's little one,' and *washi-na* 'one'='a finger only.' The Dakota for 'fingers' is *nap-sukáza* 'hand's small portions,' from *su* 'seed, grain,' i. e. 'a particle,' which in Dakota more commonly becomes, in composition, *chi* or *cho*, as in Dak. *chika-da*$^{n}$ 'very small'= Assiniboin *chika-na*=Omaha *shinga* 'young,' Mandan *-sūk*, and diminutive *shūke*, as a suffix.[8]

In Hidatsa *duetsa* (otherwise *luetsa*, *nowassa*) there is wider divergence from the root; but we recover the meaning through *shaki-adutsa'mike* 'fingers' (*shaki*=hand), *adutsúa* 'a seed,' *adutsóhi* 'a point, a tapering end or part' (Matthews).

Aubsaroke (Crow) *hamat'* has the same meaning. Comp. Mandan *hámahe* 'small,' *sūk-hámahe* 'little child'; and Aubs. *amue* 'a grain, a kernel.' The suffix *-at*, *-ate*, is the common Aubs. diminutive. *Hamat'* 'one'='the least.'

In the CHAHTA-MUSKOKI family, we find the two forms—'one only' and 'the first,' represented in

Choctaw *achŭfa* 1; 'sole, single, only one.'
*tikba* 'the first'; also, 'before,' 'ancient,' 'of time past.'
*ŭmmona* 'once.'
Muskoki *hŭm'kin* 'one.'
Coassatti *chafáka* and Alabama *chafuhka-schie* 'one.'

Without attempting an exact analysis of these names, I remark (1) that Ch. *ŭmmona* 'first,' *hĭmona* 'once,' is merely a demonstrative: *himo*, *himak* 'now,' 'at this time,' 'to-day'; *himonasi* 'instantly,' &c.; obviously related to Muskoki *hŭm'kin* 1,[9] and *homa* 'before': (2) that Ch. *achŭfa*, and Coas. *chafáka*, seem, like the Algonkin and Dakota 'ones,' to be derived from a root meaning 'very small,' 'a grain, particle, or point'; comp. Ch. *chufak* 'an awl,' 'a nail,'

[8] Comp. also, Dk. *su*$^{n}$*ka* 'a younger brother' (Omaha *sanga*), contracted to *su*$^{n}$; *sha-ke'* 'a claw, a nail' (Om. *sha-ge*); *cho* and *su* 'a kernel,' 'grain,' 'seed.'

[9] Mrs. A. E. W. Robertson (wife of the Rev. W. S. Robertson, of Tullahassee, Ind. Territory), whose knowledge of the Creek language is as thorough as that of any one now living, writes (under date of Aug. 3d, 1874): "I see no connection between the [lower] Muskoki numerals and the names of *hand* or *fingers*, unless *hŭmke* 1, may be a contraction of *heyŭ enke* 'this hand.' In contraction, *m* and *n* seem to run into each other: e. g. *momet* becomes *mont*, *heyŭn* becomes *hŭm*, before words beginning with *m*; as *heyŭn mechetŭ* 'to do this' becomes *hŭmmechetŭ*, *heyŭn maketŭ* 'to say this' becomes *hŭmmaketŭ*. In a similar way, *heyŭn enke* ['this hand'] might become *hŭmke* 'one.'"

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*chush* 'tip,' 'point' (e. g. *ibbak-chush* 'finger nail' = 'hand's point'), *ibak-chufanli* 'tapering,' *chubi"hasi* 'little, not much.'

Pawnee *úska* 1, is evidently from a root found in *pid-úski* and *pir-ūski* 'young,' *kitalūs'ki* 'small,' and probably in *skēts* 'finger.'

2. Names for two seem to come from roots denoting (1) separation or distinction, as 'that,' 'the other,' (2) likeness, equality, or opposition, (3) addition, 'putting to' or 'putting with,' (4) coupling, pairing, or the like. These names, as has been said, must have preceded finger-counting or any formal numeration. They are often related to — possibly may have in some languages been derived from — names of natural pairs, as 'arms,' 'hands,' 'feet,' 'wings,' etc. From them or from the same roots come, by later derivation, names of *artificial* pairs, e. g. 'moccasins,' 'leggings,' etc., and of dual relation, as 'wife,' 'husband,' 'brother,' etc.[1] And here is the explanation of that connection between names of the 'hand' and 'two,' which Mr. R. Ellis regards as evidence "that *hand* may = *fingers* = *finger-finger*," and as "helping to exhibit the radical affinity which unites the North American languages" (p. 6).

Of natural 'pairs,' the *hands* have most often given a name to — or received it from — the numeral; because they *are* two, not because they "= finger-finger." Pott (Zählmethode, 29) notes Puris (Brazilian) *core* 'hand,' *curiri* 2; Hottentot *t'koam* 'hand' and 2; Sanskrit *kara* 'hand,'

[1] For example, Kioway *ki-iâ* 'husband,' *ki-u"* 'wife,' *gi-ă* 'two,' and *ki-atsi'* 'near,' i. e. 'next to'; of all which the common root is found in *ki-n* 'he,' i. e. 'another'; and Choctaw *tuk-lo* 'two,' *tek-chi* 'wife.' The connection of the grammatical dual with the idea of correlation, or of collocation merely, is illustrated by a peculiarity of Kechuan speech. The regular termination of the plural is *-cuna*, but there is a special plural in *-ntin*, for objects belonging to or associated with the noun in the singular: e. g. *khuasi* 'house,' *khuasi*NTIN 'all who belong to the house' or are 'of the household'; and with a noun denoting affinity or consanguinity this suffix *-ntin* forms a *dual*, including two individuals in correlation: e. g. *chosa* 'husband,' *chosantin* 'husband and wife'; *mama* 'mother,' *mamantin* 'mother and child'; *ususi* 'daughter,' *ususintin* 'daughter and mother'; *pana* 'sister,' *panantin* 'sister and brother'; with *masi* 'companion' and *yana* 'servant' it forms nouns meaning 'a pair,' *masintin* being more commonly used for persons and *yanantin* for inanimate objects. — Von Tschudi, die Kechua Sprache, pp. 95, 161.

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*bâhu* 'arm,' *paksha* 'wing,' and *nêtra* 'eye,' all used also for 'two.' The Samoyed Tawgi, also, expresses the number 2 and the substantive 'hand' by terms nearly identical.[2] In Labrador Eskimo, Richardson's vocabulary has *maggok* and *aggait* for 2, *agga* 'hand' and *aggait* 'the hands.' In the Algonkin and Dakota languages names for 2 and for 'hands' or 'arms' seem to be nearly related, either by derivation of one from the other or of both from a common root. In Algonkin dialects, compare —

| | | | | |
|---|---|---|---|---|
| Chip. | *-nindj* 'hand' | *nij* | 'two.' | |
| Cree | *-nisk* " | *niso* | " | (*-nisk*, however, being used only in composition, as *kitchi-nisk* 'right hand'). |
| Mass. | *-nutch, -nitch* 'hand' | *nîs* | " | cf. *nîsin* 'copulat,' *nichaïï* 'she gives birth to a child.' |
| Abnaki | *-retsi* " | *niss* | " | |
| Illinois | | *ninch-ui* | " | |
| Miami | | *nîchué* | " | |
| Arapoho | *-ichet* " | *nis* | " | cf. *inush* 'arm,' *inachása* 'the other side,' *neshíse* 'eyes.' |
| Shyenne | " | *nicH* | " | |

In one Algonkin language only, the Micmac (of Nova Scotia), we find another name for 2, *tabu*, i. e. 'equal' ('par,' 'pair'); but that it had once a wider range, we have proof in the Cree *tepa-kup*, Abnaki *ta$^{n}$ba-wa$^{n}$s*, Mohegan *tupou-wus*, and Montauk (L. I.) *tu$^{m}$pa-wa* 7, i. e. 2 + (or 2 of the second hand). The root, in the sense of 'equal,' and of 'enough,' 'sufficient,' is found in all Algonkin languages: e. g. Mass. (redupl.) *tatup*, *tatuppi*, Abn. *tetebi-wi* 'equally,' etc.; Cree *niya-tipiyaw* 'I my-*self*,' *tipiyaw* 'he him-self,' etc., *tipi-new* 'he measures it,' i. e. 'makes it equal to,' *tepi* 'enough,' etc. Mass. *tatup-pin* 'a string' or 'cord' is as near akin to Micmac *tabu* 2, as is Engl. 'twine' to 'twain.'

[The presence of this 2 in one Algonkin language, and evidence (in the 'sevens') of its former use in others, suggested a doubt as to the origin of the relation I had believed to exist between 'twos' and 'hands' in this family of speech. The authority of W. von Humboldt[3] and of Pott

[2] Benloew, Recherches sur l'Origine des Noms de Nombre, p. 50.

[3] Die Kawi-Sprache auf der Insel Java, Bd. I. s. 20 ff.

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disposed me to recognize this relation. A comparison of the several Algonkin dialects and evidence supplied by other American languages led me to question it, and now I am nearly convinced that the connection of the numeral with natural duals, 'hands' or the like, is not by derivation of one name from the other; that the likeness, if not accidental, is a consequence of derivation from a common root; and that the primary conception of the Algonkin 'two,' whether expressed by Micmac *tabu* or Chip. *nij*, is that of 'sameness,' 'likeness,' or 'equality,' represented in the modern Chip. *iji*, Cree *isse* 'so,' 'such.'

The first three numerals are, in the Massachusetts dialect,

1. *ne qut*, 2. *n îs*, 2. *n ish;*

in the Chippeway,

1. *nin goto*, 3. *n ij*, 3. *n isswi.*

In these the prefix is, apparently, merely demonstrative (Mass. *ne* 'this,' 'that'), and does not belong to the root. In the 'two,' we have, I think, the Chip. *iji*, Cree *issi* and *iji*, 'so,' 'so as,' 'like'—which Baraga (Otchipwe Grammar, 493) classes as a conjunction, and Howse (Cree Grammar, 132, 142) as "the relative adverb of manner" and also "a generic noun." As a *verb*, it signifies, in the Chippeway, 'to be like' or 'the same as': e. g. *anishinabeg nind*-IJI 'I dress like (appear like) an Indian'; *iji-nagwad* 'it looks like' something, etc. *Niji*, contr. *nij*, 'two,' is 'this, such as' or 'like' the first — corresponding nearly to Micm. *tabu* 'par,' 'that which pairs.' The same root is in the Chip. *nidji*, or *nidj'* 'like myself,' 'my fellow,' 'alter ego,' which is only distinguished from the numeral by the change of pronoun in the second and third persons — *kidji*, *kidj'* 'thy fellow,' 'thy equal,' *widj'* 'his fellow, or equal'—used chiefly as adjectives, as *widj'-anishinaben* 'his fellow-man.' The dialectic variations of this particle correspond with those of the numeral 'two': Chip. *iji* and *n'ij*, Cree *isi* and *niso*, etc. In the Illinois dialect, *ninchui* is 2, *nichi* or *nigi* "comme cela" (Gravier).

If, then, Algonkin 'hands' and 'twos' are directly related, it is nearly certain that their relation is that of derivatives

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from a common root, or that the former receive their name from — instead of giving it to — the numeral. And this appears to be true of the relation of corresponding names in other American families of speech.]

The Dakota 'two' is the most constant of all the numerals, and dialectic variations nowhere disguise its relation to natural 'pairs.' The 'twos' are:

(Sioux) Dakota *no"pa*, *nōm*, Omaha *nombá*, *wamba*, Mandan *nūm'pa*, Osage *nombaugh*, Ponka *nánba*, Iowa *nówe*, Winnebago *nōmp*, Aubsaroke *nōmpe*, Hidatsa *nópa*, *dópa*.

With these compare: Dak. *nape* 'hands' and *napin* 'a pair, they two,' *ha"pa* (a pair of) 'moccasins,' etc., Om. *nomba* 'hands,' 'fingers,' Osage *nambe* 'hands,' Ponka *nanpé*, Iowa *nawé-pa* 'finger' = 'hand's head, or tip,' Winneb. *nábara* 'hands,' *namp-weisara* 'fingers,' Aubs. *núpere* 'both,' Hidatsa *huupa* 'moccasins.'

The primary meaning of the root, *o"pa*, seems to be 'to put to, with, on, or against,' 'ap-ponere' or 'op-ponere'; as a verb, *o"pa* is 'to place or lay any thing' on or with another: comp. *o'pa* 'to go with,' 'to be at' or 'on,' and (contr.) *om* 'with';[4] *ao"pa*, contr. *ao"*, 'to lay or place on' (as, wood on the fire); *sa"pa* 'over, beyond, more than,' used in forming the numerals 11 to 19 (e. g. *wikchemna sa"pa topa* 14 = 10 + 4); *ha"pa* 'moccasins,' *aka-sa"pa* 'opposite,' 'set over against,' etc. Perhaps, *a"pa* 'day' (*a"pa-o* 'dawn') is from the same root. We shall find it again in *topa* 4. The prefixed *n'* in *no"pa* — which in other dialects varies to *w* and *d* — seems to be merely a demonstrative or directive, as in the Algonkin numerals, and as in the Dakota verbal particle *na* 'take it' (imperative only), and in *no"* or *nu"* 'be it *so*.'

In the Chahta-Muskoki group, the 'twos' have a similar origin, in the notion of 'coupling,' 'mating,' or 'ad-joining':

Choctaw *tuklo*, Muskoki (Creek) *hokkólin*, Hitchiti *tōkh'lun*, Coassati *tókolōō*, Alab. *tókolō-chie*.

The root is represented in Choctaw *okla*, a collective

[4] "Koelle, Gram. of Vei Language, notices that *fēra* means both 'with' and 2, and thinks the former meaning original (compare the Tahiti *piti* 'together,' thence 2)." Tylor's Primitive Culture, i. 235.

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pronoun used to form the plural of nouns and both the *dual* and plural (3d sing.) of verbs, with the meanings 'they two,' 'they,' 'people,' 'tribe,' etc., modified as *oklu"ha* "all, the entire crowd, number, or quantity" (Byington, Choctaw Gram., 32, 41). The Choctaw *t'*, prefixed, probably represents the "distinctive preposition" *et* 'here, this way,' etc. (id. 42), a demonstrative. From the same root, apparently, are Ch. *ho'tohla*, conjunction copulative, 'also,' 'of the same class,' *hitukla* (=*et-okla*) 'twice,' and the verbs *tok-chi* 'to tie,' and *iba-ta"kla* 'to go with,' 'to accompany.' Comp. Musk. *sahokolŭ* 'twice,' *hlisa-hokolat* 'secondly,' etc.

Athapascan 'twos' are, more commonly, related to names for 'feet' than to 'hands.' Chepewyan "*keh* 'foot,' 'shoe,' 'track'" (or their plurals), is often used as a numeral for 2 or 'a pair.' In the Apache, 2 is *na-ki;* 'foot' or 'feet,' *ki-e;* 'moccasins,' *si-ke;* Navajo *na-ki* 2; *iké* 'foot'; *kikh* 'moccasins.'[5]

3. Names for 'three' when not taken directly from the middle finger or 'half-way' of the hand, sometimes have the meaning, 'beyond,' 'further' ('trans'), or 'greater'; sometimes 'much,' 'the many'—a *plural* as distinguished from a *dual.*

All the Algonkin 'threes' are of the 'middle' (see p. 52, ante), except the Micmac *tchicht*, which seems to have had the meaning of 'more' or 'again' (= Delaware *tchitch* 'still more').

In the Dakota family, the 'threes' exhibit wider variance than the 'twos' from the original stock:

| | | | |
|---|---|---|---|
| Dak. | *yámni, yámini* | Winneb. | *tą", tau"* |
| Assinib. | *yámini* | Iowa | *tányi* |
| Mandan | *námeni* | Omaha | *thábathi* |
| Hidatsa | *dámi, náwi* | Ponka | *tha'bthin* |
| Aubsar. | *nam* | Osage | *laubena* |

The etymology is obscure. Comparing the Dakota and Aubsaroke forms with the Omaha, Ponka, and Osage, it seems probable that *-am* is a contraction of *a"pa*—as *nom* is the contraction of *no"pa* 2, and *tom* of *topa* 4. This would

[5] Gallatin, Synopsis of the Indian Tribes, p. 215.



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refer the numeral to the same root with the 'two.' The prefix may be the simple verbal *ya* (Hidatsa *de*) 'going,' as in *áya* 'they go together' and 'it becomes,' or more probably the inseparable preposition *i* (combining with the following *a*, as *ya*) meaning 'next in order,' 'again.' This would make *yam* = *i-ao*$^{n}$*pa* or *ya-o*$^{n}$*pa* — agreeing nearly with the verb *iyáo*$^{n}$*pa* 'to lay on, to place on,' of Riggs's Dictionary. The pronunciation of the numeral is marked *ya'mni*, which suggests a reference to the verbal root *mni* 'spread out' or *mna* 'gathered, collected'; but the other dialects show that this root is not essential to the name, and if it enters into the composition of the Dakota name, it is probably supplementary to the principal root, so that *ya'mni* = *yam-mni*.[6]

The Winnebago and Iowa names have, apparently, a different origin, and Winneb. *ta*$^{n}$ may be the (regular) contraction of *ta*$^{n}$*ka* 'great.'

In many dialects of the west and southwest, the name of the numeral has this meaning of 'great,' 'much,' 'many,' or the like: e. g.

| | | |
|---|---|---|
| YUMA (Mojave) | *hamóco* 3, | *húmik* 'great.' |
| (Cuchan) | *hamúk*, | *n'yamúk* " |
| PAWNEE | *tawît*, | *-tawio* (suffix) 'over, above,' *hawa* 'more.' |
| Arikara | *tawhít* (*wh* English), | *terhue* 'many,' *tiërwheu* 'great.' |
| NAVAJO | *tahh*, | *thla* 'much,' *na-tá-ni* 'a chief.' |

4. Above 3, traces of digital numeration become more common, but the fact that in many languages 4 is a 'doubled 2,' or pair of pairs, seems to indicate that in these its conception and name were earlier than finger-counting. All

[6] The Rev. A. L. Riggs, in his letter of July 27th, before mentioned, regards *mni* as the root. He writes as follows:

"*Yamni*; root MNI or MNA. *Mni* is 'to gather in a circle or *group*'; as *yu*MNI *wachipi* 'the circle dance,' MNI*chiyapi* 'assembly.' Three is the smallest number, of course, that can make a group or circle. The correlate root MNA is more widely in use, and the meaning clearer: *ka*MNA 'to acquire or gather for one's self,' MNA*yan* 'to gather,' *opa-mna* 'a cluster,' as of young trees growing up out of the root or stump of an old one. If *yamni* comes from *mna*, the change of *a* to *i* would be for euphony. If *yamni* comes from the sight of outward objects [preceding formal enumeration], then we may find the *ya* to signify grouping by *calling* — 'calling' another to the two. If it springs from the finger count, the origin of *ya* is not clear. As *causative* affix, it should come after."

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Algonkin 'fours,' as was seen, are *demonstrative*, derived from the index-finger; but in two or three dialects the 'eights' suggest a primitive numeration by *pairs*. Of this mode I will speak more particularly hereafter, and here mention only the Dakota 4, formed apparently as a 'pair of pairs':

Dak. *tópa*, contr. *tom*, Hidatsa *tópa*, Mandan *tópe*, Ponka and Omaha *dúba*, Iowa *tówe*, Winneb. *chōp*, Aubsaroke *shōp*.

There are several Dakota expressions for 'pairs' and 'doubles'; *napin* (from *nape* 'hands'?) 'they two,' 'both,' *sakim* 'two together,' and from the numerals, by the prefix *ta*, as *ta-wa"zhi* 'a pair,' *ta-no"pa* '2 pairs,' *ta-yamni* '3 pairs.' In *tano"pa*, or rather in the earlier *ta-o"pa*, *ta-ópa*, '2 pairs,' we have, I think, the origin of *topa* 4.

In some languages 'all the fingers' give the name to this numeral, as, apparently, in Pawnee *skítiks* 4, = *skēts-iks* 'fingers [of] hand.'

5. There is much diversity, even in languages of the same stock, in expressions for 5 and 10. In these sometimes, but by no means always, is found a name of 'hand' or 'fingers,' or a suggestion of such name. In the instances — comparatively few — in which names for 'hand' and 5 are identical, or nearly so, we cannot confidently decide which of the two is borrowed from the other.[7]

Of Algonkin 'fives' there are two principal types:

(1.) Massachusetts *napanna*, meaning 'on one side,' i. e. 'one of the two hands.' It is the Chip. *nabane*, Cree *nabat*, but is not in either of those dialects used for the numeral. In Abnaki *bare-nesku*, Del. *palena*CH, the name for 'hand' is added, the expression corresponding to Chip. *-bane-nindj* 'of one hand,' as in *ningoto-bane-nindj* 'one handful,' *nin*

[7] "A. v. Humboldt's plausible comparison between Skr. *pancha* 5, and Pers. *penjeh* 'the palm of the hand with the fingers spread out, the outspread foot of a bird,' as though 5 were called *pancha* from being like a hand, is erroneous. The Persian *penjeh* is itself derived from the numeral 5, as in Skr. the hand is called *panchaçâkha* 'the five-branched.' The same formation is found in English; slang describes a man's hand as his 'fives,' or 'bunch of fives,' thence the name of the game of fives, played by striking the ball with the open hand, a term which has made its way out of slang into accepted language."—Tylor's Primitive Culture, i. 235, note.

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*nabane-nindj* 'I am one-handed,' 'have only one hand,' etc. The Abnaki *na"neda* '5 times' and *na"na"kao* 'fifteen' (=5+) are from a different root, and are related to

(2.) Chip. *nánan*, Cree *niannon*, *niyânan*, Micm. *nān*, Moheg. *nunon;* and Shawano *nialin-ui*, Miami *yàlan-ué*, Illin. *miaran-ui*, etc. These, though perhaps not all from the same root, have nearly the same meaning, 'gone,' or 'spent,' i. e. all the fingers of one hand. Comp. Cree *niyân* 'va, pars,' pl. *niyânk* 'allez, partez,' a "verb used only in these two persons of the imperative"; *niyâk* 'forwards,' 'onward' (Lacombe, Dict. Crise); Shawn. *niala*, Illin. *miara*=Cree *niyân*.

Dakota 'fives' are plainly *digital:* Dak. *zápta"*, Om. *sátan*, Ponka *sáta*, Iowa *tháta*, Osage *sattah*, Winneb. *satch;* Hidatsa *kíCHu*, Mandan *keCHūn*. Dak. *zápta"*=*za* (for *suka-za* 'fingers')+*pta"* 'turned down.'[8] Hidatsa *kíCHu*, from CH*u* 'thrown down' or 'overturned,' with *ki*, the intensive and frequentative prefix, 'wholly, completely,' i. e. 'all turned down.' Or, if we suppose the word to have lost a syllable, and restore it as *sáki*-CHŭ, we have 'hand turned-down'=Dak. *za-pta"*.

Choctaw *tahlapi* 5, seems to be compounded of *tahli* 'to finish' or 'complete' and *ahpi* 'the first'='first hand ends.' In Muskoki *chagh'kih'pĭn*, and Hitchitee *chaghkü'pun*, the Musk. *chunggi* or *chuñki* 'my hand' may perhaps be recognized, but if so, it is nearly lost in the Musk. ordinal, *hlisa cholíkepe* 'fifth.'

Pawnee *si'hūks* is from *iksu-hūks* 'hands half'; still more contracted in Arikara *she'hu* (*íshŭ*='hand').

In the Athapascan, *la* 'hand' seems to be found in Navajo *ást-la*, Apache *ásht-la* 5; but only in these two of the eleven languages of that family compared by Buschmann, who remarks on the general resemblance of the Athapascan 5 to the 1. Eskimo (Labrador) *tedli-ma*, *tellimet* 10, is probably related to *tallek* 'hand.'

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[8] This agrees nearly with the meaning given by the Rev. A. L. Riggs (in his letter of July 27th): "*Zapta"*. Roots ZA and PTAN. *Za* is 'the hand'; thus, *yu-za* is 'to hold,' 'to handle.' *Ptan* is 'turned over.' The whole of the *hand* [i. e. all the *fingers*] is now *turned down.*"

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In the Shoshoni family, Comanche *mowaka* (*mowa* 'arm, hand, fingers,') and in another dialect *mo'ovet* (*moö-oyet* 'hand all'), Shosh. *manáget*, Yute *manigin* (*moo-ninch* 'hand'), all give evidence of their *manual* origin.[1]

6. Names for this numeral in Algonkin, Dakota, and some other families of language, mark it as the first that is counted on the second hand. This is done (1.) by affixing to a name for 'hand' a particle meaning 'one,' 'first,' or 'other,' or (2.) by repeating the name for 1 and affixing a word meaning 'again,' 'besides,' 'beyond,' 'more,' or the like, or (3.) by merely expressing change 'to the other side.' Of these, the second is the most common type: e. g. —

ALG. Cree *nikoto-wasik*, *nikūt-wassik* = '1 on the other side' (Cree *awas* 'further on,' *awasâ-yik* 'on the other side'), Chip. *ningot-wásswi* (*awássaii* 'further'), Abn. *nekūda"s*, Moh. *n'guittus*, Shaw. *nigote-wathwi*, Sauki *kotoashek*; Mass. *nequtta-tahshe*, Del. *quttasch* (the affix, *adtahshe*, means 'counted' or 'added'). — Micmac *ashugōm* (*apch* 'again,' 'following'; *apchku* 'going back'), and Mareschit *kámachin*, seem to be similarly formed. — Illin. *kakatchui* 6 denotes 'passing beyond the middle' (*kakatahe*). — Shyenne *nasutu* (*nahsoto*, Abert) is 'one over.'

The DAKOTA presents two types — which, however, may prove to be originally identical:

| | | | |
|---|---|---|---|
| Dakota | *shá-kpe* | Hidatsa | *aka-wa, aka-ma* |
| Assinib. | *shá-kpa* | Winneb. | *aké-we* |
| Om. and Ponka | *shá-pe* | Aubsaroke | *kí-ma* |
| Iowa | *sha-kwe* | Mandan | *aká-mak* |
| Osage | *sha-pah* | | |
| Oto | *sha-kwa* | | |

Hidatsa *m* and *w* = Dak. *p*. The only question is as to the precise meaning of the Dakota prefix. Dakota *pe* is 'finger' or 'fingers' (hand 'points,' as in *napchu-pe*, etc.), as is more clearly shown by Ponka 7, *pe'namba* (= 2 fingers), and 8, *pe'thabthin* (= 3 fingers). The prefix I take to be Dak. *a-kshá* 'more, in addition to.' Then *shákpe* = *a-kshá-pe* = '1 in addition' or 'besides' (the 5); and Hidatsa

[1] For other 'fives' of Buschmann's Sonora family, including the Shoshoni, see his Grammatik d. Sonor. Sprachen, 3te Abth. ss. 114, 119.

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*akáwa*=*aká-ma* 'one over'; comp. Assinib. *akán* 'above,' *hakê-cha* 'afterwards,' &c.[2]

In the Athabascan family, Buschmann[3] finds 6 expressed by 3×2 in five languages (of eleven compared).

7, 8. The composition of these numerals from 2 and 3 is as common in American as in other families of speech. An independent name for either 7 or 8 is exceptional. The 8 is sometimes designated from its proximity to 10—as 'two less,' 'two left,' or as 'coming near' the end; 7, more rarely, as 'wanting 3,' or the like. The common expression for both numerals is formed by affixing to the names for 2 and 3, respectively, a word denoting *addition* or *repetition*. In some languages, an indication of 'hand' or 'finger' is comprised in the name. The Algonkin 7 has generally the same affix as the 6, meaning 'on the other side' or 'again.' The full expression is preserved in Chip. *nij-wásswi* 7, *nish-wásswi* 8; compare *ningot-wásswi* 6: a contracted form, in Del. *chash* and, with a guttural modification, in Moh. *ghusū*. The Cree and Chippeway languages have each another name for 7: Cree *tépakūp* (*téypuckoop*, Howse), Chip. *tupouwus* (=*tepu-awasswi*), the latter agreeing with the Abnaki 7, *ta"bawa"s;* all formed from a 'two' which is not now found in any Algonkin language except the Micmac (see p. 58, ante). The Crees have also two names for 8: *shwâssik* (=*nishu-awâsik*) and *aienânewu* or *ayenâneū*. The latter is peculiar. It seems to be formed of *iyin* 'more' and *néwu* 4='4 again' or 2×4. An exceptional name for 7 is found in the Narragansett *énada* (Mass. *enotta* of Wood's Vocabulary); perhaps related to Mass. *nahohtoë'u* 'second,' literally 'that which comes next,' or perhaps from the index-finger and act of 'showing' (Mass. *nâtin-au* 'he shows it to,' Chip. *enoad* 'showing with the fingers'). The Sauki 7, *nówia*, may have had a similar origin.

Illinois *parare*, Miami *polâne* 8, mean 'nearly ended,' 'almost done.' The composition of Illin. *suatatchüi*, Mi. *suaχtetsūi* 7, is not clear.

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[2] The Rev. A. L. Riggs has suggested a different derivation of Dak. *shakpe* "from *shaki* 'the nail' and *kpa* or *kpe* 'punched out.' The prominent thumb nail of the second hand is now pushed down."

[3] System. Worttafel d. Athap. Sprachstamms (3te Abth. des Apache), s. 508.

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In the Chahta-Muskoki group we have —

| | | |
|---|---|---|
| Choctaw *tuklo* 2, | and | *un-tuklo* 7. |
| *tuchina* 3, | | *un-tuchina* 8. |
| Coassatti *tókolōō* 2, | | *hon-tókolōō* 7. |
| Alabama *tókoló-chie* 2, | | *hon-tókoló-chie* 7. |

The prefix *un-* or *hon-* (=Choctaw *ont*) means 'again.' In other languages of this family, the names for 2 and 3 are similarly modified by a *suffix:*

| | | |
|---|---|---|
| Musk. *hokkolen* 2, | and | *kólŭ-paken* 7. |
| Hitchiti *tokhlun* 2, | | *kola-paken* 7. |
| Musk. *tutchenen* 3, | | *chenŭ-paken* 8. |
| Hitchiti *tohchítnŭn* 2, | | *tósna-paken* 8. |

One of Mr. Ellis's mistakes is that of regarding these adverbial affixes as representatives of names for 'hand' or 'finger,' or 'five'; and some of the most striking of the coincidences that seem to him "to exhibit the radical affinity which unites the North American languages" vanish with the correction of this error. He finds, for example, his "*az* finger" or his "*baz* finger," or the two combined as "*azbaz* 'finger-finger'=hand," in Delaware *cottash* 6, *nishash* 7, old Algonkin (Nipissing) *ninshwassoo* 7, *nisswassoo* 8, Cree *nikūtwassik* 6, *nishwassik* 7, etc.[4] Whatever the Basque *zaz* (conjecturally extracted from Basque *zazpi* 'seven') or a possible *svas* of "the original Aryan vocabulary" may have denoted, it is certain that in the Del. *-ash*, Alg. *wassoo*, Cree *wassik*, etc., we have merely an adverb meaning 'further,' 'on the other side,' or the like.

In the (semi-Algonkin) Atsina dialect, 7, 8, and 9 are formed respectively from 3, 2, and 1, by a suffix that denotes the 'fingers' remaining to be counted.

In the Dakota family, there are at least two and perhaps three types of 'sevens':

| | | |
|---|---|---|
| Ponka *pé nanba* | Dak. *shakó-wi* | Hidatsa *shápua* |
| Omaha *pé-namba* | Assinib. *shakó-wi* | Aubsar. *kháp*ua |
| Osage *pá-nompâ* | Winneb. *shagó-wi* | Mandan *kúpa* |
| | Iowa *shâhma* | |

The first three prefix to 2, *pé*, *pá* 'fingers' (lit. hand 'points'). Of the others, I find no satisfactory analysis that

[4] Numerals as Signs of Primeval Unity, pp. 7, 8, 9.

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will apply to both groups. [The Rev. A. L. Riggs has suggested, for the Dakota proper, *shake* 'a nail' and *win* 'to bend,' with the preposition *o* 'in' or 'on' interposed, the fore-finger (of the second hand) being *bent upon the nail* of the previously turned thumb.]

The Ponka and Omaha 'eights' are formed like the 'sevens'—by prefixing *pe* to 3; the Hidatsa and Aubsaroke, by suffixing *pe*, *pi*, to 2, the numbers of fingers remaining uncounted:

| | | |
|---|---|---|
| Hidatsa *dópa* 2, | *dópa pi* 8, | (*pítika* 10). |
| Aubsar. *nōp* 2, | *nōpa-pe* 8, | (*píraka* 10). |

Dr. Matthews (Hidatsa Grammar, 56) remarks that *dopapi* probably signifies 'ten less two,' and that *pi* seems to be the root of *pítika* 10. But the primary meaning of *pi*, *pe*, is 'pointed' (or as a verb, 'to penetrate'), and hence 'point,' 'extremity,' 'finger,' as in Hid. *icpu* and *ichpu* = Dak. *chupe* in *nap-chupe* 'fingers,' i. e. 'hand points.' In *icpe* 'the tail of a bird,' Dak. *upi*, we have another modification of this root; and again in Hid. *ipí-ta* 'at the rear, behind,' i. e. 'at the end.'

Iowa *kre-ra-pa-ne* 8, is clearly related (as a diminutive?) to *kre-pa-na* 10. Dakota *sha-hdo'gha*ⁿ and Assinib. *shakando'-ghah* follow the 'sevens,' the first element of the name being the same in each, but I must leave both—with Mandan *tetuk'e*—unexplained.

9, very generally, is named as being the 'last but one'; occasionally, as 'fourth' of the second hand:

Alg. Cree *kéka mitātat* 'almost 10.' √*keka* 'au point de.'
Chip. *sháng-asswi* (and contr. *shang*); comp. *chágisse* 'used up,' 'all spent.'
Shaw. *chakatswi* " " " "
Mass. *paskugun* 'it comes near.'
Del. *pechkunk* 'coming near.'
Illin. *nigutu-manekki* 'only one left,' lit. 'only one, no more.'
Arapoho *thiatokh'* or *siatokh'* 'again last,' 'one after'; from *chía* 'again' and *tákh* (comp. *tákh-su* 'last,' *takhú-ŭ* 'after').

Dak. Omaha, Osage, and Ponka, *shánka*, Iowa *shangke*. Sioux *nap-chiⁿwanka*. Prince Maximilian von Wied notes the Osage as a contracted abbreviation of *grābena-tcheh-winingka* = 10 less 1. This is certainly the *meaning*, but not a *translation* of the name. In the Sioux, *nap* = *nape* 'hand.' In other dialects, *shánka* is Iowa *iyangke* 'one,' 'little one' (and, as diminutive, *chínge*), Mandan *ingka* (as in *ungkni-ingka* 'the little finger'), Omaha *shínga* (redupl. *shínge-shínge* 'an infant,' very small), Sioux *chiⁿchá* 'little one,' and in *chi-ka-daⁿ*

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'very small' (with which compare *wanika-dan* 'very little,' *wa$^{n}$ske* 'the fourth (female) child'). Sioux *chi$^{n}$wanka* in 9, seems to contain an additional element, which may be *eché* 'only' or *echi$^{n}$* 'now.'[5] The meaning is the same, in all these dialects, 'only one finger' remains.

Hidatsa *duetsa-pi* and Aubsaroke *amáta-pi* have the same meaning — 'one finger'; and so has Mandan *mac*H'*pe*, from *mac*H'*ana* 1.

CHAHTA-MUSKOKI. Choctaw *chakali* 9 = *cheki-ŭhli* 'soon the end,' next the last. [The root, *cha, che,* is the nearest approximation to a conjunction copulative, and may be translated 'and then,' or 'next.'] The same component is in Alabama *íbi-cháhkali-chíe* (*chíe* = finger) and Coassatti *bih'chákaälii'*. Musk. *ōs'ta-páhkin* and Hitchiti *ōsta-pákin,* are from Musk. *ōstin,* Hitch. *sitákin,* 'four.'

| | | | |
|---|---|---|---|
| NATCHEZ | *witip'katipis,* 1 left? | from | *wíta* 1. |
| CADDO | *hiwéisika,* 4 + hand, | " | *hiweit* 4, *sé*CHE 'hand.' |
| Adaiz | *sikinish,* 'hands' minus? | " | *sekut* 'hand.' |
| PAWNEE | *d'hūk'sidi-wa,* 10 minus, | " | *d'hūksidi* 10. |
| Arikara | *nu*CH*ini'wun,* " | " | *nu*CH*ini* 10. |
| Wichita | *chíus-skinte,* 1 left? | " | *chíus* 1. |
| Kichai | *tanerókat,* ? | | (*arísko* 1). |
| SHOSHONI | *shimmér-omen,* 10 minus? | " | *shimmer'* 10. |
| Comanche | *shéman'-uwum,* " | " | *shéëman* 10 (Pike, MS.). |
| " | *se'ermano,* " | " | *se'ermano-wūmpnet* 10. |
| Yute | *surrom-suene,* " | " | *tom-suene* 10. |
| " | *suwárroümsoyuni,* " | " | *tóamsuniyuni* 10 (Powell, MS.). |
| YUMA: Cuchan | *hum-hamook',* 3 × 3? | " | *hamook'* 3 (*humhook* 6). |
| Mojave | *pai'a* 'near' | " | (*hipau'ac* 'near'). |
| " | *elyu-thouk* 'near' | " | (*thowk* " ). |

10 The tenth finger — the *little* finger of the second hand — gives in some languages a name to the corresponding numeral; but more often, 'ten' is designated as the 'completion' of the digital series, 'all gone,' 'none remaining,' or the like. Occasionally, the name may have been taken directly from the 'hands' or 'all the fingers.'

In ALGONKIN languages, the 'tens' are of four types — of which two are nearly related:

1. Chip. *midasswi, mitasui,* Illin. *matatchui,* Shawano *metathwi,* Cree *mitatat,* Shyenne *mato*CH*to,* Arapoho *metaito*CH, and Atsina *matatasits* — meaning 'no further,' 'completed.'

[5] I formerly regarded this *chi$^{n}$* as the representative of the verbal root *chi$^{n}$* 'wanting.' To this, the Rev. A. L. Riggs objects, with good reason, that "*chi$^{n}$* is not 'want' in the sense of 'lack,' but always of 'desire';" and that, if it made part of the name, "it should come *last,* as the principal verb." I do not agree with him, however, as to the impossibility of getting 'one' (or rather 'finger,' or '*little* one') out of *wanka.* The other related dialects seem to testify unmistakably to this meaning.



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2. Abnaki *m'tára*, Micmac *m'teln*, Delaware *m'tellen*, *tellen*, Moheg. *m'tannit* = 'no more.'

3. Massachusetts and Narraganset *pai'uk.*

4. Sauki and (Northern) Chippeway *kwetch*, used occasionally in rapid counting. This is either a contraction of *iskwátch* (Cree *iskweyâtch*) 'lastly,' 'at the end' (comp. *ishkwétchagan* 'the last or youngest child in a family'), or it is Nipissing-Algonkin *kagowetch* 'no more.'

The prefix in Illin. *mat-atchui*, Chip. *mid-asswi*, Abn. *m't-ára*, etc., is the negative and privative particle, found in all Algonkin languages, though less common in Chippeway than in eastern dialects. It is found, however, as a prefix, in many Chippeway words (e. g. *nin géssikan* 'I arrive in time,' *nin med-assikan* 'I do not arrive in time,' 'I am too late'; *nind apáb* 'I sit upon' (a seat), *nin mit-ab* 'I sit upon the bare ground, the snow, or the like,' 'have nothing to sit upon'; etc.). As a verbal prefix, it has sometimes, with a modified vowel, the meaning of 'ceasing,' 'leaving off,' 'completing'; e. g. Mass. *mahtu* 'he ceases speaking,' Abn. *met-anaskiwi* 'finally,' Illin. *mita-tewi* 'an abandoned cabin,' *ni metassa* 'I bury (i. e. have done with) him' = Chip. *mid-ágwena* 'I put him aside, or out of the way.'

The suffix *ásswi* is the same as in Chip. *ningot-wásswi* 6, *nishwásswi* 7, meaning 'further' or 'beyond.' At 10, there is 'no further' count, 'a completion.' Abn. *-ara*, Del. *-elen*, Moh. *-anit*, are forms of the same particle of comparison, meaning 'more,' 'above'; and *mid-ásswi* = *m't-ára.*

I have the more particularly pointed out the composition of this Algonkin 'ten,' because more than one writer on American languages has been struck by the likeness of Chip. *midasso* (the ordinal) 10 and *midáss* 'a legging.' Mr. R. Ellis[6] observes this likeness in six or seven Algonkin languages, and infers that "forms like *-doswe*, *-tathi*, *-tato*, *-tato*, etc., may be compared with Uchee (Florida) *tethah* 'shoes,' and *tetethah* 'feet,'" etc., all contributing to show that the "*az* finger" and the "*azbaz* hand" prevail, and are employed numerally, over the greater part of North America

[6] On Numerals as Signs of Primeval Unity, etc., p. 9.

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as well as on the eastern continent, "the *m-* prefix" in *midasso*, etc., "appearing the same as a Californian and New Mexican prefix *m-*, which is used to convert 'arms' into 'legs.'"

The learned author of "Études Philologiques sur quelques Langues Sauvages" (pp. 131, 132) has given an etymology of *mitasui* which is ingenious, but to which there is, I think, one insuperable objection. He derives the name from the particle *mi* 'so,' and *tasui*, *taso*, "a particle that expresses quantity and is the equivalent of [the French] adverbs *tant*, *autant*, *combien*." When an Indian would express 'ten,' he puts forward both hands and spreads the fingers saying, *mi-tasui* 'so many.' The objection to this is, that it will not apply to other Algonkin dialects, nor to other numerals in the same dialect: it will not serve either for Abn. *m'tára* and Cree *mitatat* 10, nor for Chip. *ningotasui* 6, *changasui* 9, etc., in which M. Cuoq finds, not *dasso* 'so many,' but *asui* "en sus, de plus."

In the Massachusetts and Connecticut dialects another name is found for 10, *paiuk* (*piuk*, *piogqué*, Eliot), but the Chippeway *mitasui* is represented in Mass. *muttásons* 'the youngest child in a family' (*mat-ásŭ* 'not after,' with *-ons* diminutive), and in *muttaso-nitch* 'the little finger,' i. e. the least and last. Mass. and Narrag. *paiuk* is, probably, a similar expression, related to *pesuk* (=*pi-es-uk*, dimin. of *pi-ak*) 'least,' 'one *only*,' and to Cree *peyak* 'one,' 'alone,' as well as to *piko* 'only,' 'no more than,' and *piyis* 'finally,' 'lastly.'

The Dakota 'tens' may be reduced to two groups, the name having in both the same general meaning, but not formed from the same roots:

(1.) Sioux-Dak. and Assiniboin *wikchémna, wikchem'ini.*
Ponka *gthe-ba.*
Omaha *chräbene*, and *g'èth'ba*,[7] Iowa *krepana*, Oto *krahbra*[n], Osage *krabra*, Winneb. *kherapun* (or *kherapún-aze*, Hayden).[8]

(2.) Mandan *pirakh*, Aubsar. *piraká*, Hidatsa *pitika.*

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[7] Prince Maximilian's vocabulary gives *chrabëne;* Dr. F. V. Hayden's (in Proc. Am. Philos. Society, x. 407), *g'èth'-ha*, but the second *h* probably is by misprint for *b*, since 20 is *g'th'eba-namba* 'two tens.'

[8] In this group of Dakota 'tens' we have a good illustration of one difficulty in

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At 10, the fingers that have been bent down are *straightened*, and "the hands *spread out* side by side."[9] *Wikchemna* is from *kcha* 'straight,' 'unbent,'[1] and *mna* 'spread out,' with the *generalizing* prefix of Sioux nouns, *wi* or *w'*. Hidatsa *pitika* is from the verb *ptíki* "to smooth out, to iron clothes," which Matthews (Hidatsa Dictionary) refers to *pakíti* (from *kiti*) 'to press to smoothness *with the hands*.'[2] Both expressions "gehn aus von den Fingern," but in neither does a *name* of 'finger' or 'hand' show itself.

How slowly the savage advanced in numeration may be inferred from the traces found in many languages of a mode of reckoning by *pairs* and *triplets*. There are some reasons for believing, not only that conceptions of 'one,' 'two,' and 'three' (as 'this,' 'that,' and 'beyond'—or the like) were antecedent to digital numeration, but that the first definite conception of 'four' was as a 'pair of pairs,' and that *multiplication* of the lower numbers often preceded formal numeration to the higher. *Number* begins at 'two,' and we may assume—without venturing far into the 'metaphysics of language'—that 2 was the *first named* numeral, though an earlier *conception* may be expressed in the name given to 1. Considering that every decimal system is in fact a doubled quinary, and was constructed with as constant reference to

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the way of proving—or disproving—the 'primeval unity' of American speech, on no better evidence than is afforded by brief and often inaccurate vocabularies. In *wikchemna* (discarding the prefixed particle), *gtheba*, and *kherapun*, the same name appears under three dialectical variations: *kche-mna* = *gthe-ba* = *kh'ra-pun*. And the results of 'laziness' and 'emphasis' are so nearly balanced that—tried by the Indo-European standard—it would be hard to say which of the three forms best represents the primitive roots.

[9] The Rev. A. L. Riggs, MS. The derivation he suggests for *wikch'emna* is "from *w*, the sign of the abstract form, *ikche* 'in a common manner,' and *mna* 'gathered together.'"

[1] *ksha* 'bent,' *yu-ksha'* 'to bend, to fold, to double'; *kcha* 'straight,' 'loose' (*un*-bent), *yu-kcha'* 'to untie, to loose,' etc. *yu-kcha*[n] 'to understand, to comprehend' (i.e. to straighten out?).

[2] If the Hidatsa *pitaka* stood alone—the more probable derivation would be from *ipí* 'extremity, end,' as in *ipíta* 'at the rear, behind,' and *ipítakoa* 'at the end'; which last might have been contracted to *pitaka*. But the meaning of the name in other Dakota dialects—'unbent'—favors *ptíki*, notwithstanding the change in accent.

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the number of the hands as of the fingers, numeration by pairs would seem to be a natural expedient for rising to the higher numbers.

In various North American languages of the West and Southwest, we find 'fours' formed from 'twos,' 'eights' from 'fours,' and, more rarely, 'sixes' and even 'nines' from 'threes.' East of the Rocky Mountains, traces of similar numeration are uncommon. The Dakota *tópa* 4=2 pairs, has been mentioned (p. 63). The Catawba (North Carolina) *purre-purra* 4, apparently comes, by reduplication, from *na-perra* 2; but both may have been derived from a common root, found also in *du-punna* 1, *pukte-arra* 5, and *dipk-urra* 6. In the (Algonkin) Cree, one of the two names for 8 is *ayenâneū*, which seems to be a 'double 4' (see p. 66, ante); and in the semi-Algonkin Shyenne, *nōCH* is 1, *enōka* 'a pair'; *niCH* 2, *eniCH-anst* '2 pairs,' *ni-nish-ish'* 'you two'; *na'a*, *nā* 3, *e-na-hanst* 'a pair of threes,' '3 pairs' (Hayden).

In the Athabascan family, Buschmann's comparison of the numerals in twelve languages gives these results: 6 has an independent name in six languages and in six others is formed as 2×3 or 3×2; 8 is expressed as 4×2 in eight languages, and 9 is formed on the 3 in only one.[3]

For example, in the northern Athabascan, Howse's vocabularies[4] give—

| | | |
|---|---|---|
| Chepewyan | 3, *tahhee*, | 6, *elke tahey*. |
| | 4, *dinghee*, | 8, *ellkee dinghe* (also *narky-ah-ahtah* = 2 less). |
| Biber | 2, *onghaty*, 4, *tenter*, | 8, *enchet'hentir* (2×4). |
| | 3, *tâhtir*, | 6, *enchet'hâty*. |

In the southern branch of this family, the same system may be found, though less distinctly marked:

Navajo 3, *t'ha*, 6, *has-tár*, 9, *nas-taĭ*.

In another family, the Shoshoni (classed by Buschmann with the Sonora), doublets and triplets are common:

| | | |
|---|---|---|
| Comanche | 3, *pa-hist*, | 6, *óyoh-pafist*. |
| Chemehuevi | 3, *paĭ*, | 6, *na-baĭ*. |
| | 2, *waĭ'i*, | 4, *wat-chu'*. |

[3] Worttafel d. Athapask. Sprachstamms (3te Abth. des Apache), § 114, n. 2.

[4] Proceedings of the Philological Society (London, 1850), iv. 192 ff.

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| | | | |
|---|---|---|---|
| Shoshoni | 2, *wat,* | 4, *wat-suit.* | |
| Cahuillo | 2, *mewi',* | 4, *mewi'chu.* | |
| Kizh | 2, *huehe,* | 4, *huatsa,* | 8, *huehesh-huatsa.* |

In one Yuma dialect, the Cuchan, we have

3, *hamook',* 6, *humhook',* 9, *hum-hamook;*

though in the Mojave, of the same group, the 6, 7, and 8 are regularly formed as 1, 2, 3 of the second hand.

The numeral system of the Arikaras is peculiar, and deserves special notice. The Arikaras, or 'Rees' as they are called by the French traders, were originally the same people as the Pawnees of the Platte River, their language being nearly the same.[5]

The first five Pawnee and Arikara numerals correspond nearly. From 6 to 10, the Pawnees proceed in the more common mode, by repeating 1, 2, and 3, as 'added' to 5, or 'of the second hand,' and naming 9 as 'less than 10.' The Arikaras named 8 from 6 (by prefixing a particle), and the odd numbers 7 and 9 by a diminutive suffix to the name of the next *higher* even number: thus,

| | | |
|---|---|---|
| 6, *sha'pis* | 8, *tup-sha'pis* | 10, *nukh-iní* |
| | 7, *tup-sha'pis-wan* | 9, *nukh-iní-wan* |

And so with occasional variations, numeration proceeds to 20, which is 'a man'—for the system is vigesimal; 12 is 2+10; 11 is (2+10) *minus;* 13, *nákugit'-wan*, is 'less than' 14, *nákugit'*, which, again, seems to have been formed from 15, *akh'kogit'u* (=*akh'u git'u* 'the whole foot'). In the next quinate the names *all* come from the 20, *wi-tau'* (*wíta* 'a man'), those of 16 and 18 being the less composite and probably the older:

| | | |
|---|---|---|
| 20, *wítau'* | 18, *wítau'-an* | 16, *wítútch'* |
| 19, *wítau'-akhko-káki* | | 17, *wítutch-iskugit.* |

The 19 is literally 'man one-not.' Dr. Hayden's vocabulary gives the numerals as high as 1000, and similar derivation of

[5] Dr. F. V. Hayden's "Contributions to the Ethnology and Philology of Indian Tribes of the Missouri Valley" (Philadelphia, 1862), p. 351. His Arikara vocabulary is the best and largest yet published. For the Pawnee numerals, I use his "Notes on the Pawnee (and other) Languages," in Proc. Am. Philos. Society, vol. x. (1868), pp. 389 ff.; and for the Arikara, have compared Prince Maximilian Wied-Neuwied's vocabulary (Reise, T. II. s. 465 ff.), and that of Geo. Catlin, in "Letters and Notes on the N. A. Indians," ii. 262.

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lower from higher numbers is observable throughout, combined with the common expedients of vigesimal notation:

| | |
|---|---|
| 30, *sawi'u* (*saüï*, Maxim.) | 40, *pit'iku-nanú* = 2 persons |
| 32, *wītau-pitikōkh'ini* = 20 + 12 | 38, *pitikunanu-wan* = 40 − |
| 31, *wītau-pitikunūkh'ini-wan* = (20 + 12) − | 39, *pitikunanu-akhokaki* = 40, 1 not |

100 is '5 men,' 98 is '5 men *minus*,' and 99, '5 men, 1 not'; and so on.

I will not add to the length of this paper by pointing out its shortcomings. It is offered not as a contribution to American linguistics, but with the purpose of showing, by examples taken from a few families of American speech, that it is unsafe to assume uniformity in the conception or the expression of numbers, even in dialects of the same language, much less in languages whose affinity is not yet proved; and that it is equally unsafe to assume that the 'hand' or 'finger' always gives its own name to the number it serves to mark in digital numeration—in other words, that 'two' must = 'hands' or 'fingers,' and 'five' or 'ten' = 'hand'; that although a general correspondence of numeral series in two languages may justify the inference that both came from one stock, yet no evidence of such affinity is presented by occasional coincidences between single numerals in different languages or between the name of any number in one language and that of the 'hand' or 'finger' from which in another that name might have been derived; but that the value of such coincidences must depend on the analysis of the names and the ascertained meaning of their components or roots. I have thought it not impossible that, from a field as yet almost unworked, some of the results obtained in even so partial a survey might interest comparative philologists, as bearing on the question of the origin of ideas of number and the beginnings of the art of counting—antecedent to digital numeration.

The comparison of only a few dialects is sufficient to prove that the process of mental development in the apprehension of numbers has not been uniform. The Algonkin Indian and the Arikara have not taken the same way from the primary conception of number to the full decimal system. It is

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equally evident, that one tribe may have advanced further than another *before* resorting to finger-counting or establishing a regular sequence of earlier-acquired conceptions of number. The priority of the conception of 'one' to that of 'two,' or of 'three' to 'four'—or of the vocal expression of either conception—is not determined by priority in the numeral series. To one tribe, progression by *pairs* may have seemed as natural as progression by *units* does to those of higher culture; and the result would be a system—partially represented by the Arikara—in which the *even* numbers were the earlier named, and the odd numbers intercalated, just as differences by *halves* or other fractional parts might be intercalated in the Indo-European decimal system. The pre-digital numerals so formed might include the 4, the natural order being

2, 1, 4, 3,

that is:

a pair, less, 2 pairs, between (2 and $2 \times 2$).

Or it might stop at the 3, as *trans* 2. No evidence is found that any tribe has advanced beyond 4 without digital numeration, and there are few numeral systems in which some reference to the hand or the fingers may not be detected in the name either of 3 or of 4. But when 3 = 'middle,' 'between,' or 'half-way'—as in the Algonkin languages—it is not possible to decide whether this meaning comes directly from the 'middle finger' (half-way to 5), or from position between 'pair' and 'pair of pairs,' i. e. between 2 and 4.

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## VI. — *On the Distinction between the Subjunctive and Optative Modes in Greek Conditional Sentences.*

By J. B. SEWALL,

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In a discussion at the session of the Association last year upon the use of the subjunctive mode in Greek conditional sentences, it was maintained on the one hand, that the difference between the subjunctive and optative in these sentences was only that of more or less vivid presentation, that is, a difference of degree; on the other, that it was the difference of supposed fact as contingent and supposed fact as merely conceived, that is, a difference in kind. It is the object of this paper briefly to discuss this point.

If we turn to the four classes of particular suppositions in Greek conditional sentences,* and ask how the fact of supposition is presented in each case, the answer, I think, will be somewhat as follows.

In a conditional sentence of the first class, having in the condition *εἰ* with a present or past tense of the indicative, and in the conclusion the indicative without *ἄν*, or a verb of commanding, exhorting, or wishing, there is a simple supposition relating to the actual state of the case, to reality: e. g. Dem. Phil. i. 29, *εἰ δέ τις οἴεται μικρὰν ἀφορμὴν εἶναι σιτηρέσιον τοῖς στρατευομένοις ὑπάρχειν, οὐκ ὀρθῶς ἔγνωκεν*, 'if any one thinks it to be a small start for the soldiers to begin with ration-money, he is wrong.' The condition, *εἰ δέ τις οἴεται*, 'if any one thinks, or is thinking,' is question of what really is, a supposition relating to actual fact. No implication that the fact supposed is or is not actual is involved. Dem. Phil. i. 38, *εἰ δ' ἡ τῶν λόγων χάρις ἔργῳ ζημία γίγνεται, αἰσχρόν ἐστιν, κ.τ.λ.*, 'if agreeableness of speech proves a harm to deed, it is a shame,' etc. Is it the fact? It either is or is not. The supposition relates clearly to actual fact. Xen. Mem. ii. 1. 28, *ἀλλ' εἴτε τοὺς θεοὺς ἵλεως εἶναί σοι βούλει, θεραπευτέον τοὺς θεούς*, 'if

* Goodwin's Greek Modes and Tenses, § 48.



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you wish the gods to be propitious, you must serve the gods.' 'If you wish.' Do you wish or do you not? It is question of actual fact. THUC. ii. 45. 3, *εἰ δέ με δεῖ καὶ γυναικείας τι ἀρετῆς μνησθῆναι, βραχείᾳ παραινέσει ἅπαν σημανῶ*, 'If it is needful at all for me to make mention, etc., I will declare all in a brief exhortation.' DEM. Cor. 52, *εἰ δ' ἀπιστεῖς, ἐρώτησον αὐτούς*, 'if you disbelieve, ask them.'

It would seem, then, that if we were to characterize a condition of the first class from the manner of its presenting the fact in supposition, we might call it a supposition relating to actual fact, generally implying nothing as to its existence in reality one way or the other, though sometimes assuming or taking it for granted.

In the second class, having in the condition *εἰ* with a secondary tense of the indicative, in the conclusion *ἄν* with also a secondary tense of the indicative, we have plainly a supposition implying the contrary to be the fact: e. g. DEM. Phil. i. 1, *εἰ μὲν περὶ καινοῦ τινὸς πράγματος προὐτίθετο λέγειν, ἡσυχίαν ἂν ἦγον*, 'if it were proposed to treat of any new subject, I should keep silence,' implying plainly that it is not proposed to treat of any new subject, and therefore he does not keep silence. Id. ib. 5, *εἰ τοίνυν ὁ Φίλιππος τότε ταύτην ἔσχε τὴν γνώμην, οὐδὲν ἂν ὧν νυνὶ πεποίηκεν ἔπραξεν*, 'if then Philip at that time had entertained this opinion, he would have done none of those things which he now has done,' implying that he did not entertain this opinion at that time, and therefore did do the things he has done. So always; and we may characterize a condition of the second class as a supposition implying the contrary to be the truth, or, for the sake of brevity, a supposition of contrary fact.

Passing for the time being the third class, having in the condition *ἐάν* with the subjunctive, we have for the fourth class a conditional sentence with *εἰ* and the optative in the condition, and the optative with *ἄν* in the conclusion. An example is DEM. Phil. i. 25, *εἰ γὰρ ἔροιτό τις ὑμᾶς, εἰρήνην ἄγετε, ὦ ἄνδρες Ἀθηναῖοι; μὰ Δί' οὐχ ἡμεῖς γε, εἴποιτ' ἄν*, 'for if any one should ask you, Are you at peace, O Athenians? No, by Zeus, we are not, you would say.' 'If any one should ask,

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you would say.' The fact of the supposition here is presented merely as hypothetical, merely as conceived, without reference or implication in any way as regards actual fact. Nor is it future any farther than a supposition of fact not a reality now nor in the past must be in the future if at all. The verbs in the condition and conclusion. *ἔροιτο, εἴποιτε,* are both in the aorist, which means that the Greeks eliminated the facts of the supposition from the element of time and held them in the mind as mere conceptions, never having been, not now being, never to be, in reality, so far as this assertion is concerned. Again, PLAT. Phaed. 67 E, *εἰ φοβοῖντο καὶ ἀγανακτοῖεν, οὐ πολλὴ ἂν ἀλογία εἴη;* 'if they should fear and complain, would it not be very absurd?' Here, again, the fact of supposition is purely hypothetical, placed before the mind as a conception without any reference or implication in relation to reality, likewise not future except as in the previous case. So generally. The optative mode in the conditional sentence is the mode of possibility, that which might be, the mode of fact simply as conceived or existing as a conception in the mind. Accordingly we may characterize a condition of the fourth class as a supposition of conceived fact.

We return now to the conditional sentence of the third class, *ἐάν* with the subjunctive in the condition, and a principal tense of the indicative, commonly the future, or the imperative, in the conclusion. PLAT. Phaed. 69 D, *ἐκεῖσε ἐλθόντες τὸ σαφὲς εἰσόμεθα, ἐὰν θεὸς ἐθέλῃ,* 'when we shall have arrived there, we shall know the truth, if God wills.' What is the force of the subjunctive *ἐθέλῃ* here? It expresses an action continuous, uncertain, and future: 'if God be willing' at that time. The continuousness arises from the tense, which is present; the futurity partly from the tense of the principal clause expressing the fact, a future one, of which this is the condition, and partly from the mode, which, it seems to me, we may describe as the mode of uncertainty or contingency, that is, the mode by which the Greeks chose to represent an action as uncertain or contingent whether in reality it was so or not. The principal verb, *εἰσόμεθα,* expressly declares a fact, 'we shall know,' but it is contingent, and the

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mode used to express that contingency is the subjunctive. What would be the force if the sentence were a conditional of the fourth class, that is, with the optative in both condition and conclusion, thus: *εἰδεῖμεν ἄν, εἰ θεὸς ἐθέλοι?* It seems plain that the assumed fact expressed by the words *ἐλθόντες ἐκεῖσε*, 'when we shall have arrived there' (into the future state), would be thrown back from an assumed fact into a simply conceived fact of condition, 'if we should arrive there,' and then the conclusion also would be thrown back into a simply conceived fact, 'we should know,' and the present condition, as expressing only uncertainty, would become a second condition, likewise of simply conceived fact, 'if God should will.' That is, the sentence in the first form positively declares a fact with a condition of mere contingency; in the second, it presents the fact merely as a conception and its conditions also as conceived facts. The difference therefore is not one of degree, more or less vividness, but of *kind*, mere uncertainty or contingency on the one hand, and pure conception on the other. DEM. Phil. i. 29, *τοῦτ' ἂν γένηται, προσποριεῖ τὰ λοιπὰ αὐτὸ τὸ στράτευμα ἀπὸ τοῦ πολέμου*, 'should this be done, the army itself will provide the remainder from the war.' What, again, is the force of the subjunctive here? *τοῦτ' ἂν γένηται*, 'should this be done,' 'if this shall have been done.' Is it not plainly question of fact which is uncertain, contingent, and not presented to the mind as a mere conception? If Demosthenes had said *εἰ τοῦτο γένοιτο, προσπορίζοι ἂν τὰ λοιπά, κ.τ.λ.*, would he not have meant 'if this should take place, the army itself would provide the remainder,' etc., presenting the fact merely as a conception in the mind? And is there not plainly here a distinction in kind — fact in the first place as contingent, in the second as purely hypothetical — and not of degree, as more or less vivid? THUC. ii. 39. 4, *ἢν δέ που μορίῳ τινὶ προσμίξωσι, κρατήσαντές τε τινὰς ἡμῶν πάντας αὐχοῦσιν ἀπεῶσθαι*, 'if ever they have had an engagement with any small portion of our army, having conquered some, they boast that we all have been driven.' *Ἢν προσμίξωσι*, 'if they may have engaged.' What is the force of this subjunctive? It is an aorist — an action

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'brought to pass.' It is in the past, not future. There is nothing future about it. It is not the positive declaration of an act as a positive fact. The speaker, rather, most evidently wished to present the case as an *uncertainty*. It may have taken place and it may not, so far as he asserts. He wishes not to say that it has, but to grant that it may have, and to leave his hearer to believe rather that it has. It is the assertion in supposition of uncertain fact.

The last example is a general supposition,* and perhaps better illustrates the nature of the subjunctive mode than the third class particular. We will take another. EURIP. Alc. 671, *ἢν δ' ἐγγὺς ἔλθῃ θάνατος, οὐδεὶς βούλεται θνήσκειν*, 'if ever death comes near, no one wishes to die.' Here also is an aorist subjunctive in the condition, *ἔλθῃ* — aorist to signify that the fact is viewed as one 'brought to pass,' done and complete in itself whatever the time be, past, present, or future, and subjunctive to represent the fact as an uncertain one, 'if death may have come near.' This is a general supposition of the first class, denoting a general fact now true, and it is easy to see why the Greeks should have used the mode of uncertainty — the subjunctive — in the hypothesis, since the fact may or may not be at any given time, while they would use the mode of pure hypothesis, of mere conception — the optative — in a general supposition of the second class, where the case supposed is in past time. Compare, for example,

*ἐάν τις τούτου πίῃ, ἀποθνήσκει*, and
*εἴ τις τούτου πίοι, ἀπέθνησκεν.*

The first makes hypothesis of a fact which *may take place now;* the second, of a fact which possibly took place, which may be conceived of only as taking place, in past time.

The same may be seen equally well in conditional relative sentences. Compare

*ὅ τι ἂν βούληται, δώσω, I will give him whatever he may wish,* and
*ὅ τι βούλοιτο, δοίην ἄν, I should give him whatever he might wish.*

My conclusion then is, that the subjunctive in conditional sentences differs from the optative in that it is a form of the verb to represent the fact as uncertain, or, in general,

* Goodwin, Greek Moods and Tenses, § 51.

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contingent, while the optative is a form to represent it as merely conceived; and the difference between them is one not of degree, but of kind. And in accordance with what has been said, the four classes of conditional sentences particular may properly be described, viewing them with reference to the manner in which the fact of supposition is presented, the first, εἰ with the indicative, usually a primary tense, as a supposition relating to actual fact; the second, εἰ with the indicative, secondary tense, as a supposition relating to contrary fact, or implying that the contrary is the truth; the third, ἐάν with the subjunctive, as a supposition relating to contingent fact; and the fourth, εἰ with the optative, as a supposition of conceived fact.

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## VII. — *On the Age of Xenophon at the Time of the Anabasis.*

By CHARLES D. MORRIS,
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THE biographies of Xenophon represent that he was born in B. C. 444 or 443, and that he was in consequence forty-three or forty-two years old at the time he joined the expedition of Cyrus. So far as I have been able to examine the current authorities, I do not find any who do not give their adhesion to this view. The life of Xenophon prefixed to the edition of Prof. Anthon, which is mainly taken from that in the Penny Cyclopedia, that in the edition of Prof. Boise, that prefixed to Kühner's edition, and that in Smith's Dictionary of Biography, are unanimous on this point. These all acknowledge their obligations to a tract of C. G. Krüger, published at Halle in 1822, entitled "De Xenophontis Vita Quaestiones Criticae," which I have unfortunately been unable to procure or even to get a sight of, and I can in consequence deal with it only at second hand.* Sir G. C.

* Since this paper was read, Mr. A. Van Name, Librarian of Yale College, kindly sent me a volume containing a number of Krüger's philological papers, and among them this discussion of Xenophon's age. I find that the "lives" above referred to have extracted all that is of weight in it, and the perusal of it has not led me to alter my own opinion in any degree.

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Lewis, also, in a note on a learned article in the Classical Museum (vol. ii. p. 17), says incidentally: "Xenophon was about forty-two years old in B. C. 401, and consequently was born about 443." Clinton, indeed, in his Fasti Hellenici (sub ann. 401), though he holds the same opinion, refers to a note in Mitford's History of Greece, in which the latter, rejecting the main authority for the current belief which I will quote presently, attempts to establish that Xenophon was not more than thirty years old at the time of the Anabasis by two arguments which Clinton quotes and of which the one cannot be verified and the other is not true. This is unfortunate for me, as I purpose to maintain the view which Mitford adopted and to press it even further than he did; and it is with reluctance that I am thus compelled to discredit by anticipation my own position. Moreover, Prof. Boise, still, I presume, following Krüger, refers to several of the points, which I shall adduce, only to reject their force. But I think that the present case is one in which the whole power of an argument is lost if it be merely alluded to and not stated in full, and therefore I hope you will allow me to consider as an open question one supposed to have been long ago settled, and to lay before you the evidence on both sides.

My own early impression of Xenophon's age at the time of the Anabasis was derived from a passage in Bacon's "Advancement of Learning," which I shall venture to read.

"And here it were fit to leave this point touching the concurrence of military virtue and learning; for what example would come with any grace after those two of Alexander and Caesar? were it not in regard of the rareness of circumstance that I find in one other particular, as that which did so suddenly pass from extreme scorn to extreme wonder; and it is of Xenophon the philosopher, who went from Socrates' school into Asia, in the expedition of Cyrus the younger against King Artaxerxes. This Xenophon at that time was very young, and never had seen the wars before; neither had any command in the army, but only followed the war as a voluntary, for the love and conversation of Proxenus his friend. He was present when Falinus came in message from

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the great king to the Grecians, after that Cyrus was slain in the field, and they a handful of men left to themselves in the midst of the king's territories, cut off from their country by many navigable rivers and many hundred miles. The message imported that they should deliver up their arms and submit themselves to the king's mercy. To which message before answer was made, divers of the army conferred familiarly with Falinus; and amongst the rest Xenophon happened to say, 'Why, Falinus, we have now but these two things left, our arms and our virtue! and if we yield up our arms, how shall we make use of our virtue?' Whereto Falinus, smiling on him, said, 'If I be not deceived, young gentleman, you are an Athenian, and I believe you study philosophy, and it is pretty that you say; but you are much abused, if you think your virtue can withstand the king's power.' Here was the scorn; the wonder followed; which was that this young scholar, or philosopher, after all the captains were murdered in parley by treason, conducted those ten thousand foot, through the heart of all the king's high countries, from Babylon to Graecia in safety, in despite of all the king's forces, to the astonishment of the world, and the encouragement of the Grecians in time succeeding to make invasion upon the kings of Persia, as was afterwards purposed by Jason the Thessalian, attempted by Agesilaus the Spartan, and achieved by Alexander the Macedonian, all upon the ground of the act of that young scholar."

It was, I confess, with a certain amount of dismay that, when it became my duty to teach boys their Xenophon, I found that this spirited sketch of Bacon's must have its most characteristic touches blotted out; that probably it was not Xenophon at all who was the object of Phalinus's scorn; and that, if it was Xenophon, he was no youthful inexperienced scholar, but a middle-aged veteran. In the passage of the Anabasis referred to (ii. 1. 12), the best MSS. read Θεόπομπος. Krüger indeed maintains that Ξενόφων is the true reading, and thinks that the name Θεόπομπος crept into the text from a marginal note of a scholiast, which may perhaps have been Θεόπομπος δὲ Πρόξενον τοῦτο εἰπεῖν φησί, as in

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fact the *mot* is attributed to Proxenus by Diodorus. I myself am glad to agree with Krüger in his conclusion on this point, as it is probable that the historian Theopompus in his σύνταξις Ἑλληνικῶν did treat at length of the expedition of Cyrus, and there is no other indication in the Anabasis that an Athenian of that name was present in the army. I do not, however, consider that the point I wish to establish needs any such repudiation of MS. authority; and I shall therefore leave Theopompus in the enjoyment of such credit as this single incident can give him.

The only argument adduced in support of the assumption that Xenophon was born about B. C. 444 is the fact that Strabo and Diogenes Laertius report that Xenophon was present at the battle of Delium, which occurred in the latter part of B. C. 424, and was saved in the subsequent flight by the intervention of Socrates. Strabo's story is as follows.

In his description of Boeotia, he comes to the south-easterly corner, and says: *εἶτα Δήλιον τὸ ἱερὸν τοῦ Ἀπόλλωνος ἐκ Δήλου ἀφιδρυμένον, Ταναγραίων πολίχνιον, Αὐλίδος διέχον σταδίοις τριάκοντα, ὅπου μάχῃ ληφθέντες Ἀθηναῖοι ἔφυγον· ἐν δὲ τῇ φυγῇ πεσόντα ἀφ' ἵππου Ξενοφῶντα τὸν Γρύλλου ἰδὼν κείμενον Σωκράτης ὁ φιλόσοφος στρατεύων πεζός, τοῦ ἵππου γεγονότος ἐκποδών, ἀνέλαβε τοῖς ὤμοις αὐτοῦ καὶ ἔσωσεν ἐπὶ πολλοὺς σταδίους, ἕως ἐπαύσατο ἡ φυγή.* (Strabo, Book ix., p. 403, Ed. Cas.) Diogenes Laertius, in his life of Socrates (ii. 22), speaks as follows: *ἐπεμελεῖτο δὲ καὶ σωμασκίας καὶ ἦν εὐέκτης. Ἐστρατεύσατο γοῦν εἰς Ἀμφίπολιν* καὶ Ξενοφῶντα ἀφ' ἵππου πεσόντα ἐν τῇ κατὰ Δήλιον μάχῃ διέσωσεν ὑπολαβών· ὅτε καί, πάντων φευγόντων Ἀθηναίων, αὐτὸς ἠρέμα ἀνεχώρει, παρεπιστρεφόμενος ἡσυχῇ καὶ τηρῶν ἀμύνασθαι εἴ τίς οἱ ἐπέλθοι.*

Now, if this story is true, it is assumed that Xenophon must have been at least in his twentieth year; as youths between the ages of eighteen and twenty were formed into a kind of horse-patrol, under the name of περίπολοι, to guard the

* Diogenes refers here in all probability to the expedition of B. C. 432–430 against Potidaea, in the winter blockade of which place Socrates served with conspicuous hardihood. (Plat. Symp. p. 220 a.) He is said to have been also on the expedition which Cleon led in B. C. 422 to attempt the recovery of Amphipolis; but, though he no doubt did his duty there as elsewhere, I can find no mention of any unusual gallantry or endurance displayed by him in that service. This may, therefore, be a further instance of the inaccuracy of Diogenes.



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frontier, but were not required to serve beyond it; and, though the so-called battle of Delium was actually fought within the limits of the Athenian territory in the vicinity of Oropus, it is taken for granted that because Delium, the objective point of the expedition, was over the boundary, Xenophon could not have been permitted to take part in it unless he had passed beyond the age of the περίπολοι. But, when you read the narrative of Thucydides, and remember that Delium was only about a mile from the border territory of Oropus, and Oropus itself only a day's march from Athens, you feel that Grote has good ground for saying, as he does, that "it is probable that men of all ages, arms, and dispositions crowded to join the march, in part from mere curiosity and excitement." Assuming, therefore, for the moment the truth of the story in Diogenes, Xenophon may well have been from five to ten years younger than it is asserted that he was at the time of the battle of Delium; and this reasoning of mine should find favor with those who wish to accept the literal truth of Strabo's statement that Socrates took him on his shoulders, and carried him safely for several stades.

But this conjecture is in my judgment by no means sufficient to harmonize the story with the passages I shall presently quote from the Anabasis; and I am forced, therefore, to discredit it altogether. No doubt both Strabo and Diogenes found the fact asserted in the authorities they consulted. But you must remember that Strabo was a contemporary of Augustus, and lived certainly some years into the reign of Tiberius; while Diogenes probably flourished at the close of the second century after Christ, and is by some placed as low as the time of Constantine. Strabo was no doubt accurate and painstaking in the verification of his statements as to matters of geography; but such stories as the one in question were probably introduced into his account by way of enlivening it and without any special examination into their truth or falsehood. It was enough for him that such a story was current in reference to the locality to warrant him in inserting it. Diogenes, however, was eminently uncritical. The writer of his life in Smith's Dictionary (Adolf Stahr) says of him:

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"His work is in reality nothing but a compilation of the most heterogeneous and often directly contradictory accounts, put together without plan, criticism, or connection." "His object evidently was to furnish a book which was to amuse its readers by piquant anecdotes." "The traces of carelessness and mistakes are very numerous; much in the work is confused, and there is also much which is quite absurd." "In order to rescue the common sense of the writer, critics have had recourse to the hypothesis that the present work is a mutilated abridgment of the original production of Diogenes." I maintain, therefore, that an anecdote which we find introduced incidentally into the work of a geographer who lived four hundred years after the time of the alleged occurrence, and into the work of an uncritical biographer of philosophers who lived at least six hundred years after it, is not to be accepted as true, if there is any considerable weight of probability against it, and much less so if the acceptance of it renders several statements in the writings of the subject of the anecdote preposterous and absurd.

First, then, as to the antecedent improbability of Xenophon's having been present, under the circumstances supposed, at the battle of Delium.

1. The story is not perhaps irreconcilable, but it is certainly not in obvious accord, with the fact stated in Plutarch's life of Alcibiades, that "in the battle of Delium, when the Athenians were routed, and Socrates with a few others were retreating on foot, Alcibiades, who was on horseback, observing it, would not pass on, but stayed to shelter him from the danger, and brought him safe off, though the enemy pressed hard upon them and cut many off." The natural inference from this would be that Socrates had quite enough to do to save himself, and was not in a condition to take on his shoulders a young man of twenty, and walk off with him for several stades.*

* It is notable that Krüger is inclined to question the accuracy of this narrative of Plutarch, on the ground that Alcibiades, in *Plato's* "Banquet" (pp. 220, 221), when he is represented as pronouncing his panegyric on Socrates, does not claim to have contributed anything to his safety. But Krüger has not a word to say about the singular fact that Xenophon, in *his own* Memorabilia, makes no allusion

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2. On the assumption that Xenophon was of military age at the time of the battle of Delium, where was he and what was he doing during the remaining twenty years of the war? It is hard to believe that a man of such remarkable resource and practical efficiency should have remained unemployed during all the exciting scenes of the Sicilian expedition and on the coast of Asia Minor; and it is nearly as incredible that, if he had been engaged in those affairs, he would have told us nothing about them himself (for reticence about his own achievements is certainly not to be attributed to him), or that we should have had no notices of his adventures from other sources.

3. It is highly improbable that, if he had been indebted for the saving of his life to Socrates, we should have had no intimation of so striking a fact in any of his numerous writings, particularly when one of them is expressly devoted to the vindication of the character of Socrates as in all respects a good citizen.

4. Lucian (in his Μακρόβιοι, §21) states that Xenophon lived beyond his ninetieth year. This may very well be the case. But it is hard to believe that he could have maintained to the verge of that age so much literary activity as he was exhibiting at or after the date of the battle of Mantinea, B. C. 362. The narrative of the Hellenica is continued to that date, when he must have been, according to the common view, eighty-two years old. But this is not all; for in Hell. vi. 4. 35, the assassination of Alexander of Pherae is mentioned, which Clinton and Grote place in B. C. 359, when Xenophon would have been eighty-five years old, and Diodorus places three years later; and at the end of the chapter an expression is used which would suggest that a considerable interval elapsed between the murder and the writing of the narrative: *τῶν δὲ ταῦτα πραξάντων ἄχρις οὗ ὅδε ὁ λόγος ἐγράφετο Τισίφονος πρεσβύτατος ὢν τῶν ἀδελφῶν τὴν ἀρχὴν εἶχε.* Moreover, the last chapter of the Cyropaedia, called the Epilogus, which,

to his own supposed debt to Socrates. Krüger does not observe, moreover, that in the "Banquet," though Alcibiades describes Socrates's dauntless bearing in the face of the foe, he does not mention his having Xenophon on his back, which would have surely enhanced greatly the noteworthiness of the scene.

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though its genuineness has been questioned, is now, I believe, generally recognized as a fitting conclusion to the book, speaks of events connected with the revolt of certain satraps from Artaxerxes Mnemon, which occurred B. C. 361; and Xenophon undoubtedly wrote his Hipparchicus and his Poroi after the repeal of the decree for his banishment, which Krüger places in the same year as the battle of Mantinea, B. C. 362. He must, therefore, have been engaged on at least four of his works after he was, as is generally assumed, eighty-two years old; I say 'at least,' for the passage at the beginning of the third book of the Hellenica, in which Xenophon speaks of an account of the expedition and retreat of the Greeks having been written by a certain Themistogenes of Syracuse, induces Schneider to conclude decisively that the Anabasis was written after the Hellenica. This, however, is very doubtful on other grounds.

5. Photius states that Xenophon was a pupil of Isocrates, who was born B. C. 436. This may be true, as George Long (in Smith's Dict.) says; but, if it is true, it is at least exceedingly improbable that Xenophon should have been born eight years before his future teacher.

I now pass to the passages in the Anabasis which bear upon the question, and which seem to me to prove that if Xenophon was at the battle of Delium at all, he must have been so in the same sense as that in which in the Scripture Levi is said to have paid tithes to Melchisedec.*

1. In the last chapter of Book ii. we are told that of the five generals who were assassinated, Proxenus was about thirty years old (*ἐτῶν ὡς τριάκοντα*), Clearchus about fifty (*ἀμφὶ τὰ πεντήκοντα ἔτη*), while Agias and Socrates were about thirty-five (*ἥστην ἄμφω ἀμφὶ τὰ πέντε καὶ τριάκοντα ἔτη ἀπὸ γενεᾶς*). Menon's age is not mentioned; but it may be inferred from what is stated that he was considerably younger than any of the others. Now Xenophon joined the expedition through the influence of Proxenus, who was, according to the view I am criticizing, his junior by thirteen years. It is antecedently improbable that influence such as this would have been exerted

* Heb. vii. 9, 10.

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by a young man upon one so much his senior. But omitting this point, with which circumstances may have had something to do, it is at least certain that Xenophon must have been perfectly familiar with the phenomenon of men of thirty or thirty-five years of age discharging the functions of generals, and he could not, therefore, if he were older than this, have appeared to himself too young to exercise such functions. It is of course true that, in case of an election of their commander by the soldiers, a man of mature years and an experienced veteran would, other things being equal, be preferred to a mere youth of no recognized preëminence. But, in the absence of any *lex annalis* to control them, soldiers in such a strait as the Greeks were in would be likely to yield submission to the man, whatever were his age, who seemed to possess in the highest degree the qualities needed for their deliverance; and certainly if they were, as was the case with Proxenus's officers, accustomed to obey a man of thirty, they would not be likely to look upon a man of forty-three as too young for the position. But what does Xenophon say (iii. 1. 14) when he tells us his meditations after awaking from his dream? "No one," he says, "is taking any thought about our dangers. Why do I wait for the general of some other city to undertake these things? and what age do I expect to come to myself? for I shall not be any older if I give myself up to the enemy to-day"; or, as Grote puts it, "Why do I wait for any man older than myself or for any man of a different city to begin (*ποίαν δ' ἡλικίαν ἐμαυτῷ ἐλθεῖν ἀναμένω; οὐ γὰρ ἔγωγ' ἔτι πρεσβύτερος ἔσομαι, ἐὰν τήμερον προδῶ ἐμαυτὸν τοῖς πολεμίοις*)? Here the word *ἡλικίαν*, which by itself implies simply 'time of life,' is confined to the notion of 'youth' by the subsequent *πρεσβύτερος*.

2. When Xenophon has roused the captains of Proxenus, and has expressed to them his anxiety and his views as to the proper course to be pursued, he says (iii. 1. 24; I use again Grote's adaptation): "Let us not wait for any one else to come as monitor to us; let us take the lead, and communicate the stimulus of honor to others. Do you show yourselves now the best of the lochages, more worthy of being generals

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than the generals themselves. Begin at once, and I desire only to follow you. But if you order me into the front rank, I shall obey without pleading my youth as an excuse, accounting myself of complete maturity, when the purpose is to save myself from ruin" (iii. 1. 25, *κἀγὼ δέ, εἰ μὲν ὑμεῖς ἐθέλετε ἐξορμᾶν ἐπὶ ταῦτα, ἕπεσθαι ὑμῖν βούλομαι· εἰ δ' ὑμεῖς τάττετέ με ἡγεῖσθαι, οὐδὲν προφασίζομαι τὴν ἡλικίαν, ἀλλὰ καὶ ἀκμάζειν ἡγοῦμαι ἐρύκειν ἀπ' ἐμαυτοῦ τὰ κακά*). How absurd would it have appeared to Proxenus's captains, accustomed as they were to obey the commands of a man of thirty, to hear a man of forty-three suggesting that perhaps he might appear to them too young to act as their leader!

3. After suggesting the best formation for the army on its march, Xenophon proposes that the officers who are to command on each quarter shall be at once selected, and says: "Let Cheirisophus lead the van, since he is a Lacedaemonian; and let two of the oldest generals have charge of the two wings; and let me and Timasion, who are the younger, guard the rear" (iii. 2. 37, *ὀπισθοφυλακῶμεν δὲ ἡμεῖς οἱ νεώτεροι, ἐγώ τε καὶ Τιμασίων, τὸ νῦν εἶναι*). We are not told the ages of the generals who were chosen to fill the places of those who had been murdered; but it is exceedingly improbable that they were all over forty;* and yet they must have been considerably so, if Xenophon, being forty-three, could speak of himself as younger than they.

4. When it is necessary to make a supreme effort to gain a certain height, in order to dislodge the enemy from their threatening position, Xenophon, discussing the matter with Cheirisophus, says that he will either take command of the force which is to scale the height or stay with the army in the plain, and Cheirisophus, not to be outdone in generosity, replies: "Well, I allow you to choose which you please"; and then we are told that "Xenophon, saying that he is the younger, elects to go" (iii. 4. 42, *εἰπὼν ὁ Ξενοφῶν ὅτι νεώτερός ἐστιν, αἱρεῖται πορεύεσθαι*). Here again there is the same assertion

* It is proper to say that Krüger assumes the truth of this improbability, and, if I understand him, believes it likely that the rank and file of the army also consisted of men over forty. I do not think that many who consider the circumstances under which the Cyreian force was collected will agree with him.

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of comparative youthfulness, which would have seemed impertinent and ridiculous in a man of middle age.

5. When the Greeks had reached Trapezus, and, after vainly waiting for a number of vessels sufficient to transport the whole army, decided that they must make a move, they placed on board the ships which they had secured the feeble and *those above forty years* with the children and women, and they placed Philesius and Sophaenetus, the eldest of the generals, in charge of them (v. 3. 1, *καὶ εἰς μὲν τὰ πλοῖα τούς τε ἀσθενοῦντας ἐνεβίβασαν καὶ τοὺς ὑπὲρ τετταράκοντα ἔτη καὶ παῖδας καὶ γυναῖκας—καὶ Φιλήσιον καὶ Σοφαίνετον, τοὺς πρεσβυτάτους τῶν στρατηγῶν, εἰσβιβάσαντες τούτων ἐκέλευον ἐπιμελεῖσθαι*). Here it is to be observed that it is manifest that only a small number of men in the army were as old as forty years; for they had only a small number of vessels, and the subsequent fighting strength of the enemy was not seriously lessened; and that two of the other generals are distinctly spoken of as older than their brother officers.

6. On a certain occasion, Neon, who was in command of the division of Cheirisophus, when the army was in great straits for provisions, led out two thousand volunteers for an attack on some Bithynian villages, though the sacrifices persisted in presenting unfavorable indications. Subsequently his force was surprised by the horsemen of Pharnabazus, and five hundred of them were cut off, and the rest took refuge on a mountain. On hearing this, Xenophon, first sacrificing one of the baggage oxen, hurried to their aid, and with him all the others up to thirty years (vi. 4. 25, *ἐβοήθει καὶ οἱ ἄλλοι οἱ μέχρι τριάκοντα ἐτῶν ἅπαντες*). One would naturally infer from this that Xenophon did not appear conspicuously unequal to his comrades, as he would have done if he had been nearly forty-five years old. For in the following chapter (vi. 5. 4) we are told that after this reverse the generals organized an expedition for forage, leaving the slaves and mixed multitude (*τὸν ὄχλον καὶ τὰ ἀνδράποδα*) in a strongly fortified camp with Neon to guard it (*οἱ μὲν δὴ ἄλλοι πάντες ἐξῆεσαν, Νέων δὲ οὔ· ἐδόκει γὰρ κάλλιστον εἶναι τοῦτον φύλακα καταλιπεῖν τῶν ἐπὶ τὸ στρατόπεδον*).

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And then it is said* that when Neon's captains and soldiers, feeling ashamed to stay behind when the rest had gone forth, left those who were in the camp with Neon, then all went forth on the expedition, and only those were left behind who were more than forty-five years old (*κατέλιπον αὐτοῦ τοὺς ὑπὲρ πέντε καὶ τετταράκοντα ἔτη*), from which we may gather that an age such as is commonly attributed to Xenophon was generally regarded as affording some justification for a certain remissness and willingness to be behind a rampart.

7. After the Greeks had taken service with Seuthes, when it was necessary to make a very rapid attack, Xenophon dismounted from his horse, and on being asked why he did so, replied that the hoplites would run faster and more cheerfully if he led them on foot; and then it is said that Xenophon ordered the men who were not over thirty to join him from the companies, and that he himself ran fast with these, while Cleanor (who is mentioned in ii. 1. 10 as *πρεσβύτατος ὤν*) led the rest of the Greeks (vii. 3. 46, *Ξενοφῶν δὲ παρηγγύησε τοὺς εἰς τριάκοντα ἔτη παριέναι ἀπὸ τῶν λόχων εὐζώνους. καὶ αὐτὸς μὲν ἐτρόχαζε τούτους ἔχων· Κλεάνωρ δ' ἡγεῖτο τῶν ἄλλων Ἑλλήνων*). Here again the natural inference is that Xenophon was certainly not inferior in bodily activity to those who were under thirty, and that therefore he was himself probably under thirty.

There are several other passages which I might quote, in which, though he says nothing by which his own age is directly implied, he calls attention to the fact that others are of more or less advanced ages (*πρεσβύτεροι* or *πρεσβύτατοι*). Now I think that this is of itself an indication of youthfulness on the part of the person who so speaks or writes. For as it cannot be supposed that there were any in the army who were what we should call really old men, it would be an impertinence for a person who was himself forty-three years old to talk about his comrades so readily as Xenophon does as being "older" or "oldest."

There is only one passage in the Anabasis which is supposed

* This passage is a little obscure. I give substantially the interpretation of Kühner and Freund.



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to imply that Xenophon was a man of mature years. It is when Seuthes, among other fair promises by which he hoped to induce Xenophon to engage the Greek army to take service with him, says: "And to you, Xenophon, I will give a daughter of mine; and if you have a daughter, I will buy her after the Thracian manner" (vii. 2. 38, *σοὶ δὲ, ὦ Ξενοφῶν, καὶ θυγατέρα δώσω, καί, εἴ τις σοὶ ἔστι θυγάτηρ, ὠνήσομαι Θρᾳκίῳ νόμῳ*). This, it is said, implies that Xenophon must have seemed to Seuthes old enough to have a marriageable daughter. But no such inference is necessary. For anything we know to the contrary, Xenophon, though as young as I think he was, may have been bronzed by the hardship and exposure he had encountered during the previous months so as to look as old as Krüger makes him; or Seuthes may have thought that perhaps he had an infant daughter whom he might purchase, in eastern fashion, for his harem; or, more probable than either of these suppositions, it may have been an offer made as recklessly and with as little thought or care for the possibility of its fulfillment as any other of the engagements which Seuthes entered into at the same time. There is, moreover, a passage subsequently (vii. 6. 34) in which Xenophon implies distinctly that he had no children at that time.

My own strong impression is that Xenophon was under twenty-five at the time of the Anabasis, though, of course, I do not pretend to have established anything so precise as this; and that, therefore, whether it was Xenophon or a certain Theopompus at whom the repartee of Phalinus was aimed, we may still allow the account of scorn and wonder to stand as Bacon puts it. If we believe that Xenophon was a mere youth, the remarks which Grote makes on the superiority of Athenian training as compared with that of other parts of Greece will be felt to be more strikingly appropriate; and I quote a few lines of them in order to call due attention to the personal qualities which Xenophon possessed, and which secured the admission of his superiority notwithstanding his apparent youthfulness. Grote says, in his account of the ready way in which Xenophon's suggestions were adopted: "Cheirisophus had not only been before in office as one of

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the generals, but he was also a native of Sparta, whose supremacy and name were at that moment all-powerful. Kleanor had been before, not indeed a general, but a lochage, or one in the second rank of officers. He was an elderly man, and he was an Arcadian, while more than the numerical half of the army consisted of Arcadians and Achaeans. Either of these two, therefore, and various others besides, enjoyed a sort of prerogative or established starting-point for taking the initiative in reference to the dispirited army. But Xenophon was comparatively a young man" — I should say, a very young man — "with little military experience. He was not an officer at all. He had nothing to start with except his personal qualities and previous training." "In him are exemplified those peculiarities of Athens — spontaneous and forward impulse as well in conception as in execution, confidence under circumstances which made others despair, persuasive discourse and publicity of discussion made subservient to practical business, so as at once to appeal to the intelligence, and stimulate the active zeal, of the multitude." "The Athenian Xenophon was among the few who could think, speak, and act with equal efficiency." "It was this tripartite accomplishment, the exclusive possession of which, in spite of constant jealousy on the part of the Boeotian officers and comrades of Proxenus, elevated Xenophon into the most ascendent position in the Cyreian army."

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## VIII. — *Φύσει or Θέσει — Natural or Conventional?*

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THE Greeks, it is well known, disputed of old with one another whether the names of things existed *φύσει*, 'by nature,' or *θέσει*, 'by attribution' — that is, as we should say, 'by convention.' Into the history of this dispute, into the question as to what philosophers took ground on the one side and on the other, with what arguments they supported their views, and how near they came to a final agreement, there

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is no need that we enter. Their basis of argument was so much more restricted than ours that their discussions would have for us only a historical interest; and the inquiry itself is still a living one. Notwithstanding all the progress that linguistic science has made in this century, general opinion — nay, even the opinion of linguistic scholars, of writers upon language — is still so far at variance that both answers are given. This may be, at least in part, not so much from a real essential difference of view, as from a different understanding of the meaning of the terms used. But, whichever it be, the discordance is not to the credit of the new science of language: if that science has not been able yet to settle so fundamental a question, between views as different as white and black, it cannot claim to have accomplished much; it is still in its infancy.

It may be sufficient to quote, as the starting-point of our own inquiry, the expressed opinion of one well-known and highly meritorious author, Archbishop Trench, of Dublin. In his "Study of Words" (p. 173, note), he remarks, after noting the fact of the dispute, whether words were θέσει or φύσει, "it is needless to say that the last is the truth"; and one seems to see on his face the smile of conscious superiority to those poor Greeks, who labored so long over a matter which could be settled in half a sentence, by a mere unargumentative "it is needless to say," without statement of reasons or explanation of meaning. And the Archbishop is supported, solidly and heartily, by that immense majority of the human race who know each his own language alone, and who are persuaded that only those that speak it really speak at all. Every linguistic scholar is aware how wide-spread and deep-rooted this feeling has been and still is; how it has been the foundation of many a race-name, assumed by the race to itself as self-asserted 'speakers,' all outsiders being "barbarians" or 'babblers.' And it would be very easy to find even in our enlightened communities men who, though they may know that other people have other names for things than their own, yet believe, outspokenly or in their secret hearts, that those are mere nicknames, only their own being the real thing.

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Doubtless we should do wrong to assume that Trench and his fellows hold names in this sense to exist φύσει: that is to say, that for every conception there exists a single "natural" name, all the others being "unnatural," or "artificial," or whatever else they may choose to set up as opposed to "natural."

For, as every well-informed person is aware nowadays, there are for our current conceptions as many different names, names somewhat unlike or totally diverse, as there are languages in the world — let us say, a thousand; and, apparently, each one of the thousand has as good a right to claim that it exists φύσει as any of the other nine hundred ninety and nine. Can any good reason be discovered why the term applies to one more than to another? or why it belongs alike to all?

Each of the thousand plainly has its own supporting community, its constituency. Perhaps, then, each corresponds to the peculiar nature of its community, comes φύσει to every individual member thereof. There are, in plenty, differences of race-endowment, differences of common circumstance and education, of community atmosphere; with some of these the differences of expression may be correlated. May be so, certainly; but are they so? As regards race, it is indeed true to a very considerable extent that men of the same race employ more or less kindred expressions for a good part of their common conceptions. But then, there are ways enough of accounting for this without involving the answer φύσει; and there are also exceptions enough to make us cast out this answer as impossible. Take, for example, the full-blooded Celt of Ireland who uses only English names for things, the one of Wales who uses only Celtic, the one of France (there must probably be such, if there were only a test by which we could discover him,) who uses only Romanic. Take the Jew of pure lineage, talking just as the community talks with whom his lot happens to be cast. Note what names the African uses, in the various lands of his former or present servitude, while bearing in his aspect the most convincing marks of undiluted descent. Or come into an

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American community, and pick out, by a little careful examination or genealogic inquiry, the representatives of a dozen diverse nationalities, and find them all calling the same things by the same names, knowing no other. This does not look very much as if names came by any kind of φύσις that is characteristic of a race. As for one that should be characteristic of a grade of ability, a cast of personal disposition and character, a tone of education and enlightenment, that is still more out of the question; every one knows that in any single community of accordant speakers such discordances, in all possible kind and degree, are abundantly found.

But if, weary of this superficial and empirical inquiry, we look more deeply to see how such a state of things comes about, we shall find a not less total absence of φύσις. We shall see that every normally constituted human being that comes into the world has a linguistic faculty amounting simply to this: that he is able to learn to speak, by acquiring those particular signs for ideas, and those methods of their use, which are established and current in the community into whose midst he is born. The whole consideration of the process by which the individual gets his "native language" teaches us this; and there is no other way of accounting for the fact that each person grows up to speak the tongue of his own community, and of his own special class of the community, without any regard to the race from which he comes, or to the capacity and disposition with which he is endowed, or to the grade of culture which he attains. If there be — we will leave that possibility open for the present, to take it up again later — a mode of expression that is natural to the individual as such, that forms a part of his φύσις, it is at any rate overborne and stifled by that other unnatural mode which his teachers impose upon him. It is difficult to see how, without laying himself open to the charge of an absurd disregard of patent facts, any one can put forth a different doctrine; can maintain, for example, that the child creates his speech by independent action, but creates it in necessary accordance with the speech of those about him. As well maintain that he creates certain melodies, devises certain trades, develops

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certain branches of knowledge, dances certain combinations of steps, without learning them, but by a spontaneous mental action, which some mysterious, undefined and indefinable, force brings into wonderful accordance with the like action of his fellows.

It may be asserted, I believe, without any chance of successful contradiction, that not a single item of the traditional English speech received by us from our forefathers has a vestige of right to claim to exist φύσει in any one of the innumerable individuals that employ it, to have been produced by him under government of an internal, instinctive impulse, that made it what it is and no other. The tie existing between the conception and the sign is one of mental association only, a mental association as artificial as connects, for example, the sign 5 with the number it stands for, or $\pi$ with 3.14159+.

That a system of signs won after the openest and most conscious fashion in this way is capable of answering to us the purposes of a language may be clearly shown in the acquisition of a foreign tongue. One may take a grammar and a dictionary, and commence, by the tedious method of translating into his own set of familiar signs that set which the French or the German child learns by a directer process, and may keep so long at it that a French or German page is as readily and surely intelligible to him as an English one; moreover, by going among the people who use that other set, and practicing himself in the use of them, he may "get them loose," as the Germans say, may mobilize them, associate them in such fashion with his conceptions that they will come into his mind, at first not less readily than his old English signs, and then even more so; and when this last takes place, he has deposed his first acquisition in favor of a second. If the process of substitution be not begun too late, after the habits of thought and habits of utterance have become too far fixed to be altered, it may go on even to the oblivion of one's "native speech," and to the winning of a command of the "foreign tongue" not inferior to that of any person to whom the latter is "native." In fact, *native tongue* means simply 'tongue first acquired": acquired under peculiar

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circumstances, and therefore in its own peculiar way; and having upon the mental powers, in respect to training and development, an effect which no second acquisition can have, in anything like the same degree.

There are, it is true, differences between the conceptions attached in different languages to words that seem synonymous. But these have nothing to do with determining the peculiar form of the varying signs. So there are marked differences between the conceptions of individual speakers of the same language. Every child begins with using a host of signs of which he is far enough from apprehending the meaning in fullness and with accuracy; and this imperfection of apprehension cleaves to him, in greater or less degree in different parts of his vocabulary, to the end. However much an idea may expand and grow clearer in his mind, or in that of the whole community, there is no corresponding change of the sign.

But there are not a few pictorial, imitative, onomatopoetic signs in our speech: is not the case otherwise with them? do not they, at least, have in them something of a φύσει character? Yes, in a certain sense; but not at all as the term φύσει is meant in the controversy which we are judging. So, among the mathematical signs we use, a round mark, reminding one of a hole, may be said to be more suggestive of vacancy or nothingness, and a single straight mark of unity, than the other figures are suggestive, each of its own meaning; they have in them an element of what we may call onomatopoetic force. But there is no necessity about this; nothing that makes the signs in question, to the exclusion of others, the "natural" representatives of their meaning. If there were, no other sign for 'naught' would be acceptable; and we should have to signify 'two' by two strokes, and 'three' by three strokes — as, in fact, the Romans and Chinese have done — and so on. Just so, when it is pointed out, we see that there is a kind of adaptedness in two parallel lines (=) to signify equality, especially when compared with > and <, as used to signify superiority and inferiority; yet, in the great majority of cases, the signs used (like + and —) are purely conventional, and answer their purpose precisely as well; and

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these particular purposes would be answered just as well by other signs, if once established in use for them. There is no such thing as a "natural" symbol for nonentity, or unity, or plurality; it is only that, in casting about for signs for this whole class of conceptions, we find certain ones for certain uses more readily suggested than others, which would have served equally well: the effective use is not dependent on any such considerations. That a certain bird is called a *cuckoo*, by a rude imitation of its note (for the bird really utters neither proper *k* sound nor *oo* sound, and its distinct interval of musical tone is lost in our reproduction), is an obvious and generally intelligible onomatopœia; but if the word *cuckoo* were φύσει the name of the animal, then the other animals that make imitable sounds would have also to get their names from them. And there is certainly no φύσις in calling, for example, the related American species by the same name, since they do not utter the same note. So the *crack* and *crash*, the *hiss* and *whiz* and *buzz*, and all their kin, have a like pictorial character, of a like value: it is by no means essential to their usefulness as signs, but is rather ornamental, giving them an added attraction. Such words testify to a disposition which is an interesting and a highly important one in language-making, and has to be taken carefully into account especially by those who are discussing the problem of the origin of language — the disposition, namely, to form and use signs that have about them an immediate suggestiveness, inside those rather narrow limits, imposed by the nature of the thing signified and the instrumentality employed for signifying it, within which it is practicable so to do. These imitative signs are by no means all primitive; the disposition toward their use also leads to their production from time to time, or, in the history of manifold change in the form of words, acts as a shaping force. It is essentially the same with the disposition which expresses itself in such lines as those celebrated ones of Pope: —

> When Ajax strives some rock's vast weight to throw,
> The line too labors, and the words move slow.
> Not so when swift Camilla scours the plain,
> Flies o'er the unbending corn and skims along the main. etc.



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Its office is not unlike that belonging to tone and gesture in our ordinary speech — impressive, decorative, artistic, but not indispensable in order to mutual intelligence, which is the great object of speech, and is fully attained by the use of signs respecting which we only know that others have formed with them the same associations as ourselves, and will, when we use them, think what we are thinking and desiring them to think. There is not one of these onomatopoetically signified conceptions which is not in other languages, or even also in our own, intimated by signs possessing no trace of an imitative character.

In full view, therefore, of the not wholly insignificant list of onomatopœic words existing in English, we may still maintain that the English names of things do not exist φύσει, that they are the results of a θέσις, of a θέσις which each one of us is led to make under government of the example or the direct instruction of others.

There is, however, another department of expression in which we might plausibly look for the clearest signs of a φύσις: namely, among the interjections, which should be, not the medium of signification of conceptions and judgments, but direct intimations of will and outbursts of emotion; and which thus lie upon the border between human speech and animal expression. Yet even here the effects of educated habit show themselves in the most perplexing manner. Speech is so essentionally conventional that its character infects even our exclamations: which, after all, are not so much means of relieving feeling as of signifying to others that we have such and such feeling. The Englishman, accordingly, does not say *ach* and *weh* and *so*, like the German, nor *fi* and *bah*, like the Frenchman. So far as consonants and vowels are concerned, we have no available evidence that the untrained, the purely natural, human animal would give vent to any definite system of utterances in order to express any definable variety of emotions. As regards, indeed, the tone of utterance, the case is very different. The capacity of tone to serve as the immediate expression of feeling, intelligible to all human beings without explanation and without training, is beyond

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dispute. This is even added as a powerful auxiliary, along with the other natural means of expression, to our conventional speech. Language without it loses half its power to move and sway, to incite and persuade. Here we seem to touch the true sphere of instinctive expressiveness. And this kind of utterance shades off into those universal acts of expression which belong to man purely as an animal, the laugh and the cry, the groan and the sob, involuntary movements of the muscles, which are analogous with the shiver, the rise of the hair and falling of the jaw, the smile, the watering or beaming of the eye, and all the other physical movements which make the countenance, the arms, the whole body, indicative of a felt emotion.

So far, then, as our present audible speech is concerned, we are able to find in it nothing but the added tone, the modulation of the voice, which can be said to have its existence and its value φύσει, by its own intrinsic nature. But the question still remains whether this must be regarded as the only possible sphere of natural expression. May there not be, after all, a connection between some part of the muscular apparatus and the intellectual action of the soul or inner self, whereby an idea, a conception, a judgment, has also its corresponding external and sensible action? If these meddling teachers, with their elaborated systems of conventional signs, would only keep out of the way, might not each human being, as fast as it formed ideas, produce a natural language for their expression?

In investigating this question, we are cut off from the aid of direct experiment. Every child does actually grow up in the company of trained and practiced speakers; it hears them speaking together; and, long before it can govern its own organs of utterance so as to reproduce the signs they make, it understands what many of these mean; it crows and prattles in imitation of them. To get at even a little community of two or three persons untaught to speak seems an impossibility; for humanity forbids us to bring up human beings in utter ignorance, like mere animals, merely to satisfy our curiosity; to deny them the fundamental human privilege

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of instruction in speech, in order that we may see how they would act. And accident neither has created nor is likely to create, the necessary conditions of the experiment. The nearest approach to it is made in the case of individuals who by exceptional causes are cut off from the ordinary education of their kind. This may be by isolation, or it may be by deafness. Cases of the former kind, of wild and solitary men, are exceedingly rare, and the accounts given of them are of doubtful authenticity or competency. But the deaf are abundantly found and easily observed; and the ordinary name of *deaf-mute*, by which we know them, shows what is their condition in reference to speech. One of this class ordinarily differs from a normal human being only by the disabling of a single nerve, that which is sensitive to the vibrations of the tympanum, and reports them to the brain as sound, or else in the more external organs that produce the vibration. The apparatus of mental action is perfect, the apparatus of articulate utterance is also perfect; nothing is amiss with the mechanism which connects the two and coördinates their movements. Here, then, is quite what the φύσει theorist wants; a human being cut off from the disturbing influences of linguistic education, but accessible to light of every other kind, so far as it is not dependent on that education. He is placed in the midst of human society, which the great apostle of the φύσει theory, Steinthal, declares* to be the only condition indispensable to the development of speech. If, now, the deaf person produces articulate utterances as distinct permanent signs of his conceptions, if deaf persons of the same race or community produce utterances accordant with one another, such as are those of the ordinarily educated individuals in a community, if deaf persons of different race or community produce utterances that vary by differences resembling those found to prevail among existing dialects and languages, then the φύσει theory has a basis of observed fact to rest on; if otherwise, it has none. And that the case is otherwise does not need to be pointed out. Even the man isolated by solitude gets by degrees, in the conflict between his higher than

* *Abriss der Sprachwissenschaft*, i. 83, 84.

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merely animal powers and the circumstances of his life, a certain amount of education by experience: he learns to know and classify the objects of his daily observation, to appreciate rudely the operations of the more obvious natural causes, to connect and separate and anticipate, in a manner which, if far short of what is easily within our reach, is at least beyond what any other animal can compass: he ought, then, if language is an instinctive human product, to have something of a language for his entertainment and his aid. It is certainly more important to him than to others, since he is debarred most of the means of improvement which are open to them. Yet, as we have seen, even Steinthal does not venture to claim that he will talk, but rather postulates society as the only medium in which the heaven-implanted germs of speech can develop themselves. I do not question that he is right as to the fact; but his admission appears to me a virtual abandonment of the φύσει theory.

As the anomalies of linguistic life thus seem to furnish no evidence of a power of immediate natural expression, we have next to examine the regular progress of the history of language, and see if this exhibits any traces of such a power. If there were a natural adaptedness of certain signs to certain ideas, we ought to be able to discover its influence among the variety of those which govern the development of speech. But, in the first place, it seems to make decidedly against the existence of the influence that there is such utter discordance among the names given by different communities to the same conception. Within the sphere of emotional expression, as pointed out above, the elements are of kindred character in all beings, and universally intelligible. The laugh, the scream of pain, the tone of anger or of grief, need no interpreter. But it is far otherwise with the signs of ideas. Languages, words, are absolutely unintelligible to him who has not learned to speak them. It is all in vain to appeal to the inner sense of meaning to help the explanation, for instance, of a Lycian or an Etruscan inscription; he who should attempt it would be simply laughed at. In the changes of form and changes of sense which constitute the main growth of speech,

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we equally fail to find any regulating princple of the kind here referred to. Let us take as an example our word φύσις itself. It contains as its central element the root φυ (*phü*, a *p* with an audible *h*, a puff or flatus, following it), altered, it is believed, from a yet earlier *bhû*, and having the sense of 'grow.' That there is in any human organization a state of things conditioning *bhû* or φυ as the natural expression of the conception of 'growing,' no one probably, will be bold enough to maintain. Far from this, we do not even know whether that sense was absolutely the earliest one belonging to the word, whether it was not obtained by a transfer, even a distant one, from some other sense. Were it not for Greek usage, the root would seem rather to signify simple existence (Skt. *bhû*, Lat. *fu-i*, our *be*); and all the acuteness of the φύσει theorists would have been incompetent to demonstrate the transfer. The ending σι which makes the derived word is altered from an earlier *ti*; the same element is found, still otherwise altered, in our *grow*TH. Here, again, if there had been any natural adaptedness in the syllable *ti* to express, in combination with a root, the particular modification which this actually expresses, it ought to have exerted a conservative influence, keeping the element unchanged in form, or allowing it to alter only in a certain way, in accordance with the change of the idea. But no such thing is true here; nor anywhere else in language. The word *bhûti* has become φύσι- without any reference to meaning; the transformations of its *bh* and *u* and *t* are due to phonetic influences which wrought equally through the whole language, regardless of the sense of a single element affected by them. Comparative philologists have not seldom claimed that the onomatopoetic character of a word has protected it from phonetic change; but no one has ever detected a similar protective influence as exercised by the sense of the word. Nor can we discover any conservation in the opposite direction — any, namely, that has prevented a transfer of meaning, as being inconsistent with the unchanged audible form. Of the absence of such an influence we may find evidence enough in the history of this same word φύσις and its relatives. Φύσις, we have seen,

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means most literally the 'action of growing'; and how far this lies from its other uses, so much more wide and indefinite as they are, needs not to be pointed out. The addition of a simple adjective ending makes the derivitive φυσικός; and while *physics* and *physical* and *physicist* show only a development of meaning akin with that which has taken place in φύσις itself, *physic* and *physician* and *metaphysics* exhibit curious movements in quite other directions. We have noted above the change, in Sanskrit and Latin and Germanic, of the signification of the root from 'grow' to 'be.' And *bhûti*, the close analogue of φύσις in Sanskrit, has taken the prevailing sense of 'prosperity,' instead of 'nature.' *Nature* itself, our equivalent for φύσις, is a word of Latin origin. It likewise has a root at its centre; and the oldest form of this is *ga* or *gan*, 'be born.' Relics of the *g* which was once the main stay and support of the meaning are to be seen in *cognate*, *agnate*, and their like. All, then, that is left in *nature* of the significant syllable which lay at the foundation of its history is the initial *n*, which many etymologists, not without a certain reason, look upon as a secondary addition, forming *gan* from a more original *ga;* the rest is a mere accumulation of formative elements, suffixes. And though there may be a degree of analogy between the conceptions 'be born' and 'grow,' it is by no means such as should by any necessity lead to the development out of both of a name for 'nature.' The Latin derivatives which have most analogy in point of formation with φύσις are *natio* from the altered root, and *gens* (*genti*) from its more primitive form; and how unlike they are in meaning to φύσις, and even to one another, is plain enough; while from *gens* we get in our language, secondarily, such curious varieties as *gentle*, *genteel*, and *gentile*, in defiance of all laws of the connection of sound and sense.

And so, if we were to extend our search, we should find it to be, through the whole domain of language: the utmost conceivable variety of expression of the same idea in different tongues; a great diversity of derivation of the expressions for any given idea; a bewildering multifariousness of meaning in families of related words: nowhere in the known history of

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language-development any trace of a domination of sound by sense, or of sense by sound. Not by any means that there are not reasons, and in a host of cases discoverable reasons, why things are called as they are; but they are reasons founded, not in natural connection, but in previously formed associations, in already established conventions. When we nowadays want to signify a new conception, we have recourse to the (as above shown) purely conventionally used material lying within our reach, in our own tongue or elsewhere. We make a transfer of meaning, without other change, in a word already in use, as in *gravity;* or a derivative, as *galvanism;* or a compound, as *lightning-rod;* or we go deliberately to the anciently used stores of expression of some extinct tongue, and piece together a new vocable, as *thermometer;* or we variously combine two or more of these methods. There is always involved in the act some change of form, or of meaning, or of both; but the single underlying principle is that the new designation is obtained where, according to the existing habits of the language, it can most conveniently be found. No one ever sits down to let the idea strike in upon his soul and evoke an answering utterance: the very suggestion of such a thing is ludicrous; nor does the utterance ever slip out instinctively, without premeditation. It is all a process of the development and multiplication of usages. People having been in the habit of doing so and so, they are led, when occasion arises, to do this and that also: the new habit being connected with the old by some tie of association, it matters little what. To follow the history of this development is a task of the highest interest; in it are bound up the most valuable results of the science of language; by its aid we trace the evolution of knowledge, of thought, of institutions. But it does not bring us to—nor even, in my opinion, toward—a condition of things where we recognize the existence of any natural tie between the conception and its expression, between the idea and the word. On the contrary, we are led thereby to see the more clearly the essential congruence, in the midst of their more adventitious characteristics and their circumstances, of all the various processes of language-getting

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and language-making. He, in the first place, who acquires a "foreign language" finds, by the ear or by the eye, certain combinations of sounds, which he is able more or less accurately to reproduce, and which he learns to associate with their several ideas, and to use in combination with one another, familiarly and freely, and also "correctly": that is, according to the methods usual in a given community, methods which might just as well be otherwise, if the common consent only willed it so. Again, the child learning to speak does only the same thing: he too hears and imitates certain combinations of sounds, associates them with rudimentary conceptions which he is led to form, and puts them together, at first imperfectly and awkwardly, into the phrases which the usage of his community accepts. And, in the third place, through the whole traceable development of language, the language-makers have not been giving vent to natural and directly intelligible utterances; they have, rather, been increasing, by methods of whose nature and results they were themselves only dimly conscious, their store of conventional signs, elaborating new combinations of sounds which should henceforth be associated with certain ideas, and used as their representatives. It makes, properly speaking, no difference to the users whence their sign is obtained; only, as this is intended for the general use of a community, and as it must pass the ordeal of their acceptance before it can become a part of language, it is gained in such a way as involves the least practicable change of existing habits, the least possible shock to prevailing preferences—or prejudices, if we choose to call them so. We express this prosaic fact in imaginative form by saying that it must not be "opposed to the genius of the language." This does not, however, prevent the tie of association whereby the new sign is connected with the old from being often a very slender, a remote, even a fantastic or senseless one. Such cases, to be sure, are the exceptions, and to be explained by the special circumstances of each, if we can only command knowledge of them; but they have a high theoretic importance, as showing what the practical end of word-making is, and how it justifies even the most questionable



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means. On the whole, the body of expression grows and changes by an almost insensible process, step following step, each new sign attaching itself quite closely to an old one.

It is only by taking this view of the history of speech that we can explain its leading facts, and especially that capital fact, the oblivion of etymologies. In any given language, it is but a part of its words, often only a very small part, which even the skilled etymologist can carry back through even a few steps of their history, toward their ultimate roots. And as for the generality of speakers, they are ignorant and heedless of all etymological connections; to them, the word means the thing, and that is the end of it. For a time, and in a measure, the relation between primitive and derivative maintains itself; but it is by the mere power of inertia; if there were a positive conservative force involved, if its maintenance were essential or important, it would not be let go. As things are, it is of great consequence to the practical usefulness of language as an instrument of communication and of thought that the oblivion in question do take place, that our signs for ideas be not encumbered with etymological reminiscences. And the changes of form and of meaning, under the government solely of convenience, do go on unchecked, and independent of one another: there is no limit to the extent to which a word may change its form while retaining its old meaning, or its meaning while retaining its old form; or to which it may wander from its primitive condition, both inner and outer.

We do not find, then, in the traceable history of language, any more than in its present condition, evidence that the names of things exist φύσει. No such principle is called for in order to explain the facts; none such seems even admissible, as reconcilable with the facts. It now only remains to inquire whether there was or must have been something different at the outset, in the actually primitive period, that of the origin of language. Each existing conventional usage or habit founds itself upon a predecessor of the same character, as far back as we can go: was the absolutely lowest course of the foundation of another character? are we to recognize

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there a real internal correspondence of sound to sense? If there be any such thing in language, it is to be found only there.

But, as hardly needs to be pointed out, if this last be true, there is a strong presumption against its being found there, any more than elsewhere. What we can discover no traces of in all the later periods of speech, we may well despair of detecting in the earliest. To assume it out of hand, as the manner of some is, without even deigning to attempt its proof, but simply setting down as superficial or mechanical those who hold any other view, is certainly in the highest degree unreasonable. On the contrary, it may properly enough be claimed that if any sufficient and satisfactory way can be made out, of accounting for the origin of speech without bringing in as a factor any natural correspondence of sound to sense, but by appealing only to those forces which are seen in action in the later periods, and in their recognized and usual modes of action—then that account of origin will have the whole body of probabilities overwhelmingly in its favor.

And certainly, such an explanation lies close at hand, and is easy enough to find. We need only to recognize the impulse to communication as the force most immediately active in the production of speech, to acknowledge that man spoke primarily in order to make his feeling or thought known to his fellows, and all difficulty is removed. It will then follow that whatever would most readily conduce to mutual intelligence would be made the first foundation of expression: whether a reproduction of the natural tones and cries expressive of emotion, or an imitation of the sounds of nature, living or lifeless, or any other kind of imitation; whether, again, by tones addressed to the ear, or by gestures or grimaces addressed to the eye—for the theory would fully combine and turn to account all the known varieties of expression, leaving that one which experience should show the most available for its purposes to win the preference over the rest, and finally, perhaps, to well-nigh crowd them out of use. The beginnings thus made would certainly be of a rude character—even as sticks and stones for instruments, as

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fig-leaves and skins for garments, as caves and holes in the ground for dwellings, as scratches with sharp points and daubs of colored earth for pictorial art, as yells and groans for musical art: and so on. To adopt the theory of origin here proposed is equivalent to paralleling speech with these other human acquisitions and branches of culture, as being an instrumentality, gradually wrought out by the exercise of the peculiar powers with which man is endowed, and answering purposes which are human only; as brought into its present state of perfection, greatly different in different races, by slow accumulation, improvement, evolution, according to the various gifts and circumstances of each race. This view of language doubtless appears to some to be lacking in dignity; but if it is supported by all the facts and inferences of language-history, a sentimental prejudice can avail nothing against its reception.

And that it is so supported appears to me true beyond all reasonable question. If there is any other acceptable theory, I know not who has set it forth and given it a solid foundation. Those who reject it have wholly failed to realize that the burden of proof rests upon them, to show, or make probable, that there is, or ever was, a power of natural expression in men whereby certain combinations of articulate sounds are produced as the instinctive signs of certain articulate conceptions. I cannot see that they have produced any good evidence that there exists such a thing as the natural uttered sign of a conception. As has been pointed out above, the natural utterences of man do not signify conceptions; they intimate only feelings, emotions. If a human being feels a certain kind of lively pleasure, he laughs; if the contrary, he cries, or groans, or sighs, or something of the sort; if he is struck with astonishment or horror at the sight of anything, he may utter an exclamation; but it will only signify his feeling in view of it, not the thing itself. So much as this is instinctive, subjective; but it is not of the nature of human language; it is on the same plane with the ordinary utterances of the lower animals. There is no conversion of it into language until that motive is added which is the dominant and almost the

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only conscious one through the whole after-history of language: namely, the intent to communicate. This, by a change which is almost imperceptibly slight at first, while yet of deep and wide-reaching importance, lifts the whole action up to a higher plane. It inaugurates an instrumentality which, though cut loose from any internal connection with the operations of the mind, yet makes itself their ally and aid, and is, precisely on account of its extraneousness and its conventionality, capable of indefinite increase, development, refinement. It is like the production of instruments, in place of a sprouting out of new arms and legs, to answer to the higher needs of the more skilled workman. It comes to bear a wonderful part in the development of the individual mind, and in the cultural progress of the race.

There is nothing really derogatory to the creative power and self-centred action of the human soul in making it thus dependent for its development upon what seems a slight and extraneous motive: nothing, at any rate, more than in making man's development in all other respects dependent upon his position as a social being. It is confessed that the wholly solitary man would never be anything but an utterly wild savage; in the collision, the emulation, the mutual helpfulness, that come of sociality, are born all the arts of life. The greatness of man consists in what he was capable of becoming, not in what he actually was at the outset. In his low estate he was accessible to only the lower motives. He is, at the best, a short-sighted being, capable of taking but one step forward at a time, and never quite knowing where that will lead him; but also capable of maintaining the ground he has won, finding out what it is worth to him, and in due time taking another step. All his grand acquisitions have had their small beginnings and their slow growth, each generation adding to what it had received from its predecessor; and language just as much and just as plainly as the rest.

The doctrine of those who deliberately answer φύσει to our question I cannot help regarding as mainly a prejudice, and resting on a foundation of misapprehension. Because, in the history of development of human expression, the voice has

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come to be the greatly-prevailing, the well-nigh exclusive, instrument of expression, therefore they hastily conclude that there is a special natural relation between the mental apparatus of conception and judgment and the physical apparatus of sound-making—a relation which, as we have seen, is wholly imaginary. They talk learnedly about the reflex-motor action of the nerves, and assume that, when an impression comes over one, it causes him to utter or imagine a responsive sound, somewhat as a sense of the ludicrous calls forth a laugh, a sensation of fear, a crawling feeling, a dash of cold water, a shiver, and the like. They overlook certain essential differences between the two cases: in the first place, that these reflex-motor actions are the intimation of subjective conditions only, which conditions confessedly give rise also to utterances—but these utterances are not language, are not even its beginnings, but only its suggestion and preparation; and, in the second place, that the actions referred to are actually seen and demonstrated in living men, of every race, that they are substantially the same in all, that they may be controlled, but not altogether obliterated, much less interchanged and varied, under purely social influences, without regard to race; while the variety of expression of ideas is unlimited, and its choice dependent on nothing but education. To support the φύσει doctrine by quoting sporadic efforts at independent expression on the part of children growing up in the midst of speaking men is quite futile. Children are imitative beings, and sometimes a little wayward; they catch soon from their surroundings the trick of applying names to things, and, being aware of no particular reason for those they are taught, they try now and then a new one of their own making, enjoying the exercise of a degree of independent ingenuity. Nothing more than this is needed, I believe, to explain away all the scanty array of alleged facts which have ever been brought up in defense of the theory of natural expressiveness. To give that theory a real basis, it would be necessary to show that a child growing up alone, or among mutes, would also produce a body of articulate utterances, of definite meaning and application: or (what has been noted above as a much more accessible proof) that the deaf do the same thing.

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Eminent knowledge in psychology, in physiology, in phonetics, in any of the single departments which contribute their part, or their aid, to the science of language, does not by any means lead necessarily to correct views in linguistic philosophy. One may, for example, be the greatest living phonetist, and yet be still puzzling himself with the question what is, after all, the real tie of connection between sound and sense in language. One may be a profound metaphysician, and yet wholly mistake the same connection, taking with regard to the most essential points in the history of language an untenable, even absurd, position. It would not be difficult to cite individual examples of both these classes.

Our conclusion then is, that there is no proper sense in which the names of things can be said to exist φύσει; not only now, and through the ages of recorded speech, but even back to its very beginning, every name has been the result of a θέσις, an act of human attribution.

And yet, there is at least a certain sense in which the θέσις itself may be said to be performed φύσει; and it is in great part owing to a misapprehension of this sense that the answer φύσει has been so often given to the main question. It is undoubtedly, in a manner, "natural" to man to speak. We have to say "a certain sense," "in a manner," because the naturalness does not consist in man's individual nature alone, but also in his circumstances; with all his gifts just as they are, he would not speak unless placed in the company of his fellows. It is in just the same sense "natural" to man to live in houses, to wear clothes, to make instruments, to form societies, to establish customs and laws; yet hardly any one would think on that account of maintaining that, for example, coats and telescopes existed φύσει: while it is nevertheless quite as true of them as of nouns and verbs.

He who answers φύσει, therefore, to the question we have been discussing, lays himself open to the charge of total misapprehension of the most fundamental facts of language-history; he who answers θέσει needs only to show by due explanation that he does not mean to imply that any individual can successfully fasten any name he pleases upon any idea he

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may choose to select; since every change must win the assent of a community before it is language, and the community will ratify no arbitrary and unmotivated changes or fabrications. It is in this action of the community that another great part (besides that spoken of above) of the difficulty resides for those who hesitate to admit the doctrine of θέσις: they see so clearly that no man can do what he will with language that they are led to deny the action of individuals on language altogether. To do this is to mistake the nature of the conservative force which resists change: in reality, this force all resolves itself into the action of individuals, working under the same guidance and limitation, of motives and of circumstances, by which each of us is directed, and of which each one may, if he set himself rightly at work, become fully conscious.

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# PROCEEDINGS.

SIXTH ANNUAL SESSION,

HELD AT HARTFORD, JULY, 1874.

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# AMERICAN PHILOLOGICAL ASSOCIATION.

HARTFORD, CONN., July 14th, 1874.

The Sixth Annual Session was called to order at 3 o'clock P. M., in the hall of the Public High School, by the President, Professor Francis A. March, of Lafayette College, Easton, Penn.

Addresses of welcome were made by the Rev. Professor William Thompson, D.D.. of Hartford, chairman of the Committee on Entertainment, and the Hon. Joseph H. Sprague, mayor of the city, chairman of the Local Committee, to which the President replied.

The Secretary presented his report, announcing that the persons whose names follow had been elected members of the Association:

**Professor Stephen G. Barnes, Iowa College, Grinnell, Iowa; Mr. Thomas Davidson, St. Louis, Mo.; Mr. A. Eiswald, Savannah, Ga.; Professor John L. Johnson, University of Mississippi, Oxford, Miss.; Professor Joseph Milliken, Ohio Agricultural and Mechanical College, Columbus, Ohio; Professor E. C. Mitchell, Baptist Theological Seminary, Chicago, Ill.; Professor Philip Schaff, Union Theological Seminary, New York City; Mr. Edward F. Stewart, Easton, Penn.; President James C. Welling, Columbian University, Washington, D. C.; Professor John Williams White, Baldwin University, Berea, Ohio.**

The Secretary also reported that M. Abel Hovelaque, of Paris, had presented to the Association copies of several of his philological publications.

On motion, Professor William F. Allen and Mr. Charles J. Buckingham were appointed auditors of the Treasurer's report.

On motion, it was

***Resolved*, That the Treasurer be requested to place fifty copies of the volume of Transactions recently published at the disposal of the President, for distribution to contributors to the funds of the Local Committee at Easton, Penn.**

Professor Charles H. Brigham, of Ann Arbor, Mich., exhibited an Ethiopic manuscript.

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This manuscript is apparently a collection of prayers, and probably prayers used at the altar service. It is on thin parchment, in three strips sewed together, in the whole six feet in length, and three and a half inches in breadth. The script is partly in black and partly in red ink, the red lines apparently marking the responses of the attendants in the service. Three-fourths of all the Ethiopic alphabetic characters are found in the script. The execution is very careful and nice. Each strip has at its head an "illumination" rudely done. The reading is from left to right. The age of the manuscript cannot be determined; but it is probably not very old. The manuscript was found by a workman in the yard of the railway station at Jackson Junction, Michigan, in the month of November, 1873.

Professor S. S. Haldeman, of the University of Pennsylvania, Philadelphia, read a paper on "An English Vowel-mutation, present in 'cag—keg.'"

The short vowel of "fat" is rather rare in the dialects of Europe, and, when present, it is probably due to a Celtic influence. It is well established in English, where, from its affinity with ŏ of "ebb," the two present more than two hundred examples of interchange, when archaic and local forms are enumerated. Among these are ambassador and embassy (where am- is deemed to be the more correct), annual and perennial, arrant and errant, assay and essay, bank and bench, brant and brent, canal and kennel, catsup and ketchup, charity and cherish, drag and dredge, frantic and frenetic, hackle and heckle, tarras and terrace, thrash and thresh, wrack and wreck, wrastle and wrestle.

Mr. W. W. Fowler, of Durham, Conn., read a paper on "Paradoxes in Language."

Words standing for white (color), light, and heat, in the Indo-Germanic languages, are from roots signifying to shine; on the other hand, many words standing for black (color), darkness, and cold, are from the same class of roots; for instance:

English *black*, *blank* (white), and *bleach*, from root *bha*, "to shine"; English *swarthy*, German *schwarz*, from root *svar*, "to shine."

Latin *furvus*, "dark, black," *baliolus*, "dark, swarthy," from root *bha*, "to shine"; Latin *candidus*, "white," from root *skand*, "to shine." *Ater*, "black," is probably from root *ath*, "to burn" (cf. Sanskrit *athara*, and Persian, *atar* "fire").

Greek *αἰθός*, "black," from root *idh*, *αιθ*, "to burn," "to glow"; *λευκός*, "white," from root *ruk*, *luk*, "to shine."

Sanskrit *krshṇa*, and Lithuanian *karsna*, "black," from root *kar*, "to glow," "to burn."

This paradox is explained by the use of the same or similar words to express the primary and the secondary effects of the sun and fire; the primary effects being light, brightness, whiteness; the secondary effects, a change in the color of substances—blackening (or darkening). Words meaning dark (color) or black, may be translated by the terms "sun-burned" or (simply) "burned"; a black color as well as a brown color is a burn-color. The English *swarthy* is "sunburned"; so originally was the German *schwarz*. The words *ink* (*encaustum*," burned in"),

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*coal* (from the root *gvar*, "to glow"), and *soot* (from the root *su*, *sva*, "to glow") illustrate the process by which many words standing for dark colors, arose from roots signifying "to shine." The principal color-names (generic as well as specific) being derived from radicals signifying "to shine"—in other words, the sunlight being the main source of color—we may come to know how it was that the same color-names stand for different colors in different languages, e. g.: Latin *flavus*, "yellow," corresponds to Teutonic *blava*, "blue"; or for different colors in the same language, e. g.: Greek, γλαυκός, "blue," "green," and "gray."

Again, the words, *glow*, *gleam*, *glimmer*, as well as *gloom* and *gloaming* (the twilight), come from root *ghar*, "to shine." *Gloom* appears to mean, first, the flashes of lightning from a thunder-cloud, secondly, the lowering darkness of a thunder-cloud. *Gloaming* is. properly, light by flashes, intermittent light, as at twilight, particularly in high latitudes. *Morning* (*morgen*) and *murky* convey opposite ideas, the former of light, the latter of darkness, but the primitive meaning of both referred to light, i. e., twinkling or intermittent light. *Day*, *dawn*, and *dazzle*, as well as *dim*, and perhaps *dusky*, are from the root *da*, "to shine"—a root which appears as the basis of a large number of Indo-Germanic words referring to the different phenomena of the visible heavens; *day*, *dawn*, and *dazzle* describe the brightness, while *dim* and *dusky* describe modified or lessened brightness of the sky, light being the fundamental idea in both cases. *Blind*, from root *bha*, to shine, expresses *blended*, mixed light, when things are not clear.

Certain words, expressing heat and cold, are alike derived from roots which signify to burn; compare Greek αἴθω, "to burn," αἴθων, "burning," with αἴθριος, "cold," from root *idh*, αιθ, "to glow," "to burn." Sanskrit *çyâ*, *çyâ-yate*, "to burn," and "to freeze"; *çîta*, "cold," and Latin *ci-nis*, "ashes"; German *hei-ss* and English *heat*; from root, *kî*, "to burn." Sanskrit *plush*, *ploshati*, "to burn"; Latin *pruina*, "a glowing coal," *prurire*, "to burn," "to itch"; Gothic *friusa*, Old Norse *frostr*, English *frost*, Old High German *freosan*, English *freeze* (cf. German *frostbrand*), from root *prus*, "to burn." Greek καίω, and καῦμα, "to burn," and "to be cold." Latin *uro*, *urere*, "to burn," and "to freeze" (so used by Cicero, Virgil, Pliny, and many other classic authors), from root *us*, "to burn."

All the cases cited in this paper may be explained by showing that the same or similar names are often given to cause and effect, or to two similar or apparently similar effects from different causes, or to different effects from the same cause.

Professor Fisk P. Brewer, of the University of North Carolina, Chapel Hill, N. C., described a fragmentary Manuscript of mediæval Latin preserved in the Library of the University of South Carolina, and exhibited a copy of it.

It is a single leaf of parchment, bound up with a folio edition of Pliny printed at Treviso, near Venice, in 1479. It is written with two columns to the page, in the style prevalent about 1450. The letter *e* is used for the diphthongs *æ* and *œ*; for *nihil* is written *nichil*; for *distrahant*, *distrant*; for *vehiculum*, *veiculum*; for *mitto*, *micto*; for *missus*, sometimes *mixus*; *cura* and *curia* are interchanged; as also *publicatio* and *puplicatio*, *estimatio* and *extimatio*.

The manuscript is a leaf from the middle of a series of statutes of a king who

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refers to himself by the exclusively regal title of *nostra celsitudo*, and alludes to his own previous *nova statuta*. In the present edicts he orders that market magistrates shall no longer compel citizens to purchase salt in greater quantity than they desire, nor restrict the places where salt and other necessaries of life may be sold. He prohibits officers of the provinces in general, justitiarii, camerarii, and others, from accepting loans and gifts from the provincials, as had been customary under a variety of pretexts. He further commands local authorities to respond promptly to requisitions of procurators for help in preparing camps and buildings and in planting and cultivating vineyards, and, in case of their delay, directs the procurators to have the necessary castle-repairs effected, with the assurance that their expenses shall be repaid from the treasury. The practice of impressing men and animals into the public service without proper compensation, is prohibited. The hire of a man and a horse is fixed at one *tar*, and it is ordered that in the purchase of horses, or the death of hired animals, the value shall be estimated by three or four good and worthy men.

The following words of late Latin are found in this document: *fundicus* connected with our *funds*, meaning a "bourse" or "market-place"; *magistri fundicarii*, "market officers"; *fundicare*, "to pay the market tax"; *bajulus* (bailiff), the title of a magistrate; *azarium* (French acier), "steel."

A recess was taken till 8 o'clock.

## Evening Session.

The Association resumed its session, Dr. J. Hammond Trumbull, of Hartford, Conn., Vice-President, in the chair.

The Secretary reported the election of new members as follow:

Rev. W. L. Gage, Hartford, Conn.; Professor G. S. Hall, Antioch College, Yellow Springs, O.; Rev. Charles Hammond, Principal of Munson Academy, Munson, Mass.; Professor Selah Howell, Christian Biblical Institute, Stanfordville, N. Y.; Professor John S. Lee, St. Lawrence University, Canton, N. Y.; Professor R. H. Mather, Amherst College, Amherst, Mass.; Mr. Sydney P. Pratt, Boston, Mass.; Mr. H. B. Richardson, High School, Springfield, Mass.; Professor Charles C. Shackford, Cornell University, Ithaca, N. Y.; Rev. Leopold Simonson, Hartford, Conn.; Professor William Thompson, Theological Institute of Connecticut, Hartford, Conn.; Mr. Minton Warren, High School, Waltham, Mass.; Professor James H. Worman, New York City.

Professor Francis A. March, of Lafayette College, President of the Association, delivered the Annual Address.

The study of the ancient literary monuments of the Indo-European speeches is now giving place to the study of living dialects, and of the relics of the ancestors of barbaric tribes. The more sober western leaders of the new generation are trying to ground the laws of language in physiological necessities and the facts of living dialects; the more adventurous are leaving the familiar fields of the Indo-Europeans.

A brief sketch was given of the work of the year in the study of dialects. An English Dialect society has been formed under the direction of Mr. Skeat and

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the inspiration of Mr. Ellis, and is vigorously at work collecting all the living varieties of English speech, and asking our aid. With it should be put A. J. Ellis's work on "The English Dialects in Great Britain and America," forming a part of his great work on "Early English Pronunciation"; J. A. H. Murray's "Dialect of the Southern Counties of Scotland"; C. C. Robinson on "The Yorkshire Dialects"; Prince Louis Lucien Bonaparte on "The English Dialects," in the Philological Society's Proceedings; Sweet on "Danish Pronunciation"; John Winkler's "General Netherland and Frisian Dialecticon," a thousand solid Dutch pages on the continental Low German dialects; Tobler on "The Aspirates and Tenues in the Dialects of Switzerland," an excellent paper in Kuhn's Zeitschrift; Halévy on "The Dialect of the Jews of Abyssinia"; the Abbé Martin on "The Chief Aramaic Dialects"; Dr. Bleek on "Grimm's Law in South Africa"; Dr. Carter Blake on "The Dialects of Nicaragua"; Mr. Thomas on "The French of the West Indian Negroes," especially at Trinidad; Professor Hartt on "The Language of the Amazons," in our own Transactions; and, most notable of all in its kind, Professor Trumbull's "Notes on Forty Versions of the Lord's Prayer in the Algonkin Languages." The greater part of this work on dialects is done with scientific caution, and is in full accord with the best scholarship of the old school.

In phonology, we have Mr. Ellis's work, and the invention by Mr. W. H. Barlow of an instrument, called a logograph, by which the comparative force and duration of the sound made in speaking is registered.

Of the more adventurous work, mention was made of a grammar by M. Lenormant, of the speech of the primitive population of Babylonia, which is claimed to be a representative of the parent speech of the so-called Turanian or Scythian family of languages, and to be likely to play the same part in reducing the languages to order which the Sanskrit has done in the Indo-European family, and also a comparison of it with Modern Finnish dialects, by Lagus; Mr. Isaac Taylor's book on the Etruscan, trying to show that to be Finnish or at least Turanian; F. Delitsch and J. Grill on "The Relation between the Roots of the Semitic and Indo-European Speeches"; and J. Edkins on "The Relation of the Chinese to the European Roots."

There has been also good work done in the old fields. Pott's great Lexicon of Roots has been completed, and only awaits an index. In the Celtic speeches, especially, we have a number of new undertakings of considerable interest. Chevalier Nigra's essay on the Irish manuscript of St. Gall, and the work of Ascoli on the ancient Irish glosses of Milan, and many articles in the Revue Celtique, are worthy of note, while the publication of a volume of essays in English on Celtic subjects, by Whitley Stokes, and the introduction of Celtic comparisons into the fourth edition of Curtius's Grundzuge, show the firm and familiar establishment of Celtic studies in England and Germany. This year is marked in Scandinavia by the Icelandic Millenial and the completion of Cleasby's Icelandic Dictionary. The early English Text Society has also celebrated with rejoicings and pride the tenth year of its labors, and has finished the texts of Pierce Plowman, and given us a new volume of most welcome Anglo-Saxon Homilies. Then there is the establishment of the New Shakespeare Society and the commencement of scientific and other linguistic examinations of Shakespeare's plays, all apparently going on with enthusiasm.

They are interested in England also, as in this country, in reforming the

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school pronunciation of Latin and Greek; but its promoters seem to be in unreasonable haste, and speak despondingly of the real progress of the year towards the new standard. The advanced studies of women in connection with the university examinations appear a decided success, and their permanent establishment and use seem to be already accepted in England.

After a brief reference to the triumphs of philology, it was asked what the advance of philology may be expected to do for improving the estate of man; and in answer followed discussions of a reform of English spelling; a universal alphabet; improvements in the structure of words, to make language more harmonious, more regular, and better suited to express scientific truth, and to aid in scientific discovery; improvements in the methods of education, and in the selection of objects of study; and changes in the treatment of psychology and the philosophy of history.

At the conclusion of the address, the Association stood adjourned to 9 o'clock Wednesday morning.

### Wednesday, July 15th—Morning Session.

The Association met at 9 o'clock, the President in the chair.

On motion, Mr. Alonzo Williams, of Providence, R. I., was appointed Assistant Secretary.

The Treasurer presented his report, which the Auditors certified to be correct, and it was, on motion, accepted. The receipts and expenditures of the past year were as follow:

RECEIPTS.

| | |
|---|---|
| Balance in treasury, July 22d, 1873, | $1,029.68 |
| Fees of 20 new members, | 100.00 |
| Annual assessments, | 465.00 |
| Interest, | 42.00 |
| Sales of Transactions, | 40.06 |
| | $1,676.74 |

EXPENDITURES.

| | |
|---|---|
| Printing Transactions, 1872, | $638.84 |
| " Proceedings, 1873, | 149.10 |
| Postage, express, stationery, and sundries, | 43.82 |
| Secretary's bill for postage, copying, etc., | 27.00 |
| | $858.76 |
| Balance in hands of Treasurer, | 817.98 |
| | $1,676.74 |

Professor W. S. Tyler, of Amherst College, Amherst, Mass., read a paper on "The Prepositions in the Homeric Poems."

The "parts of speech," as they are called by grammarians, are a classification, founded in the nature of language, but inevitably more or less artificial and

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imperfect, of the different kinds of words in their relations to each other and to the sentence. The number has varied much at different times. Aristotle, in one treatise, makes three; in another, four. The Stoics made nine. Some Roman grammarians made ten, eleven, or even twelve. The same words are continually passing from one part of speech into another. Thus, by a progressive falling off of emphasis, the demonstratives in many languages (e. g. English *that*, Greek ὅτι, and Latin *quod*) became first relatives, and then articles or conjunctions.

The prepositions were originally and properly adverbs, few in number, scarcely a score in Greek, about the same number in Sanskrit, and but little more than that number in Latin and the modern European languages. Primitive words with monosyllabic roots, although for the most part made dissyllabic in Greek by the annexation of a final vowel, the *proper* prepositions seem originally to have expressed such essential relations as up and down, over and under, to and from, in and out, on and off, etc., etc., which, in the nature of the case, would gradually pass from mere adverbs denoting the *direction* of motion or action, into prepositions expressing the *relations* between such motions or actions and the places, persons, and things affected by them. In the Homeric Poems we see this class of words in the transition state between adverbs and prepositions, sometimes standing alone with a fully adverbial force, and even when prefixed to a noun or compounded with a verb sometimes hovering between the office and force of the adverb and the preposition. In subsequent writers, such as Sophocles, Herodotus, Xenophon, and still more in the Greek of the New Testament and the Modern Greek, there is a constant decrease of the adverbial and separate use of the prepositions, and a regular and progressive increase of their use both as prepositions governing cases of the noun and as prefixes in compound verbs. A careful examination of all the cases in which words of this class occur in specimen passages of these authors yields the following table of statistics:

| | Before Substantives. | Prefixes to Verbs. | By themselves. |
|---|---|---|---|
| Iliad, | 47 per cent. | 34 per cent. | 19 per cent. |
| Sophocles, | 35 " | 59 " | 6 " |
| Herodotus, | 47 " | 53 " | 0* " |
| Xenophon, | 41 " | 59 " | 0 " |

Of all the verbs in the specimen passages, in the Iliad about 14 per cent. are compounded with prepositions; in Sophocles, 26; in Herodotus, 32; in Xenophon, 36; in the Acts, 40; in Tricoupes (the Modern Greek historian), 43.

Parallel with this relative increase of verbs compounded with prepositions, and apparently consequent upon the continually diminishing emphasis and force of that class of words, the repetition of the same preposition, both in composition with the verb, and again before the substantive, grows more frequent. There is scarcely a trace of it in Homer or Sophocles. It is rare in Herodotus. In Xenophon, it is not unfrequent.† It is common in the New Testament.

In the Iliad, not only is the verb less frequently compounded with the preposition, but the oblique cases of the substantive occur more frequently without a preposition or any other governing word. And when the preposition does stand before the substantive, or enter into composition with the verb, it seems often to

*That is, none in the passage of several pages which I used as a specimen. There are not wanting sporadic cases of tmesis and adverbial use, e. g., ἀπὸ δ' ἔθανε, vi. 114; μετὰ δέ, vi. 120.

† In such constructions as εἰσ-(or ἐμ-)βάλλειν (or βαίνειν) εἰς; ἐκβάλλειν ἐκ; συνστρατοπεύεσθαι σύν, etc., etc.



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retain more of its original adverbial force, or to hover between an adverb and a preposition, as in the familiar line, Il. 3, 12:

*τόσσον τίς τ' ἐπιλείσσει, ὅσον τ' ἐπὶ λᾶαν ἵησιν.*

Professor J. B. Sewall, of Bowdoin College, Brunswick, Me., read the second paper, on "The Distinction between the Subjunctive and Optative Modes in Greek Conditional Sentences."

In a discussion upon this subject at the last meeting of the Association, it was maintained on the one side that the difference between the Subjunctive and Optative modes in conditional sentences was only that of greater and less vividness, on the other that it was a difference in kind between supposed fact as contingent and supposed fact as merely conceived. The object of this paper is to briefly discuss this point.

If we ask how the fact of supposition is presented in the four classes of Greek conditional sentences respectively, the answer will be somewhat as follows:

1. In a conditional sentence of the first class there is a supposition relating to the actual state of the case—to reality; e. g. Dem. Phil. 1., 29, *εἰ δέ τις οἴεται . . . οὐχ ὀρθῶς ἔγνωκεν*, "if any one thinks . . . he has not judged rightly." It is a question of what really is, a supposition relating to actual fact. No implication that it is or is not reality is involved. Xen. Mem. 2, 1, 28, *ἀλλ' εἴτε τοὺς θεοὺς ἵλεως εἶναί σοι βούλει, θεραπευτέον τοὺς θεούς*, "if you wish the gods to be propitious, you must serve the gods." Do you wish, or do you not wish? It is a question of actual fact. So always. And if we should characterize a condition of the first class from the manner of its presenting the fact in supposition, we should call it a supposition relating to actual fact, or, for the sake of brevity, supposition of actual fact, generally implying nothing as to its existence in reality one way or the other, though sometimes assuming or taking it for granted.

2. In the second class, having secondary tenses of the indicative in both condition and conclusion, we have plainly a supposition implying the contrary to be the fact; e. g. Dem. Phil. 1., 1, *εἰ μὲν περὶ καινοῦ τινὸς πράγματος προυτίθετο λέγειν, ἡσυχίαν ἂν ἦγον*, "if it were proposed to treat of any new subject, I would keep silence;" implying plainly that it is not proposed to treat of any new subject, and therefore he does not keep silence. Id., ib. 5, *εἰ τοίνυν ὁ Φίλιππος τότε ταύτην ἔσχε τὴν γνώμην, οὐδὲν ἂν ὧν νυνὶ πεποίηκεν ἔπραξεν*, "if then Philip at that time had entertained this opinion, he would have done none of those things which he has done;" implying that he did not entertain this opinion at that time. We may characterize a condition of this class therefore as a supposition implying the contrary to be the truth, or, for brevity, a supposition of contrary fact.

3. Passing the third class for the moment, we have in the fourth class *εἰ* with the optative in the condition, and the optative with *ἄν* in the conclusion; e. g. Dem. Phil. 1., 25, *εἰ γὰρ ἔροιτό τις ὑμᾶς, εἰρήνην ἄγετε, ὦ ἄνδρες Ἀθηναῖοι; μὰ Δί' οὐχ ἡμεῖς γε, εἴποιτ' ἄν*, "for if any one should ask you, 'Are you at peace, O Athenians'? 'No, by Zeus, we are not,' you would say." The fact of supposition is here put forward as merely hypothetical—a fact of conception, without reference or implication in any way or kind as regards actual fact. It is not future any farther than a supposition of fact not a reality now nor in the past must be in the future if at all. The verbs in the condition and the conclusion are both in the aorist, which means that the Greeks eliminated the facts of the supposition

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from the element of time and held them in the mind as mere conceptions, never having been, not now being, never to be, in reality, so far as this assertion is concerned. Plato, Phaedo 67, E, *εἰ φοβοῖντο καὶ ἀγανακτοῖεν, οὐ πολλὴ ἂν ἀλογία εἴη*; "if they should fear and complain, would it not be very absurd?" Here again the fact of supposition is purely hypothetical, placed before the mind as a conception, without any reference or implication in relation to reality. So generally. The optative in the conditional sentence is the mode of possibility, that which might be, the mode of fact simply as conceived. And we may characterize a condition of the fourth class as a supposition of conceived fact.

4. We will return now to a condition of the third class, *ἐάν* with the subjunctive, etc. Plato, Phaedo 69, D, *ἐκεῖσε ἐλθόντες τὸ σαφὲς εἰσόμεθα, ἐὰν θεὸς ἐθέλῃ*, "when we shall have arrived there, we shall know the truth, if God wills." The subjunctive *ἐθέλῃ* here expresses an action continuing, uncertain, and future. The continuousness arises from the tense, the futurity partly from the tense of the principal clause, and partly from the mode, which, it seems to me, we may describe as the mode of uncertainty or contingency, i. e., the mode by which the Greeks chose to represent an action as uncertain, whether in reality it was so or not. *Εἰσόμεθα* expressly declares a fact, "we shall know," but it is contingent, and the mode used to express that contingency is the subjunctive. What would be the force of the sentence if it were a conditional of the fourth class? It seems plain that the assumed fact, *ἐλθόντες ἐκεῖσε*, would be thrown into the form of a simply conceived fact of condition, "if we should arrive there," and the conclusion also, "we should know," and the present condition, now only expressing uncertainty, would become a second condition, likewise of simply conceived fact, "if God should will." That is, the sentence in the first form positively declares a fact with a condition of mere contingency; in the second, it presents both the fact and its conditions merely as conceptions. The difference, therefore, is not one of degree, more or less vividness, but of kind, mere uncertainty or contingency on the one hand and pure conception on the other. So in the following examples: Dem. Phil. I., 29, *τοῦτ' ἂν γένηται, προσποριεῖ τὰ λοιπὰ αὐτὸ τὸ στράτευμα ἀπὸ τοῦ πολέμου*. Thucyd. II, 39, 4, *ἢν δέ που μορίῳ τινὶ προσμίξωσι, κατήσαντές τε τινὰς ἡμῶν πάντας αὐχοῦσιν ἀπεῶσθαι*. The latter is a general supposition, yet well illustrates the nature of the subjunctive as the mode of uncertainty or contingency.

The conclusion reached is, that the subjunctive in conditional sentences differs from the optative in that it is a form to represent the fact as uncertain or contingent, while the optative is a form to represent it as merely conceived; and that the four classes of conditional sentences may properly, and with sufficient accuracy, be thus described: the first, *εἰ* with the primary tense of the indicative, as a supposition relating to actual fact; the second, *εἰ* with the secondary tense of the indicative, as a supposition relating to contrary fact, or implying that the contrary is the truth; the third, *ἐὰν* with the subjunctive, supposition relating to contingent fact; the fourth, *εἰ* with the optative, supposition of conceived fact.

Professor L. R. Packard, of Yale College, New Haven, Conn., read a paper on "Homer's Odyssey, Book X., vv. 81–86."

The difficulty of the passage was illustrated by a review of the various explanations, ancient and modern, that have been suggested. The first line, and half of the second, it was shown, cannot be positively and precisely explained from the want of sufficient data. Only with regard to *τηλέπυλον* it was urged that it

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cannot mean "having high or wide gates," as some take it, but must mean "having gates far apart," a distance, either—as Nitzsch thinks—measured on the diameter, and so "long-streeted," or perhaps more probably measured on the circumference, and so "large in circuit," a description of the greatness of the city in Epic style.

It was pointed out in regard to the rest of the passage that previous explanations generally involve some assumption for which there is no ground here or elsewhere in Homer. Thus Nitzsch assumes, from the mention of two kinds of cattle in line 85, that cows are driven out earliest in the morning, and sheep come home latest at evening. So J. F. Lauer assumes that this meeting takes place at evening, and that the sheep-herd coming in greets the cow-herd going forth.

The view maintained in the paper assumed only this, as naturally in the mind of poet and hearers, that all kinds of flocks naturally spend only the day in pasture, and the night under the protection of the herdsman's home. This is the representation elsewhere in Homer, e. g., in regard to the Cyclops (Od. 9 *passim*), and to Eumaeus, (Od. 14, 13–22; 16, 3). This familiar idea is applied to the Laestrygonian country, without thought that the absence of any night there makes it inappropriate, and it explains the mention of the two kinds of flocks in line 85. A man who could dispense with sleep could be in the pasture through the twenty-four hours, but either kind of animal would naturally be at home for half of that time.

In the last line most explanations have translated ἐγγύς "near to one another." The word occurs some forty-five times in Homer, and in thirty-three cases in such a way (either because the subject is singular, or because some local genitive depends upon ἐγγύς) that it cannot mean "near to one another" but only "near" to something else. Of the other eleven cases (not counting the line under discussion), which all resemble this in plurality of subject and absence of dependent genitive, only one admits the meaning "near to one another." The usual word for reciprocal nearness is πλησίος. The plain inference is, that the line means "for near (to the home of the Laestrygonians) are the paths of day and night." The following journey is all near to this place, and all in a region of marvels, which is such because of its nearness to the western home of the sun (cf. Od. 10, 130; 12, 3 f., 166, 175 f., 201, 261, 284–93). The whole story is probably a natural exaggeration of the stories of shorter nights in higher latitudes brought home by sailors, which seems to be localized near sunset, and described without any thought of logical consistence in the parts of the fable.

Professor M. L. D'Ooge, of the University of Michigan, Ann Arbor, Mich., read the last paper of the afternoon, on "The Documents in Demosthenes on the Crown."

Professor Lipsius, of Leipzig, called my attention, not long since, to a new argument against the genuineness of the documents in the oration of Demosthenes on the Crown, which was first stated in its general bearing by Prof. Sauppe, of Göttingen.

1. Stichometric enumerations are found not only in Σ, but also in the MSS. of other families, as in Venetus F and in Bavaricus, and these enumerations correspond so closely as to warrant the inference that they have all a common source in one and the same original codex.

2. It appears that the count of these ancient στίχοι is in proportion to the length of the speeches and the number of the lines in our editions: e. g., in

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Orat. pro Halon., 345 στίχοι = 326 lines in Reiske; in Orat. de Cherson., 590 στίχοι = 559 lines R.; so in

I. Olynth., 265 στ. = 238 lines R.;
II. Olynth., 295 " = 272 " " ;
II. Phil., 290 " = 266 " " .

From this comparison we deduce a ratio of 30 στίχοι to 29 lines (= 1 page) of Reiske.

3. Applying this ratio to the Orat. de Corona we obtain the following: The number of στίχοι is given at the close of Σ as 2768, which would equal—according to our ratio—92 pages of Reiske; but with Reiske the oration has 107 pages, and this difference of 15 pages corresponds almost exactly to the 450 lines which are taken up by the documents in Reiske. Or, to state it differently, according to the ratio of 29 : 30, the oration, inclusive of the documents, should contain about 3200 στίχοι, whereas the number contained is stated to be only 2768. That the documents are found in Σ does not, of course, invalidate this argument, since it applies only to the original root-codex, from which this enumeration is supposed to originate. Nor would this result be materially different if we suppose with Blass, in Rhein. Museum, 24, that these στίχοι are not lines, but oratorical periods—κῶλα—since according to the figures above given, these κῶλα, if not individually of about the same length, must yet collectively have occupied about the same ground.

An invitation from Professor Brocklesby, acting President of Trinity College, to visit the College buildings and grounds, was accepted with thanks.

An invitation from the Faculty of the University of Mississippi, to hold the next meeting of the Association at Oxford, Miss., was referred to a committee (to be raised) on the time and the place of the next meeting.

The Association took a recess until 2½ o'clock P. M.

## Afternoon Session.

The first paper of the afternoon was read by Professor Charles Short, of Columbia College, New York, on "The History of the Vulgate, and the Characteristics of its Latinity."

The author traced the history of the Latin version from its probable origin in North Africa in the second century to its revision by St. Jerome in the fourth, its acknowledgment by Gregory the Great in the sixth, and its formal revision, sanction, and adoption by the Roman See in the sixteenth century. He then proceeded to give the results of a minute critical examination of about one-fourth of the Gospel of St. Matthew, using Dr. Tisdendorf's edition of the *Codex Amiatinus* of the sixth century, the purest form of St. Jerome's revision now known to us.

These results were given under the following heads and illustrated in most cases by all the examples occurring in the portion of St. Matthew above designated.

(1) The order of the original is exactly preserved by the Vulgate in most instances, with here and there an unavoidable departure, and sometimes a departure that might easily have been avoided.

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(2) Many of its renderings are very close in sense or form or both.

(3) Certain of its renderings are more or less inexact or faulty.

(4) Many of its renderings are in strict accordance with the Latin idiom, even when the Latin idiom differs from the Greek.

(5) It presents instances of judicious freedom in idiomatic translation.

(6) It not unfrequently renders the Greek literally in violation of the Latin idiom.

(7) Some of its words, forms, and phrases are in very unusual, but still authorized Latin.

(8) In its use of moods the Vulgate commonly observes the nicety of classic usage; but the infinitive is sometimes employed to denote purpose, as in Latin poetry; the subjunctive is in a few instances used without apparent reason after *quoniam* and *quia;* and in one case we have the indicative employed in an indirect question, as in the early and the late Latin poets.

(9) In the use of particles the Vulgate commonly conforms to classic rule even in delicate points, but some of its uses of particles are unusual and others are unexampled.

It is the author's purpose to examine in the same manner a part of the Acts and the Epistles, this portion of the New Testament, as is supposed, not having been revised at all by St. Jerome or only very cursorily, and to compare the results of such examination with the foregoing.

Professor W. D. Whitney, of Yale College, New Haven, Conn., read a paper on "The Proportional Elements of English Utterance."

If we are rightly to estimate the phonetic character of a language, it is necessary for us to know not only the sounds which compose its spoken alphabet, but also the comparative frequency of their occurrence. In order to determine this latter for the English language (according to my own natural pronunciation of it), I have made a selection of ten passages, five in prose and five in poetry, from as many different authors, and analyzed and enumerated the sounds occurring in them, until the number of 1000 sounds was reached in each; then, adding the ten numbers for each sound together, I obtained the proportional rate of occurrence of each in 10,000 sounds; which probably gives a fairly approximative average for the language in general.

The ten selected passages were as follows: 1. from Shakespeare's "Julius Cæsar," the beginning of Antony's speech over the body of Cæsar, 288 words; 2. from Milton's "Paradise Lost," the beginning, 274 words; 3. from Gray's "Elegy," the beginning, 272 words; 4. from Bryant's "Thanatopsis," the beginning, 283 words; 5. from Tennyson's "In Memoriam," of section lxxxiii., 284 words; 6. from King James's Bible version, of Psalm xxvii., 319 words; 7. from Dr. Johnson's "Rasselas," the beginning, 263 words; 8. from Goldsmith's "Vicar of Wakefield," the beginning, 269 words; 9. from Carlyle's "Sartor Resartus," book ii., ch. 8, eighth paragraph, 258 words; 10. from Macaulay's essay on Milton, part of the passage on the Puritans, 236 words.

The main results are given in the following table, which is so arranged that it may serve as a scale of frequency either for the whole alphabet or for the vowel and consonantal systems taken separately. The figures, if read without the decimal point, give the whole number of occurrences of each sound in the 10,000 sounds; the decimal point converts them into expressions of percentage. And as it is of interest to note the limits of variation in the rate of occurrence of each sound, there is added a column of *minima* and *maxima*, or of the least and the

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greatest number of occurrences found in any of the single passages of 1000 sounds; these also are converted into percentages by the decimal point.

SCALE AND RATE OF FREQUENCY OF ENGLISH SOUNDS.

| Consonants. | Vowels. | Per cent. | | Min. and Max. |
|---|---|---|---|---|
| *r* | | 7.44 | | 5.4 — 9.3 |
| *n* | | 6.76 | | 5.7 — 7.9 |
| *t* | | 5.93 | | 4.6 — 8.9 |
| | *ɪ̆* | | 5.90 | 4.7 — 7.4 |
| | *ə* | | 5.66 | 4.3 — 6.9 |
| *d* | | 4.94 | | 4.0 — 5.8 |
| *s* | | 4.69 | | 3.7 — 5.8 |
| *l* | | 3.84 | | 2.5 — 6.2 |
| *dh* | | 3.83 | | 2.4 — 5.1 |
| | *ĕ* | | 3.34 | 2.6 — 4.7 |
| | *æ* | | 3.32 | 2.4 — 4.0 |
| *m* | | 3.06 | | 1.8 — 4.1 |
| *z* | | 2.92 | | 2.2 — 4.3 |
| | *ɪ̄* | | 2.80 | 1.5 — 4.8 |
| | *ă* | | 2.59 | 1.8 — 4.2 |
| *v* | | 2.37 | | 1.4 — 3.5 |
| *h* | | 2.34 | | 1.2 — 3.1 |
| *w* | | 2.31 | | 1.6 — 3.0 |
| *k* | | 2.17 | | 1.1 — 3.1 |
| *f* | | 2.06 | | 1.2 — 2.8 |
| | *ū* | | 2.00 | 1.1 — 3.5 |
| | *ai* | | 1.91 | .9 — 4.8 |
| | *ɜ̄* | | 1.85 | .9 — 2.5 |
| | *ō* | | 1.76 | .9 — 2.6 |
| *p* | | 1.71 | | 1.0 — 2.6 |
| *b* | | 1.64 | | 1.0 — 3.4 |
| | *ē* | | 1.61 | .5 — 2.7 |
| | *ʌ* | | 1.54 | .8 — 2.2 |
| *sh* | | .86 | | .1 — 1.8 |
| | *au* | | .83 | .3 — 1.3 |
| *g* | | .79 | | .3 — 1.6 |
| *ng* | | .79 | | .1 — 1.4 |
| *y* | | .66 | | .3 — 1.1 |
| *th* | | .58 | | .2 — 1.0 |
| | *a* | | .56 | .1 — 1.2 |
| *ch* | | .53 | | .1 — 1.2 |
| | *ʌ̄* | | .47 | .0 — 1.4 |
| *j* | | .47 | | .1 — .9 |
| | *ŭ* | | .44 | .2 — 1.2 |
| | *l̥* | | .35 | .1 — .7 |
| | *n̥* | | .16 | .0 — .3 |
| | *ʌi* | | .12 | .0 — .2 |
| | *ŏ* | | .08 | .0 — .2 |
| *zh* | | .02 | | .0 — .1 |
| | | 62.71 | 37.29 | |

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In the scheme of representation here used, *a* denotes the open or "Italian" *a* of *far;* *ă*, the sound in *what, not* ("short *o*"); *ʌ*, that in *all, awe;* *æ*, that in *fat, man* ("short *a*"); *ᴁ*, that in *care, bear* (in my mouth, a lengthened *æ*, with transition-sound to following *r*); *ĕ*, the "short *e*" of *met, pen;* *ē*, the sound in *they, mate* ("long *a*"), a somewhat closer *e*-sound than *ĕ*, and having a vanish of *i* (*ee*); *ĭ*, the "short *i*" of *pin;* *ī*, the sound in *pique, meet* ("long *e*"); *ŏ*, the true short *o*-sound heard in New England in a few words, like *whole* and *home;* *ō*, the "long *o*" of *hole*, having a vanish of *u* (*oo*), as *ē* of *i;* *ŭ*, the true short *u*-sound of *pull, wool;* *ū*, the pure *oo*-sound of *rule, fool;* *ə*, the short "neutral vowel" sound in *but, son, blood;* *ᴚ*, the corresponding long, before *r*, as in *hurt, heard, herd, mirth, world;* *ai*, the diphthongal sound in *aisle, isle* ("long *i*"); *au*, that in *now, found;* *ʌi*, that in *boy, boil;* the *l* and *n* with subscript *o*, the consonantal vowels in unaccented final syllables like *apple* and *feeble, reckon* and *lessen*. As for the consonants, it is only necessary to explain that *th* denotes the surd sound in *thin*, and *dh* the sonant in *then; ng*, the palatal (or "guttural") nasal in *singing; sh*, the sibilant in *she, sure, nation; zh*, the corresponding sonant in *azure, occasion; ch* and *j*, the surd and sonant sounds in *church* and *judge*, which are compound, and might have been better treated here as such, being analyzable into *t-sh* and *d-zh*, only with a *t* and *d* formed farther back, more palatal, than our ordinary "dental" or lingual letters; if they are distinguished, it would be necessary also to distinguish the corresponding more palatal *n* of *inch* and *hinge* (it occurs 13 times in the 10,000 sounds).

In the number of occurrences given for *a* (of *far*) are included all such cases as *chance, pass, path, raft*, which I pronounce with the full "Italian" sound, knowing no compromise or intermediate whatever between this sound and the flat *a* of *fat* and *man;* if those classes be uttered with a somewhat flattened vowel, as is now very usual, and even enjoined by the orthoëpists, the percentage of *a* will be reduced almost to nothing. The short neutral *ə*, as given, includes the neutralized vowels of unaccented syllables (e. g. in *woman, distant, penal, nation, miller, presence*), and of enclitic words (like *the* and *a*), as judged and estimated from an ordinary reading style of utterance, neither affectedly distinct nor careless and slovenly. The percentage of *r* includes all the cases in which that letter is written; if, according to a habit which is widely prevalent both in this country and in England, the *r* be really uttered only when it has a vowel after it, the figure will be reduced to 3.74. Under *h* are counted the occurrences of that sound before the *w* and *y* sounds, as in *when* (*hwĕn*) and *hue* (*hyū*), where some hold that they pronounce instead only a surd *w* and a surd *y* before the vowel: the cases like *when* number 39 in the 10,000 sounds; those like *hue*, only 4. The "long *ū*" of *use, pure, cube*, etc., is analyzed and reckoned as *yū*, my own natural pronunciation recognizing no intermediate between this and a pure *ū* (*oo*).*

The table shows that the average proportion of vowels to consonants in English is 37.3 to 62.7 (the minimum and maximum of vowels are 35.7 and 39.6). This is just about the same as in German, a little less than in Swedish (38.3) or French (about 40), yet less than in Gothic (41), Sanskrit (42), Latin (44), or Greek (46). The average number of consonants to a syllable, then, is 1.682. The whole number of words in the ten passages being 2746, the average number of sounds

* For other details, which cannot well be included here, of the definition and estimate of the various sounds, reference may be made to the author's paper on "The Elements of English Pronunciation," in the second volume of his "Oriental and Linguistic Studies," published in the autumn of 1874.

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to a word is 3.642; that of syllables to a word is 1.358: that is to say, there is a second syllable for only about one word in four: the actual number of monosyllables in all the passages is 2028, or 73.8 per cent.; of dissyllables 510, or 18.6 per cent.; of trisyllables, 146, or 5.3 per cent.; and the words of four syllables are 50; of five syllables, 11; of six syllables, 1.

It may be worth while to make a few more general combinations and comparisons. First, the vowels may be classified as follows:

| | | | |
|---|---|---|---|
| Palatal (*æ, e, i*), | 17.44 | Openest (*a*), | .56 |
| Labial (*ʌ, o, u*), | 8.41 | Next degree (*æ, ă, ʌ*), | 7.92 |
| Lingual (*l̥, n̥*), | .51 | Medial (*e, o*), | 6.79 |
| Neutral (*a, ə*), | 8.07 | Closest (*i, u, ə*), | 18.65 |
| Diphthongs, | 2.86 | | |

The consonants, classified according to articulating organs, are as follows:

| | |
|---|---|
| Palatal, | 6.29 |
| Labial, | 13.15 |
| Lingual, | 40.93 |
| Neutral (*h*), | 2.34 |

According to degree of closeness or openness, they are:

| | | |
|---|---|---|
| Mutes | (sonant 7.84, surd 10.34), | 18.18 |
| Spirants | (sonant 6.20, surd 2.64), | 8.84 |
| Sibilants | (sonant 3.41, surd 6.08), | 9.49 |
| Nasals, | | 10.61 |
| Semivowels, | | 14.25 |
| Aspiration, | | 2.34 |

Finally, comparing the surd and sonant elements, we have—

| | Of pairs of Cons. | Of all Cons. | Of whole Alphabet. |
|---|---|---|---|
| Surds, | 18.53 | 20.87 | 20.87 |
| Sonants, | 16.98 | 41.84 | 79.13 |

Dr. J. Hammond Trumbull, of Hartford, Conn., next read a paper on "Numerals in American Indian Languages, and the Indian Mode of Counting."

No exception has been found in American Indian languages to Grimm's dictum that "all numerals are derived from the fingers." The greater number of the Indian nations of North America adopted a *decimal* system—counting the fingers of both hands. Some tribes, however, did not advance beyond a *quinary* system, and a few were poorer even than this. The Abipones of Paraguay, we are told, could not count beyond four, giving to that number a name meaning "the ostrich's toes" (i. e. three and one). Other nations, particularly the Mexican and Central American, counted by *twenties* instead of tens or fives, reckoning *toes* as well as fingers, for the base of a numeral system. The Tule Indians of Darien (a vocabulary of whose language was printed in last year's Transactions) adopt this mode of counting: "twenty" being named "one man"; 100, "five men," and so on. A general view of these vigesimal systems was given by Mr. Gallatin in 1845 (Transactions of the American Ethnological Society, vol. i.), and was incorporated by A. F. Pott in his *Zahlmethode.*

Admitting the derivation of numerals from the fingers, the question In what order are the fingers counted? becomes a necessary preliminary to the analysis of any numeral series. Which finger represents *one?* Is it the little finger, or—



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as in the counting of deaf mutes—the *thumb?* And when going from "five" to "six," that is, from one hand to the other, is the sequence from finger to finger, thumb to thumb, or thumb to finger?

The only answer given by Gallatin or Pott to such questions relates to the Eskimo numerals. We learn from other sources that nearly all American nations follow the same order as the Eskimos: namely, they count the little finger (usually of the left hand) *one*, the next finger *two*, and so on to the thumb, which is *five;* the thumb of the other hand is *six*, and *ten* falls on the little finger of that hand. Each finger as it is counted is bent down.

Whether an Indian marks *one* by his little finger or his thumb may seem of small importance to philology; but it is one of the thousand questions which a philologist must answer before becoming qualified to discuss the subject of Mr. Robert Ellis's volume "On Numerals as Signs of Primeval Unity among Mankind" (London, 1873). This writer presents, as "results of primeval affinity—indications of unity of origin in human speech and probably in the human race"—a number of presumed "coincidences, affecting not only numerals but also the names of members of the body from which those numerals are derived, in languages far removed from each other," and he finds many of these coincidences among Indian languages of America. He detects resemblances between names for "hand," "finger," "five," etc., in the Indian and in the Basque, the original Aryan, and some African languages. Even the much-vexed dice of Toscanella are made to show the likeness of an Etruscan *one* to a Comanche *hand* and an African *finger*.

After brief notice of Mr. Ellis's ingenious volume, the writer proceeded to offer some observations on the etymology of Indian numerals, and on the relations of names for numbers to the several fingers by which the numbers are designated. The little-finger, which stands for *one*, is called by some nations "the youngest son of the hand"; by others, "the little one," "the last born," etc. *Paysuk*, the Massachusetts name for *one* (*bezhik* in Chippeway) means "the little one." *Wanzhidan*, the Sioux *one*, probably means "the little (finger) bent down," as it is in counting *one*. The fourth or ring finger is nameless in many languages. The Indians often designate it as "next to the little" or "next to the middle" finger. It marks—but rarely if ever gives a name to—*two*. Some names for *two* seem to have been derived from roots meaning "to couple," "to double," or the like. Such roots must be of earlier origin than any formal arithmetical system. The dual is older than the plural. From these same roots come names of natural *pairs*, so that in many languages we find a likeness to *two* in the names of "hands," "arms," "feet," "eyes," etc. Names of *artificial* pairs—moccasins, leggings, mittens, etc.—sometimes come by later derivation from the same roots, or from the numeral *two*. In all the Algonkin languages, in the Dakota, and in some others, *two* and *hands* are very nearly related—the name for hand being derived in many of these languages from a root meaning "taking hold." The hand is the "holder" or the "seizer." The *middle* finger is so named in almost all languages, and in many it gives this name to the numeral *three*. *Eight*, which falls on the same finger of the other hand, is often named "the other three," "three again," or the like. The *forefinger* is the "index" or "pointer," as it has been in many languages of the eastern continent. It marks *four*, and names for *four* are often derived from it or from the action of "showing" or "pointing at." In the Massachusetts language *yau*, "four," is nearly identical with *yeu*, "this, that, here." The *thumb* does not often give names to the *five* and *six* which are counted on it. It is called by the Algonkins, "greatest finger"; by the

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Dakotas, "parent (or eldest brother) of the hand"; by the Choctaws, "hand's mother," etc. *Five*, that is, *one hand*, is variously named, as "a half" (i. e. of *ten*), "one side," "a stopping place," "all together," "half way," etc.

The other numerals, to *ten*, were similarly discussed, with illustrations from various Indian languages.

A recess was then taken until 8 o'clock.

## Evening Session.

On re-assembling, the Secretary reported the following names of new members:

Mr. L. A. Sherman, New Haven, Conn.; Mr. M. C. Stebbins, Principal of High School, Springfield, Mass.; Professor C. T. Winchester, Wesleyan University, Middletown, Conn.; Professor John H. Wright, Ohio Agricultural and Mechanical College, Columbus, Ohio.

Rev. Carl W. Ernst, of Providence, R. I., presented a paper on "The Pronunciation of German Vowels."

It was attempted to arrange these sounds scientifically and in the form of a table, the *fundamentum divisionis* being physiology rather than history or merely anatomy. The question when or where in a word certain vowel-sounds occur, it was stated, can be determined only after an analysis of the vowels, and when the laws of accentuation are defined. The vowels, for the present purpose, were explained genetically as the *voice uninterrupted*, consonants being vocalized or unvocalized breath checked by the tongue or teeth or lips. German vowels are simple or mixed; mixed or diphthongs when consisting of two sounds most intimately united. The *simple* vowels were divided, as to quality, into eight *long* sounds (a, e, i, o, u, ä, ö, ü), and seven *sharp* sounds (a, e, i, o, u, ö, ü), the term sharp differing from short, and being equivalent rather to abrupt. These sharp vowels are not long vowels abbreviated, but differ from them materially, and are pronounced farther back in the mouth and with the tongue lowered. As to quantity or time of utterance the vowels were divided into eight long vowels (the same as above), and ten short vowels (the sharp vowels and the three diphthongs), short merely meaning that they occupy little time and about one-fourth of the time occupied by the long vowels. The language has three *diphthongs:* au, ai (also spelled ei, ey, ay), oi (also spelled eu, äu, æu), which are always short and present to the ear the rapid transition from a sharp to a long vowel. The term *open* was used of the distance between the vocal chords, which is greatest, or as great as taste and ease of individual elocution will permit, in u, gradually diminishing through o, a, and e, it being smallest in i; i is therefore the "closest" vowel in German, and requires the least emission of breath. The aperture of the lips, horizontally and perpendicularly, is greatest in a, growing systematically less in e, i, o, and is as slight as possible in u. The lips protrude most in u, less in o, their position is normal in a, they are pressed gently against the teeth in e, and rather strongly in i. The larynx correspondingly rises in i, less in e, its position is normal in a, below this in o and u. The vowels ä, ö, ü, are pronounced like a, o, u; only the vocal chords and the larynx have the same position as in i. The relation between the vowels approaches mathematical accuracy.

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Col. T. W. Higginson, of Newport, R. I., next read a paper on the word "Philanthropy."

It has been said that there is more to be learned from language itself than from all that has been written by its aid. It is possible to reconstruct some part of the moral attitude of a race through a word of its language. This paper may illustrate such a process.

When a word comes into existence, its meaning is carved on the language which holds it. If you find the name of a certain virtue in any tongue, the race which framed that language knew that virtue. The word Philanthropy is a modern word in the English language. The Pilgrim Fathers may have practised what the word meant, but few among them had heard the word, perhaps none had used it. It is not in the writings of Chaucer or Spenser or Shakespeare, nor even in the authorized version of the English Bible, first published in 1611. The corresponding Greek word, occurring three times in the original, is each time translated by a circumlocution. The word Philanthropy does not appear in the pioneer English Dictionary—Minsheu's Guide to the Tongues, first published in 1617, nor in the Spanish Dictionary of the same Minsheu, in 1623. But two years later, in the second edition of the Guide to the Tongues, it appears as follows, among the new words distinguished by †; "Philanthropie: Humanitie, a loving of men:" and then follow the Greek and Latin words as sources of derivation.

This is its first appearance as an English word. But Lord Bacon, publishing in the same year (1625) his essay on Goodness and Goodness of Heart, uses the original word as follows: "I take goodness in this sense, the affecting of the weal of men, which is that the Grecians call *Philanthropia;* and the word Humanity (as it is used) is a little too light to express it."

The next author who uses this word is Jeremy Taylor. In his Holy Dying, (published 1651), he translates the Greek word *φιλάνθρωπος* "a lover of mankind," but in his Sermons, published a year later, though perhaps preached earlier, he uses the English word, the phrase being "that godlike excellency, a philanthropy and love to all mankind;" and again, "the philanthropy of God." The word took root slowly. In 1693, in a preface to Sir H. Steere's version of Polybius, Dryden used it with an apology, thus: "This philanthropy, which we have not a proper word in English to express."

Three leading writers of their century—Bacon, Taylor, and Dryden,—thus furnish the milestones that mark the entry of the word philanthropy into our language. Doubtless the reason of its use is correctly stated by Dryden; it was needed.

The Greek word *φιλανθρωπία* gave the avowed key-note for the greatest drama preserved to us and also for the sublimest life of Greece. It seems to have been first used by Epicharmus, who was born about 540 B. C. Its first important use was in the Prometheus Bound of Aeschylus, probably represented about 460 B. C. The vengeance of Zeus has fallen upon Prometheus for his love of man; he is to be bound to the desert rock for his philanthropy, *φιλανθρώπου τρόπου* (lines 11, 28). In the most magnificent soliloquy in ancient literature, Prometheus accepts the charge and glories in his offense; he admits that he has conveyed the sacred fire of Zeus to men, and thereby saved them from destruction. The philanthropic man is exhibited under torment for his devotion, but refusing to regret what he has done. There is no play in modern literature which turns so entirely on the word and the thing, philanthropy.

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In the Euthyphron of Plato (§ 3), Socrates uses the word thus, replying to an opponent (Jowett's translation): "I dare say that you don't make yourself common and are not apt to impart your wisdom. But I have a benevolent habit (*ἀπὸ φιλανθρωπίας*) of pouring myself out to everybody, and would even pay for a listener, and I am afraid the Athenians know this."

Coming down to later authors, we find the use of the word in Greek to be always that for which it was imported into English. How apt we are to say that the Greeks thought only of the state, not of individuals, nor of the world outside! Yet Isocrates heaps praises on a man for being *φιλάνθρωπος καὶ φιλαθήναιος καὶ φιλόσοφος*. Demosthenes uses *φιλανθρωπία* in contrast to *φθόνος* and to *ὠμότης*, and speaks of employing philanthropy towards any one, *φιλανθρωπίαν τινὶ χρῆσθαι*. Xenophon makes Cyrus describe himself on his death bed as *φιλάνθρωπος*, and Plutarch sums up the praises of a youth by the same epithet, in the passage translated by Jeremy Taylor. Plutarch also, in his Life of Solon, employs the word *φιλανθρώπευμα*, a philanthropic act. Epictetus (Fragm. 46) says that nothing is nobler than *φιλανθρωπία*. Diodorus speaks of a desert country as *ἐστερημένη πάσης φιλανθρωπίας*—destitute of all philanthropy, or, as we might say, "pitiless."

We have then a virtue thus named, which dates back within about two centuries of the beginning of authentic history. Some of the uses of the word have almost disappeared; such as its application to Deity. Aristophanes (Peace, 394) applies it thus to Hermes: *Ὦ φιλανθρωπότατε*; and Paul uses it similarly in Titus iii. 4. Athanasius uses it as a complimentary form of address, *Ἡ σὴ φιλανθρωπία*, as Englishmen might say "your grace" or "your clemency" to a titled person, and even Americans say "your honor" to dignitaries. In modern literature Jeremy Taylor, Barrow, and Young use the word in application to the Deity, but this is now rarely heard. With the Greeks, the word did duty in the double sense of "the fatherhood of God and the brotherhood of man."

It is hardly just in Max Müller to say that "humanity is a word for which you look in vain in Plato or Aristotle" without alluding to this history of the elder word. Even the omission of the word and thought in Aristotle was criticized before Max Müller by Plutarch, who says (in his "Fortune of Alexander") that Aristotle advised Alexander to treat the Greeks as friends and kinsmen, but the barbarians only as animals or chattels; but that Alexander wished that all should regard the whole world as their common country, the good as fellow-citizens, the bad only as foreigners—that every good man should be esteemed a Hellene, every evil man a barbarian. The Stoics are represented as teaching that we should look upon all men in general as our fellow countrymen. The Pythagoreans, five centuries before our era, taught the love of all to all. Menander said: "To live is not to live for one's self alone; let us help one another." Epictetus maintained that "the universe is one great city full of beloved ones, divine and human, endeared to each other." The same chain of thought was continued down through the Latin writers. Terence, Cicero, Quintilian, and Juvenal may be cited to similar effect.

It is a remarkable fact that the word "philautie" for "self-love" from the Greek *φιλαυτία*, was introduced by Minsheu, at the same time with "philanthropie," and was used by Holinshed and by Beaumont and Fletcher, but is now obsolete. The bad word died of itself, but the good word took root and flourished.

Our debt to the Greek race is not merely scientific or æsthetic, but in some

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degree moral and spiritual as well. However vast may be the spread of philanthropy in Christendom, we should give the Greek race some credit for the spirit, since at all events we must give them full credit for the word.

On motion, Professor Whitney, Mr. Buckingham, Professor Seymour, Professor Young, and Professor Haldeman were appointed a committee to nominate officers and members of the Executive Committee for the ensuing year.

On motion, Dr. Trumbull, Col. Higginson, Professor W. F. Allen, Professor Comfort, and Professor Tyler were appointed a committee to recommend a place and a day for the next meeting of the Association.

The Association stood adjourned to 9 o'clock A. M.

### Thursday, July 16—Morning Session.

At the opening of the morning session, Professor Albert Harkness, of Brown University, Providence, R. I., read a paper on "The Formation of the Tenses for Completed Action in the Latin Finite Verb."

The Latin, Greek, and Sanskrit undoubtedly inherited, from the mother tongue of the Indo-European Family, the power to express completed action by means of *reduplication*, and to create new tense-forms through the help of *auxiliary verbs*. The Latin is, however, distinguished from the Greek and Sanskrit by a freer use of compound tense-forms to supply the place of the reduplication. Indeed, in all tenses for completed action, except the perfect, compound forms alone are used. In *cecineram*, for instance, we at once recognize the modified stem *cin*, which gives the general meaning of the verb, the reduplication *ce*, which denotes completed action, and the auxiliary *eram*, which adds the idea of past time. We have, therefore, an expression for completed action in past time. But the analysis of *cecinissem* is more difficult. Corssen explains it as compounded of *cecin*, ĭ, and *ssem*, but he does not show the origin or the use of the long *i*, a fact which renders his explanation comparatively worthless. But *cecinissem* may be formed from *cecin* and *essem*, originally *esem*, which became *isem* in compounds, as *cecinisem;* then by a subsequent doubling of the *s*, *esem* became *essem*, and *isem* in compounds *issem;* hence *cecin-issem*.

But the great difficulty to be removed is found in the endings of the Perfect, *i*, *isti*, *it*, *imus*, *istis*, *erunt* or *ere*. These endings present peculiarities which have never been explained. Bopp's labored effort to bring the Latin Perfect into some sort of harmony with Sanskrit aorist forms has proved a complete failure. Schleicher's attempted explanation is admitted by the learned author himself to be incomplete, and is in the main rejected by Corssen, while the views expressed by Corssen himself upon the general subject of the formation of the Latin Perfect fall far short of meeting the real difficulty.

It is evident that the problem before us can be solved only by some new method; and numerous facts in the language suggest the inquiry whether some different

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treatment of the auxiliaries, *esi* and *fui*, which are used in the formation of Perfects in *ui*, *vi*, and *si*, may not give us the key to the true explanation of these remarkable forms. No one has ever traced *esi* back to its original form. It corresponds to the Sanskrit *āsa*, but *āsa* itself is not an original formation, but has been contracted from *asasa* or *asasma*. After the analogy of the original Sanskrit, the corresponding Latin stem *es*, seen in *sum*, *esse*, would give *esismi* inflected thus:

| | | | | |
|---|---|---|---|---|
| esismi | = | esimi | = | esi. |
| esisti | = | | | esisti. |
| esisti | = | esist | = | esit. |
| esismus | = | | | esimus. |
| esistis | = | | | esistis. |
| esisunt | = | esirunt | = | eserunt. |

The various changes by which *esismi*, *esisti*, etc., become *esi*, *esisti*, etc., are readily explained. The auxiliary thus assumes the exact form in which it appears in Perfects in *si* and *xi*, as *carp-esi*, *carpsi*, *carpsisti*, *carpsit*, etc.

The same treatment of *fui* from *fuismi*, compounded of *fu* and *es* gives the exact endings of Perfects in *ui* and *vi*, as *alui*, *amavi*, etc.

The discussion leads to the following conclusions:

1. The tense sign of the Latin Perfect in all verbs is the reduplication or its equivalent. In compound forms in *ui*, *vi*, and *si*, it is seen in the auxiliary, which is formed either by reduplicating the stem *es* or by combining it with its equivalent *fu*.

2. The peculiarities of the Latin Perfect—the final long *i*, *s* in the first syllable of *isti*, *istis*, and finally the endings *erunt* and *ere*—are the direct result of the reduplication of *es* or of its combination with *fu*. These peculiarities are readily explained without doing violence to any known law of the language, and without requiring the insertion of a single letter, even of a connecting vowel. Moreover not a single element in any of these forms sustains any important loss.

The second paper of the morning was read by Professor Gustavus Fischer, of Rutgers College, New Brunswick, N. J., on "The Present Condition of Latin Grammar."

The science of Latin grammar has not kept pace in our day with other sciences. In almost every part of syntax, the present condition of grammatical science is exceedingly defective. The grammars leave us without an answer just when they ought to answer; they often answer just when it is not worth while to ask a question. The time has come when we should apply the microscope to the study of language. True philology is one of the natural sciences, and accurate and minute observation is no less necessary in it than in any other of them. Philology, indeed, deals with the mind; we may call it a physiology, but at the same time a history of the mind. We have already begun to apply this microscopic investigation to the origin of words; it remains now to apply it to Latin syntax in the same manner as many members of this Association have successfully applied it to some parts of Greek grammar. Such a treatment of Latin grammar would be essentially historical, carefully separating the different epochs, and always beginning with the oldest writers in which a given syntactical form occurs.

One of the examples adduced was the use of the subjunctive with *sunt qui*, *est qui*, etc. (for instance, "sunt qui dicant," "there are persons who say"), in classi-

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cal prose. There is no Aryan language, except the Latin, in which such a subjunctive occurs. Some grammarians are altogether silent on the reasons for the use of this subjunctive. Others explain *sunt qui dicant* by *sunt homines tales ut dicant*. But this is evidently erroneous. For, aside from the fact that this construction is frequently used when definite and particular statements without any reference to "kind" are assigned to persons, such modal *ut*-clauses never occur in this connection, and hence the clauses with *qui* could not possibly be *substitutes* for modal *ut*-clauses. Haase considers this subjunctive a *linguistic necessity*, because, he says, the predication is contained in the principal sentence, and hence the use of another indicative for the same predication in the relative clause would be a linguistic pleonasm. This peculiar idiom can only be explained *historically*. Happily we have the first beginning of this usage before our eyes. We find that neither Plautus nor Terence ever uses a subjunctive in this construction, although the construction itself not rarely occurs in these writers, as: "*Sunt quos scio* esse amicos;" "*sunt quorum* ingenia atque animos non *queo* noscere." In Cato and Lucretius the construction does not occur. In Varro it is found six times, and only once with a subjunctive, which is not owing to the *sunt qui*. Varro's contemporary, Cicero, was the first who used *sunt qui* with a subjunctive, and so frequently that it will be difficult to count the passages. While Cicero uses the subjunctive in this construction (say) 200 times, the indicative occurs only in two or three authenticated passages, although if *sunt qui* or *est qui* is qualified by the addition of *multi*, *quidam*, or similar adjuncts, the passages with the indicative are a little more numerous. Caesar and Sallust use the construction a few times, and oftener with the subjunctive than with the indicative. Livy uses the construction oftener than all classical writers together, and always with the *subjunctive*. The poets of the classical period almost always use the *indicative*. The writers of the silver age follow the use of Cicero and Livy, though in Seneca four or five times the indicative occurs. Hence it is evident that the subjunctive in this construction had its origin in the time of Cicero, and was probably introduced by Cicero himself. On the other hand, we find that even in the classical writers the subjunctive is always used if the principal sentence is negative or contingent. But this negative or potential subjunctive has a considerably wider range than with *sunt qui*, although our grammars do not enumerate this class of subjunctives (which I call "*the subjunctive of non-reality*") among the "general" instances of subjunctives.

The subjunctive of non-reality occurs if the principal sentence is negative (and generally also if it is potential or contingent), and if this negative in the principal sentence makes the dependent clause virtually negative, although it has an affirmative form. Even in clauses introduced by the Latin equivalents of "that," the language does not generally use the regular form of an accusative with the infinitive, preferring a clause with *ut*, in order to designate an action as having no reality (while it has an affirmative *form*), since this form alone admits the introduction of a *subjunctive*. It seems evident that the very frequent uses of subjunctives of non-reality in the construction *sunt qui*, etc. (as "nemo est qui dubitet," etc.), caused the use of a subjunctive in the relative clause even when the principal sentence was not negative. Hence we must consider this subjunctive as resting upon a mere conventional usage, and as having arisen from a false analogy of those constructions in which the subjunctive expresses the idea of non-reality.

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Mr. C. D. Morris, of Peekskill, N. Y., read the next paper, on "The Age of Xenophon at the Time of the Anabasis."

The object of the paper was to show that there are many improbabilities attending the supposition that Xenophon was born B. C. 444, and was consequently forty-three years old at the time of the Anabasis, which has been taken for established since the publication of K. W. Krüger's tract in 1822; and that therefore we must discredit the story, on which alone that supposition rests, that the life of Xenophon was saved by Socrates at the battle of Delium, B. C. 424. This story is found only in Strabo (cir. B. C. 10) and in Diogenes Laërtius (cir. A. D. 200), and it is, therefore, a legitimate object of criticism. It was judged to be antecedently incredible (1) as being inconsistent with the narrative of Plutarch in his life of Alcibiades; (2) because, if Xenophon was of military age at the battle of Delium, it is hardly possible that he, with all his practical efficiency, should have had nothing to do with the subsequent events of the Peloponnesian war; (3) on the ground that, if Xenophon owed his life to Socrates, he would surely have alluded to the fact, if not in his other writings, certainly in the Memorabilia; (4) because he had at least four of his works in hand considerably after the battle of Mantinea, B. C. 362, at which time he must have been over eighty-two years old. But the strongest reason for discrediting the story is the impossibility of giving a natural interpretation to several passages in the Anabasis except on the hypothesis that Xenophon was quite a young man at the time, probably not over twenty-five years old. When we remember that Proxenus was only thirty at the time of his death, Agias and Socrates about thirty-five, and Menon certainly considerably younger, we must feel that Xenophon, when meditating on the expediency of putting himself forward, could not, if he were over forty, have seemed to himself too young for a general's responsibility, and therefore could not have said to himself (iii. 1, 14), *ποίαν ἡλικίαν ἐμαυτῷ ἐλθεῖν ἀναμένω; οὐ γὰρ ἐγωγ' ἔτι πρεσβύτερος ἔσομαι, ἐὰν τήμερον προδῶ ἐμαυτὸν τοῖς πολεμίοις*: nor could he have said to the captains of Proxenus, who were in the habit of yielding obedience to a man of thirty (iii. 1, 25), *κἀγὼ δέ, εἰ μὲν ὑμεῖς ἐθέλετε ἐξορμᾶν ἐπὶ ταῦτα, ἕπεσθαι ὑμῖν βούλομαι· εἰ δ' ὑμεῖς τάττετέ με ἡγεῖσθαι οὐδὲν προφασίζομαι τὴν ἡλικίαν, ἀλλὰ καὶ ἀκμάζειν ἡγοῦμαι ἐρύκειν ἀπ' ἐμαυτοῦ τὰ κακά.* Similar indications of an age at least under thirty are found in iii. 2, 37; iii. 4, 42; v. 3, 1; vi. 4, 25; vi. 5, 4; and the frequent allusions to others as *πρεσβύτεροι* or *πρεσβύτατοι* are themselves indications of comparative youth on the part of the person who makes them. The only passage in the Anabasis (vii. 2, 38) which has been thought to indicate greater maturity, viz., that in which Seuthes proposes to buy Xenophon's daughter, if he had any (*εἴ τις σοὶ ἔστι θυγάτηρ*), is of no weight, as we know nothing of Xenophon's looks; and probably Seuthes may have made this offer, as he did all the rest of his offers, without any thought of the probability of his fulfilling it. In conclusion, it was insisted, after Grote, that the objection to reposing confidence in one so young as Xenophon was, which would naturally present itself to the soldiers, would be readily lost sight of in view of the remarkable capacity he exhibited to think, speak, and act with equal efficiency, which was the result of his Athenian training.



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Professor W. D. Whitney, of Yale College, New Haven, Conn., read a paper on "The Relation of Vowels and Consonants, and certain Inferences from it."

The special characteristic of human speech is, that it is *articulate*. This means in reality what is literally expressed by the name. Our speech is broken into *articuli*, or joints, and is thus made both intelligible and flexible; and the joints are the syllables. A language of mere tone-sounds, shading and varying into one another without marked divisions, would be a sing-song; a language of mutes and fricatives, of explosions and buzzes, would be a splutter: both alike would be wanting in the availability for abundant and distinct expression which belong to our present utterance. The articulated or syllabic effect is capable of being given in various ways: least perfectly, by mere change from one vowel to another; distinctly enough, by a hiatus between vowels, or repetitions of the same vowel; but most effectively, and in the practical use of speech prévailingly, by the intervention of closer sounds, or consonants, between the opener sounds, or vowels. For example, *a* may be prolonged indefinitely as only one syllable; but divide its continuity with a consonant, as in *apa*, *ala*, and the effect is dissyllabic.

This brings to light the essential distinction of vowel and consonant: the one is an opener sound, with the element of tone or material prevalent; the other is a closer sound, with the element of oral modification, or of form, prevalent. All the current definitions of the two classes, so far as they are true and tenable, are founded upon and imply this. If, in the light of this description, all vowels were equally vocalic, and all consonants equally consonantal, there would be reason for treating the two classes separately, as independent systems. But this is not the case. There are series leading, by successive degrees of the same oral modification, clear through the alphabet, from the openest vowel to the closest consonant: such, for instance, is *a*, *æ*, *e*, *i*, *y*, *gh-kh*, *g-k*.*

Along these series, the two classes shade into one another, with a class of sounds near the division-line—especially *l*, *r*, *n*—which are capable of serving either office. And so the closest vowels, ī (*pique*) and ū (*rule*), are capable of passing, with no difference of articulate form, but only of quantity and stress, into the consonants (semivowels) *y* and *w*. The openest vowels are vowels only; the closest consonants are consonants only; but there is an intermediate domain, of doubtful and changeable character. Thus, in *lap* we have a central openest sound, to which the less open *l* and the yet closer *p* are felt only as accessories; in *alp* we have a transition from openest to closest through an intermediate degree, in *pla* the contrary, and it is still a single syllable; but arrange the same sounds in the order *apl* (i. e. *apple*), and the word is dissyllabic, because there are two sounds of sufficient openness separated by a closer.

The principles of syllabication may be graphically illustrated (as was done by the speaker, upon the blackboard), by representing the stream of opener vocalic utterance, with the constrictions and separations (effected by fricatives and mutes, etc.) dividing it into parts or joints.

The truest and best physical scheme of the alphabet is one which illustrates this relation of vowel and consonant by arranging all sounds between the openest of them all, the *a* of *far*, and the three closest, the mutes *k*, *t*, *p*, in classes accord-

* The vowel-signs are used as in the author's previous paper (above, page 16), and *gh-kh* represent the fricatives lying nearest to *g-k*, or the German *ch*-sound and its corresponding sonant

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ing to their degree of closeness or openiness of the articulating organs, and in lines (approximately) according to the organs used in forming them; or somewhat as follows:

| | palatal series. | | lingual series. | | labial series. | |
|---|---|---|---|---|---|---|
| sonant. | | | *a* | | | vowels. |
| | | | *æ* *A* | | | |
| | | *e* | | *o* | | |
| | | *i* | *ə* | *u* | | |
| | *y* | | *r*, *l* | | *w* | semivowels. |
| | *ng* | | *n* | | *m* | nasals. |
| surd. | *h* | | | | | aspiration. |
| sonant. | *zh* | | *z* | | | sibilants. |
| surd. | *sh* | | *s* | | | |
| sonant. | | | *dh* | | *v* | spirants. |
| surd. | | | *th* | | *f* | |
| sonant. | *g* | | *d* | | *b* | mutes. |
| surd. | *k* | | *t* | | *p* | |

In this scheme, the nasals are put next the semivowels, because, though in one sense contact-letters, mutes, they are in another respect a class of sounds in a high degree open, sonorous, and continuable; and because they share with the vowels and semivowels the possession of a common surd, the "aspiration" *h*, which accordingly finds its proper place as such.

This arrangement is of value also as casting light upon the historical development of the alphabet. In the earliest Indo-European language, the greatly predominant sounds were the extreme ones, *a* and the mutes; and the alphabet has ever since been filling up more and more with intermediate articulations. Of the fricatives (sibilants and spirants together) only the *s* is a primitive Indo-European letter. The same is true of the vowel-system; its extremes, the *a*, *i*, and *u*, are alone original. This filling up is not because the intermediate sounds are, in themselves and absolutely, easier of utterance; they are rather the contrary; they are harder for the child to learn to produce, and less frequently met with in the sum of human speech. But in the rapid transitions of fluent utterance, from vowel to consonant and consonant to vowel, there is less expenditure of force in passing back and forth between sounds of medial character; the organs find this art (unconsciously, of course) by experience, and alter the sounds of extreme into those of medial closeness. Hence there is a constant general movement from the two ends of the alphabet toward its middle, an assimilation, as it were, of the two great classes to one another: the vowels become closer or more consonantal; the consonants become opener or more vocalic. The articulated emission of sound assumes a different character: its general breadth and fullness (as depending on the vowels) are reduced or contracted; and the articulating elements, the consonants that break it into joints, are of less incisive character and of inferior dividing effect. This thinning process has gone a great way in English. The facts most strikingly illustrating it are that the open *a* of *far*, which once formed full 30 per cent. of Indo-European utterance, has sunk with us to a half of one per cent., while the two close vowels *i* and *ə* (the neutral sound in *but* and *burn*)

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make over 16 per cent.; and the fricatives have become more numerous than the mutes. This is, in its way and degree, a degeneration of the phonetic form of language; we may hope that it will not go enough farther to degrade seriously the character of our speech.

A recess was taken until afternoon.

AFTERNOON SESSION.

The Secretary reported the election of new members:

Mr. John C. Bull, American Asylum, Hartford, Conn.; Dr. D. J. Pratt, Assistant Secretary of the Board of Regents, Albany, N. Y.; Mr. J. W. Schermerhorn, New York.

Professor C. H. Brigham, of Ann Arbor, Mich., exhibited a Siamese manuscript.

This manuscript is on black pasteboard, twenty feet long and thirteen inches broad, with writing on both sides. The letters, one-third of an inch long, are painted in yellow color. The words read from left to right. The lines are divided, and judging by the similarity of sound in the endings, there is rhyme as well as poetry in them. The subjects on the opposite sides of the manuscript seem to be different. A reasonable conjecture is that it contains two Siamese poems. The manuscript was brought from the East Indies many years ago by a gentleman since deceased, who gave no information how or when he obtained it.

Professor J. M. Van Benschoten, of Wesleyan University, Middletown, Conn., read a paper on "Troy and Dr. Schliemann's Discoveries."

The paper was illustrated by diagrams and a large collection of photographs, and was based in part on the author's own investigations as to the geography of Ilium and the work which has just been carried on there. Dr. Schliemann's labors were carefully described, and his wife's assistance in them was commended. Part of the paper was devoted to an examination of the geographical knowledge of Homer. The general results were summed up somewhat as follows:

What has Schliemann discovered? Manifestly a city of very ancient date. Whether it be Troy or not is another question, the answer to which awaits further exploration and discovery immediately at Hissalik and the Greek camp at Mycenae and Argos and other countries of ancient civilization. Of the existence of an actual Troy there can hardly be a question any longer. Egyptologists have established beyond a reasonable doubt what concurrent tradition had long tried to settle. As to the age of these ruins of Hissalik there is and will be diversity of opinion. It will require more years to capture this question than Agamemnon spent in taking Troy. History never had such a problem to solve before; accepted theories of chronological sequence have broken down. A very few facts sum up ancient history. Save what concerns the Egyptians and the Hebrews we know next to nothing of the ancient world. We amuse ourselves with the terms pre-historic, pre-hellenic, etc., terms as vague as anything can well be. Schliemann's stone stratum succeeds his bronze stratum. I think it reasonable to conclude that the stone and the bronze age are not necessarily a mark of

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great antiquity, neither is the order of superposition a law. The same line of statement may with some limitation be made with regard to pottery. It is a common opinion that rude pottery, rude in texture and execution, is a certain index of a rude civilization. Not so. In historical periods undoubtedly there are certain well-settled marks of age and nationality. As to the Hissalik pottery, to much of it a high antiquity may perhaps reasonably be assigned. The rude grafiti or scrawls on Schliemann's terracottas, at one time so unpromising, are just now attracting the profoundest interest. As to his γλαυκῶπις Ἀθήνη, when he shall have made good his promise—or threat—to dig out at Mycenae a βοῶπις Ἀθήνη, we Greek schoolmasters will review and revise our Homer and read "owl-eyed Athena." This indicates Schliemann's weakness. He is so exacting that the most insignificant object, be it what it may, which his spade throws out of the hill of Hissalik *must* be connected with some Homeric name, and he is so credulous that he *believes* it to be thus associated. A battered helmet must be the helmet of Ajax ; a shivered lance must be the lance of Achilles, and so on. But in spite of defects in Schliemann's education and in spite of his over-great enthusiasm verging on insanity, he has done the world an incalculable service. Excavations on ancient sites are to be the order of the day for the next decade.

Dr. Robert P. Keep, of Hartford, Conn., read a paper on "Mr. Isaac Taylor's 'Etruscan Researches.'"

The chief source of information respecting the language of the Etruscans, is the inscriptions, which, in number not less than three thousand, have been discovered in different parts of Etruria. The character in which they are written offers little difficulty, resembling clearly as it does the character common to ancient Greek and Latin records. These inscriptions are found upon a closer examination to be exceedingly disappointing. Only seventeen of the whole number are bilingual, and of the rest many are mere mortuary records of the briefest form, while it often happens that one is but the repetition of another. Of a literature we can not seriously speak. We have only a collection of fragments, a few scattered words. The importance of the interpretation of these, however, is apparent when we consider the intimate relations which existed for several centuries between Etruria and Rome. How much indeed of what we call the essential character of Roman civilization was due to or directly borrowed from the Etruscans, how far the Roman mythology, where it differs from the Greek, may be Etruscan, we shall only know when we shall have discovered the linguistic affinities of the Etruscan language.

The latest attempt in this direction is that of the Rev. Isaac Taylor, in his book published last winter in London, and not yet reprinted in this country, entitled "Etruscan Researches." He maintains that the Etruscans are of Scythian or Turanian origin. The *presumption* in favor of this theory follows from a consideration of their architecture, their religious belief, their social customs, their artistic capacity, and their mental and physical constitution ; and the *confirmation* is sought in a comparison of the remnants of their language with the vocabularies of different people of the so called Turanian family. It is not, however, too much to say that the presumption after the perusal of the first or general part of Mr. Taylor's book, where he discusses the question on ethnological grounds, remains *against* the theory. We pass to what Mr. Taylor considers

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the more important division of his work, the attempt to identify and interpret Etruscan by the aid of Ugric words.

In the Museum at Palermo there is an Etruscan sarcophagus with a relief in the Greek style upon its face, representing the parting scene between a husband and his wife. On either side of a door which represents the entrance to the lower world, stand two winged genii and under them are written the words KULMU and VANTH. The meaning of "death angel," or "destroying angel" seems clear enough for the two words. Now in the Finnic Epic Poem, the "Kalevala," *Kalma* means "ruler of the grave;" in modern Finnic, *Kalma* is "the smell of a corpse"; in Samojed, *Kolmi* is "spirit of the dead"; in Lapp, *Kalmi* is "the grave"; *i. e.*, these different words in various Ugric dialects show a correspondence to each other in form and meaning, and KULMU resembles them in form. For VANTH, Mr. Taylor gives us Turkish *fena*, "annihilation" and Finnic *wana* "old." To show how easy it is to give from Latin and Greek examples of correspondence both in form and probable meaning to a large part of the Etruscan words which Mr. Taylor brings forward, suppose we suggest the great root *φαν* as we see it in derived words, such as *φάντασμα*, *ἱεροφάντης*. I will here and occasionally in other cases suggest such analogies. My object is rather to show that Mr. Taylor's method yields no trustworthy results than to claim for my own examples identity with the words which have suggested them.

A fresco on the walls of a tomb at Volsci represents the immolation of Trojan prisoners by Achilles. Over the head of the figure which witnesses the sacrifice is written HINTHIAL PATRUKLES, which seems to mean "Shade of Patroclus." We have also a mirror, upon which is portrayed the visit of Ulysses to the lower world. He is accompanied by TURMS AITAS, "Hermes of Hades" and near him stands a drooping corpse-like figure HINTHIÁL TERESIAS, "the shade of Teresias." Now Tungusic *Han* means "idol." For the meaning of AL we are referred to the mirror where one of the Trojans awaiting immolation is labelled TRUIALS. S is considered to be demonstrative, and AL to be a sign of descent. Trui-al-s then means "this the son of Troy." Of the word HINTHIAL, we understand now the first and last syllables. There remains the middle syllable THI which Mr. Taylor thinks signifies "grave," and he explains the whole word, taking the elements in no regular order, but in the order 1-3-2, "the image of the child of the grave." Would a connection with the root *ιδ* as modified in *εἴδωλον*, *ἰνδάλλομαι*, Odyssey III. 346, not be less far fetched, and absurd? TH represents with tolerable regularity in Etruscan words, a Greek *δ*; e. g. UTHUZE—*Ὀδυσσεύς*.

After showing the unsatisfactory treatment by Mr. Taylor of several other words, the author of the paper called attention to his interpretation of the syllables found on the so called "dice," discovered at Toscanella, in 1848. Mr. Taylor stakes his case upon his success in identifying these syllables with Ugric numerals. The following table will show what the analogies are upon which he so confidently rests:

for MACH, Turkish *bar-mach*, "finger,"=1; KI, Finnic *kez*, *kezi*, "hand,"=2; ZAL, Finnic *jalka*, "hand,"=3; SA [Total disagreement between the Ugric dialects in designating four, which Mr. T. believes to be the meaning of SA—]=4; THU, Yenisseic *ton*, "hand,"=5; HUTH, Samojedic *much-tun*, much = mach = 1; tun = 5, *much-tun* is to be regarded as suffering contraction into HUTH,=6.

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The following was the parallel, made in 1848, in the German Institute, between these Etruscan syllables and the Greek and Latin numerals:

MACH, *μία*; THU, *δύο*; ZAL, *τρεῖς*; HUTH, quatuor; KI, quintus; SA, sex.

Since Mr. Taylor's book deals with languages which few understand, it must be judged according to the merits or defects of its method. This test it can not bear. Its author lacks discrimination as well as the special knowledge which such an investigation as he has undertaken presupposes. The first facts of the theory are left unproved. The agglutinating character of the Etruscan language is not made out. The chief service which the book will render will be in calling anew the attention of scholars to an important problem, and in furnishing to the general reader a convenient manual of information about the Etruscans.

Dr. J. Hammond Trumbull, of Hartford, Conn., presented a paper on "Names for Heart, Liver, and Lungs."

Three or four Indian tribes living west of the Mississippi were designated by the Algonkins as *Panis*. This name (now commonly written *Pawnees*) did not belong to the language of those who bore it, but was an appellation contemptuously given by Algonkins to servants and inferiors. It denotes, primarily, the Lungs or Lights, of man or beast. A simpleton, coward, slave, or generally an inferior being was characterized as *lung-y* or 'all lungs'. A similar figure of speech is found in other American languages. In the Dakota, *cha'ghu* is 'lungs', *chaghu-ka* 'a fool'. In the Arapoho, *iḳun'a* 'lungs', *kuna-nit'ut* 'cowardly, easily scared'. Nor is the figure exclusively American. In the Lapp, we find *keppa* 'lung', *keppes* 'poor, mean': and in the African Mpongwe, *ibobo* means both 'lung' and 'coward'. The association of ideas of weakness and inferiority with the lungs, seems to have originated in contrasting these organs with the liver. The liver is heavy, compact, of dark color; the lungs light, spongy, pale: the liver was esteemed good for food; the 'lights' were of little value. With the one, came to be associated ideas of strength, constancy, activity, courage; while the other became the type of weakness, levity, inactivity, cowardice. The liver was regarded as the seat of the desires and passions by which men come to mastery; the lungs, as the mere *servants* of the body, kept at unceasing work day and night. The quality which in most European languages has given names to the lungs is their *lightness*. The English 'lights' and 'lungs' are etymologically identical, both being represented in the Skr. *laghu*, which has the meanings of 'feeble', 'mean', 'insignificant', as well as of 'light' (*levis*). In Polynesian languages, Tonga *mama* means 'light' and 'lungs'; Hawaiian *akemama* 'lungs' is literally 'light liver' (Germ. *die leichte Leber*). The Eskimo *puak* 'lung' is related to *puïok* 'to float on water'; and the Mohawk *ostiesera* 'lungs', to *ostosera* 'feathers', etc. The association of ideas by which 'light' takes the meanings 'slight', 'weak', 'inconstant', etc., is obvious. Less clear, at first sight, is the connection between 'lightness' and 'slowness'. We may trace it in Indo-European derivatives from the root of Skr. *laghu* and *langh*, including Irish *lag*, and English 'lag' and 'laggard', as well as 'lungs' and 'lights'. The old naturalists taught that "the smaller the lungs are in proportion to the body, the greater is the swiftness of the animal" (Plin., *Hist. Nat.*, xi. 72).

The Liver has very generally been regarded as the seat of the passions and the animal nature of man. Traces of this belief may be found in many widely-sepa-

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rated languages. The Orientals ascribed to the liver the principal agency in making the blood, and hence, perhaps, it became to them, in some sense, sacred; for "the life of the flesh is in the blood". With the Hebrews, it was 'the most precious', man's 'honor' and 'glory'. Names of the *gall* and of *bile* have generally in European languages been transferred to the evil or ignoble passions. Derivatives from Gr. χόλος, χολή, and Lat. *bilis*, are numerous in modern languages. Lat. *fel*, kindred with *bilis*, received in addition to its secondary meaning, 'poison', that of 'bitter anger' or 'wrath'; A. S. and O. Eng. *fell* was used in the double sense of 'gall' and 'anger', and had its adjectives 'fellish' and 'felly'.

Recognition of the Heart as the life-center and source of vital energy may be found far back in almost every language. To the Semitic and Aryan philosophies, this organ was the seat of mental as well as of physical activity. To it was referred, perhaps by one of the earliest, certainly by one of the most common figures of speech, all that belongs to man's inner life, to "that which perceives, thinks, wills, and desires". In every family of language, we find the name of the physical organ transferred to mental and moral faculties, to the will and the emotions. The Sanskrit *hrid* means 'mind' and 'knowledge' as well as 'heart'; and so, the later derivatives of the same root in the parent speech, Gr. καρδία, Lat. *cord-*, *cor*, Goth. *hairtô*, A. S. *heorte*, etc. In English, we borrow from more than one branch of the great family. From the Latin, through the Norman, we have *core* (the heart as a center) and *courage*. We have *cordial* as well as *hearty*, and once had *cardiac* (heartening, invigorating), now nearly obsolete except among physicians. The old verb 'to hearten' is regaining its place in our language. Other viscera have contributed to our vocabulary by transference of their names to passions and emotions of which they were supposed to be the seats. We retain the adjectives 'choleric', 'spleeny', 'splenetic', 'melancholic', 'hypochondriac', though we no longer locate melancholy in the hypochondria or attribute it solely to excess of 'black bile'.

Professor George F. Comfort, of Syracuse University, Syracuse, N. Y., next presented a paper on "Helveticisms in Schiller's Tell."

No literary production of modern times has been subjected to more searching criticism than Schiller's drama of *William Tell*. In this careful analysis the extraordinary artistic power of the poet has been revealed in nothing more strikingly than the masterly way in which he has given a perfect "local coloring" to the play, weaving in not only allusions to local scenery, customs, and usages, but also introducing local, provincial words, phrases, and expressions, with a skill that is all the more remarkable, since the poet never visited Switzerland. A large number of them are not given in any German-English dictionary, nor are some of them indeed found in even the largest German dictionaries; many of the expressions are not explained even in the dictionaries of the local dialects. Thus, *zu Berg fahren* means "to take a herd of cattle from the wintering place up to the pasture lands on the mountains as they become green through the advancing summer." *Die Alpe* means in Switzerland "a plot of pasture land high up in the mountains." A large number of other words were traced, including some proper names, in which the influence of the neighboring Italian was shown, upon the formation of provincial terms of endearment, as *Seppi* for "Joseph," from *Giuseppe*. Also the remains of old German influences were pointed out, as in

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*Kuoni* for "Konrad." That Schiller could use these provincialisms so accurately and still so freely and artistically, was owing doubtless to the care with which he studied such works as those of Tschudi, Müller, Schenchzer, Etterlein, and Ebel upon Swiss history, geography, scenery, customs, and usages, and to his long intimacy in Weimar with his Swiss friend, H. Meyer. It is a curious circumstance that these words and expressions in so classical a work as William Tell should not be found in standard German-English dictionaries. And usually the non-German student thinks that he is reading the purest German, in passages which are provincial and poetic, and are recognized to be so by the Germans themselves.

As the Local Committee had arranged for a reception to be given in the evening, the Association adjourned till 9 o'clock tomorrow morning.

FRIDAY, JULY 17TH—MORNING SESSION.

The first paper read was by Professor C. H. Brigham, of Ann Arbor, Mich., on "The Agaou Language."

This is the dialect of the Jews of Abyssinia, known as the Falasha people. These Falashas differ from other Jews in knowing nothing of Hebrew. They are equally ignorant of Greek and of Arabic. They have had no connection with other Jewish tribes, but have been familiar for ages with the dialects of the people among whom they have lived.

The language of the dominant race in Abyssinia in the early time was the Gheez, a Semitic dialect. This language early became detached from the Cushite or Himyarite. It has some resemblance to the Coptic, particularly in the form of the verb. From the 14th century it has ceased to be a spoken language, and only the learned understand it. The nearest to it of the dialects which have sprung from it is the Tigré or Khassi language. The Amharic, the official language of the land, is also spoken by the Falasha Jews, as well as by the Christians, though it is not used in religious exercises, but only in secular affairs.

The Falashinya, or Agaou dialect, which the Falashas speak in their households, has nothing Semitic in its structure. It is the descendant of the dialect spoken by the Abyssinian people before the invasion of the Semitic race from the other side of the Red Sea. This may be shown by the comparison of the Bogos-Bilen table of numbers from 1 to 10, with the Falasha names of numbers. They are nearly identical. So the common names of the elements, and of the implements of industry and domestic life, have close resemblance in sound to the ancient Bogos speech.

The Grammar of the Falasha language has several peculiarities. It has no article. The feminine gender is marked by adding *ti* or *eti* to the masculine. The plural is formed in five ways: by adding the word *ki*, which means all, as *yir*, "man," *yirki*, "men";—by doubling the word; by changing the final *a* into *t*; by changing an inner letter, as *khoura*, "child," plural *khorla*, "children"; by adding *in*, to express decimal numerals, as *lina*, "two," *linin*, "twenty." The adjective always comes before the substantive. There are three oblique cases, genitive, dative, and accusative. The personal pronouns are sometimes independent, sometimes prefixed to the word to which they belong. There is only one conjugation for all verbs. The participle is shown by the termination *aŋ*; and



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the personal pronoun before the participle implies combined action. The imperative has a double form, affirmative and negative. When two verbs are joined, the first indicates the manner of the action. There are various other ways of verbal modifications. Illustrations of all these positions were given.

The Agaou language belongs to the great family which includes the Egyptian, Berber, Haoussa, the class of tongues sometimes called "Hamitic." It is notable for the abundance of its nasal tones, for the confusion of its liquids, for its contraction of words, and for the change of gutturals into nasals. Its literature is not abundant, consisting mainly of prayers and translations of Scripture.

Professor W. D. Whitney, of Yale College, New Haven, Conn., read the next paper, on "Φύσει or Θέσει?"

The ancient Greeks disputed whether the names of things existed φύσει, "by nature," or θέσει, "by assignment," i. e. by human attribution—whether they were natural or conventional. The same question is sometimes raised and answered anew at the present time; and the answer is apt to be, φύσει: perhaps especially on the part of those who affect a philosophic profundity in their treatment of the subject. But if there is truth in that answer, it is very far from being the whole truth. On the contrary, in the most direct and obvious sense, names are certainly θέσει. That is to say, the words of all existing languages exist and are used only by convention; they were learned by those who use them; their variety, in relation to any given idea, is as great as that of human languages; they are kept in existence by tradition. There is not a known name in any dialect that has an internal necessary significance, or other than a historical *raison d'être:* even the most obvious onomatopœias are only examples of how human usage has chosen one mode of suggestion rather than another in forming its names: each idea so indicated is in other dialects found expressed by words which possess no such suggestiveness. This is true not only of all existing, but of all recorded speech, and of all that is inferable for pre-historic epochs, or restorable by scientific processes. It only remains disputable whether the very earliest stage of expression, the germ of the after conventional growth, was natural and necessary. Upon this point, opinions may and probably will long remain at variance. The speaker believed, however, that here also the only true and tenable answer is θέσει. And this in part because he held that the impulse to communication was the final and direct producer of speech; that there would have been no speech without it. It is not, of course, the whole force, or the grandest of the forces, that combine to the existence of speech. If a stone lie supported at the edge of a precipice, it may continue there for ages without stirring; all the vast cosmical forces of gravity will have no power to set it in motion; but a slight thrust sideways, from some accidental and transient cause, topples it over, and it goes crashing down. Is it the thrust, or gravity, that produces the fall? Either, or both. There would have been no fall without gravity; but gravity would never have resulted in the fall without the thrust. So all the noble endowments of man's nature would never have brought him to language without the added impulse to communication which comes from his social disposition. And names are given to things by him for the satisfaction of this impulse, being made such as conduce to intelligibility; though language as a whole becomes a worthy exponent and instrument of his best powers.

Words, then, in their individuality, exist θέσει, and only θέσει: but the θέσις itself is φύσει, if we may include in φύσις not only man's natural gifts but also his

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natural circumstances. In this sense only, and with these limitations, is it proper to answer φύσει to the question as to the existence of speech.

Mr. John Swinton, of New York, presented a paper on "Linguistic Perspective."

It related to the elements, forces, and scope of the English language. The author showed by statistics that if it continued for another century at the ratio of the growth of the past century, it would then be spoken by as many people as now inhabit the globe. He showed that it was spoken by more people than any other European language; and that it was the only language that was spoken by two great powers (England and the United States). He indulged in a series of speculations concerning his theory, showing how the dominating English dialect was absorbing all local dialects, and discussing other questions of interest.

The Committee to nominate officers for the following year made nominations as follow:

For *President*—Dr. J. Hammond Trumbull, Hartford, Conn.

For *Vice-Presidents*—Professor S. S. Haldeman, University of Pennsylvania, Columbia, Penn., Professor Charles Short, Columbia College, New York.

For *Secretary and Curator*—Professor Samuel Hart, Trinity College, Hartford, Conn.

For *Treasurer*—Professor Albert Harkness, Brown University, Providence, R. I.

For additional members of the *Executive Committee*—

Professor Fisk P. Brewer, University of South Carolina, Columbia, S. C.

Professor Martin L. D'Ooge, University of Michigan, Ann Arbor, Mich.

Professor Edwin S. Joynes, Washington and Lee University, Lexington, Va.

Professor Lewis R. Packard, Yale College, New Haven, Conn.

Professor Edward H. Twining, University of Missouri, Columbia, Mo.

The report was accepted, and the persons therein named were declared elected to the offices to which they were respectively nominated.

The Committee to select the place and the time of the next meeting recommended that the meeting be held at Newport, R. I., on the 13th day of July, 1875, at 3 o'clock P. M.

The report was accepted, and the recommendation of the Committee was adopted.

The Executive Committee were desired to take into consideration the question of holding winter sessions of the Association at places in the southern portion of the United States.

On motion, it was

*Resolved*, That the members of the Philological Association gratefully acknowledge the kindness and hospitality of the citizens of Hartford, so generously tendered at an inconvenient season; the attentions of the efficient Local Committee; the courtesy of the High School Committee, in giving the free use of their commodious building for the sessions of the Association; and the considerate favor of the railway companies in the return tickets given to the members of the Association.

The minutes of the meeting having been read and approved,

On motion, the Association adjourned.

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# OFFICERS OF THE ASSOCIATION.
## 1874–5.

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J. HAMMOND TRUMBULL.

VICE-PRESIDENTS.

S. S. HALDEMAN,
CHARLES SHORT.

SECRETARY AND CURATOR.

SAMUEL HART.

TREASURER.

ALBERT HARKNESS.

EXECUTIVE COMMITTEE.

The officers above named, and—

FISK P. BREWER
MARTIN L. D'OOGE,
EDWARD S. JOYNES,
LEWIS R. PACKARD,
EDWARD H. TWINING.

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# CONTENTS.

I. On the Formation of the Tenses for Completed Action in the Latin Finite Verb.—Second Paper.
By Professor ALBERT HARKNESS. . . . . 5

II. On an English Consonant-Mutation, present in PROOF, PROVE.
By Professor S. S. HALDEMAN. . . . . 20

III. On Begemann's Views as to the Weak Preterit of the Germanic Verbs.
By Professor FRANKLIN CARTER. . . . 22

IV. On Some Forms of Greek Conditional Sentences.
By Mr. CHARLES D. MORRIS. . . . . 44

V. On Verb-Reduplication as a Means of Expressing Completed Action.
By Professor ALONZO WILLIAMS. . . . . 54

VI. A Grammatical Analysis of the Old English Poem "The Owl and the Nightingale."
By Mr. L. A. SHERMAN. . . . . 69

PROCEEDINGS:—Seventh Annual Session, Newport, 1875.

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TRANSACTIONS

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AMERICAN PHILOLOGICAL ASSOCIATION.

1875.

# I.—*On the Formation of the Tenses for Completed Action in the Latin Finite Verb.—Second Paper.*

BY ALBERT HARKNESS,

PROFESSOR OF THE GREEK LANGUAGE AND LITERATURE IN BROWN UNIVERSITY.

IN a paper read before this Association at its last annual session, I attempted to discuss the difficult question of the origin and formation of the Latin perfect in *si*. The examination then made, aided by such light as could be gathered from Comparative Philology, seemed to warrant the conclusion that in this class of verbs the peculiar endings of the Latin perfect, as seen in *i*, *isti*, *it*, *imus*, *istis*, *erunt* or *ere*, may be directly derived from the reduplicated stem *esis* of the root *es*, from the original form *asasma*. Moreover, the change from *esismi*, *esisti*, etc., to the classical form *esi*, *esisti*, etc., was found to be quite inconsiderable, and the process by which it was effected to be at once simple and natural.

In the present paper I propose to examine perfects in *ui*, *vi*, and *i*, including of course the auxiliary *fui*. The discussion of this subject will involve a somewhat careful examination of the entire tense-formation of the Latin finite verb.

As an illustration of one of the primitive verbs of the language, we may take *cano*, from the Indo-European root *kan*. The original root remains unchanged in the present. The perfect is reduplicated, and in this particular is one of the few



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Latin perfects which conform to the original type of the Indo-European; yet even this verb, with all its primitive features, differs widely in its tense-formations from that system of verbal inflections which the Latin, the Greek, and the Sanskrit must alike have inherited from the original mother-tongue of the family. Notice for a moment its tense-forms: *cano*, *canebam*, *canam*, *cecini*, *cecineram*, *cecinero*, *canam*, *canerem*, *cecinerim*, *cecinissem*. With the exception of *cano* and *canam*, which are in their origin present formations, and of *cecini*, which we are now discussing, the obvious and distinguishing characteristic of this entire group of forms is that they are all compounds of the auxiliary, as seen in *sum* and *fui*. In making this statement, however, I assume the correctness of the generally received opinion in regard to the formation of compound tenses by means of auxiliary verbs—a point which was, I think, sufficiently discussed in my former paper. Thus, *bam* in *cane-bam* is the imperfect of *bhu* (=*fu*), as *eram* is the imperfect of *es*. In the form in which they came from the mother-tongue of the Indo-European family, both *bam* and *eram* undoubtedly had the augment, though no trace of it now remains in either, unless it lies concealed in the long *ē* in *canēbam*. *Eram* and *ero* in *cecin-eram* and *cecin-ero* are at once recognized as the imperfect and the future of the auxiliary *sum*. *Can-erem* is a compound of *esem*, the original form of *essem*; *cecin-erim* of *erim* for *esim*, the full form for *sim*, with *s* changed into *r* between two vowels. In like manner *cecin-issem* is a compound of *essem*.

It is scarcely necessary to explain in passing, how *esem*, the early form of the imperfect subjunctive of *sum*, can appear as *erem* in *can-erem* and as *issem* in *cecin-issem*. It is obvious that *esem* would regularly become *isem* in compounds, and then that *isem* would either become *erem* or *issem*, as it is a law of the language that *s* between two vowels generally passes into *r*, and that *i* before *s* becomes *e* before *r*. But if it be asked why the *s* is thus changed into *r* in *canerem* and yet doubled in *cecinissem*, we need only remark that a variety of treatment in the development of language is by no means uncommon. The treatment of the two letters *e* and *s* in the

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inflection of the verb *sum* furnishes ample illustrations. The *e* of the root is sometimes dropped, as in *sum*, *sumus*, *sim*, *simus*, and sometimes retained, as in *est*, *estis*, *eram*, *ero*. The *s* in this verb has a three-fold treatment: it is retained unchanged in *est*, *estis*, and in *sum*, *sim*, though in the full forms, *esum* and *esim*, this would have been impossible; it is changed into *r* in *eram* and *ero;* and it is doubled in *essem*. These are indeed but illustrations of the remarkable facility with which the languages of the Indo-European family have from the simplest elements produced the most varied forms, especially in their systems of verbal inflections.

The presence of the auxiliary *es* or *fu*, even in the most primitive verbs of the language, in all the tenses for completed action and for past time, unless the perfect is an exception, naturally suggests the inquiry whether the auxiliary may not exist, though in a somewhat disguised form, even in that tense. Indeed in the subjunctive, the perfect has undoubtedly a compound form. Moreover, everywhere in the Latin verbal inflections, unless the perfect is an exception, simple forms in the indicative correspond to simple forms in the subjunctive, and compound forms in the subjunctive correspond to compound forms in the indicative; *amo*, *amem; rego*, *regam; sum*, *sim; eram*, *essem*—all simple forms; *can-erem*, *cane-bam; cecin-issem*, *cecin-eram*—all compound forms. After this uniform analogy should we not expect that *cecini*, if a simple perfect stem, would form its perfect subjunctive as the present stem *audi* forms its present subjunctive—*audi*, *audiam; cecini*, *ceciniam?* There surely would be no difficulty in the way of such a formation; indeed it would only be in accordance with what we find elsewhere, both in the Latin and in the cognate languages. On the other hand, from the compound *cecin-erim* we naturally infer for the indicative a perfect compounded with *sum*—a correspondence which actually appears in the third person plural, as in *cecin-erint*, a compound of *erint* for *esint*, the full form of *sint*, and *cecin-erunt*, a compound of *erunt* for *esunt*, the full form of *sunt*.

But, turning to the passive voice, we find that all the tenses for completed action are periphrastic forms, consisting of the

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perfect passive participle with the auxiliary *sum*, while all the other tenses are formed directly from the active. Thus all the simple forms of the active, unless the perfect is an exception, correspond to simple forms in the passive, while all the periphrastic forms of the passive correspond to compound forms in the active, unless here again the perfect is an exception. Indeed, in examining the Latin verbal inflections, I have been surprised to find in how very many points the Latin perfects are anomalous upon the supposition that they are simple forms; and I have been scarcely less surprised to notice how completely all these irregularities disappear and how readily all these perfects conform to general laws, as soon as we recognize them as compounds of the auxiliary *sum*. I do not, however, claim that the considerations which I have thus far adduced furnish any positive proof that the auxiliary *sum* is an element in the formation of these perfects; but I do claim that they all point towards such a conclusion, and render it at least not improbable. They suggest the propriety of a critical examination of this formation to see whether traces of the auxiliary may not be discovered in it, while at the same time they anticipate any objections which might otherwise be brought against this view from the Greek and the Sanskrit perfect.

Let us then take the following examples as representatives of the several classes of Latin perfects: *esi*, *carpsi*, *cecini*, *fui*, *alui*, and *amavi*. They are inflected as follows:

| | | |
|---|---|---|
| 1. es-i, | 2. carp-s-i, | 3. cecin-i, |
| -isti, | -s-isti, | -isti, |
| -it, | -s-it, | -it, |
| -imus, | -s-imus, | -imus, |
| -istis, | -s-istis, | -istis, |
| -erunt (*or* -ere). | -s-erunt (*or* -s-ere). | -erunt (*or* -ere). |

| | | |
|---|---|---|
| 4. fu-i, | 5. al-u-i, | 6. ama-v-i, |
| -isti, | -u-isti, | -v-isti, |
| -it, | -u-it, | -v-it, |
| -imus, | -u-imus, | -v-imus, |
| -istis, | -u-istis, | -v-istis, |
| -erunt (*or* -ere). | -u-erunt (*or* -u-ere). | -v-erunt (*or* -v-ere). |

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The most cursory examination of these forms reveals the fact that the endings, *i*, *isti*, *it*, etc., on the one hand present the most remarkable peculiarities, entirely without a parallel in any other tense in the Latin language, while on the other hand they preserve the most unvarying uniformity throughout all classes of Latin verbs, being precisely the same in the latest derivative as in the earliest primitive. But let us see whether our six examples are really independent forms.

*Carp-s-i* is simply a compound of *esi*, and its tense-sign is in the auxiliary. In *cecini* and *fui* the tense-sign originally consisted solely in the reduplication, which has been preserved in *cecini*, but lost in *fui*, though traces of it are preserved in the earlier Latin in the form *fuvi*, and in *fūi* with the long *u*. *Al-ui* for *al-fui*, and *ama-vi* for *ama-ui* or *ama-fui*, are both compounds of *fui*; *f* is dropped, and in *ama-vi* the *u* is changed into its corresponding *v* between two vowels; the tense-sign is in the auxiliary.

We have thus found that three of our six representative examples of Latin perfects are compounds of auxiliaries: *carp-s-i* of *esi*, and *al-ui* and *ama-vi* of *fui*. We may therefore dismiss these for the present from our discussion, as they will all find a complete explanation in the analysis of the auxiliaries of which they are compounded. If therefore we can explain the origin and formation of *esi*, *fui*, and *cecini*, we shall solve in full the problem of the Latin perfect. But the difficulty lies in the peculiar endings of which we have already spoken. Now the fact that such remarkable peculiarities are found with unvarying regularity in every perfect active in the language renders it quite certain that they have a common origin. In the previous paper we discovered that the forms of the Latin *esi* and of the corresponding Sanskrit *āsa* differ so widely from each other that they must have been reached by different methods. The original form from which they were both derived was probably *asasma*, from the root *as*. From this the Sanskrit forms can be reached only by dropping the first *s*, and contracting *aa* into long *a*. In the Latin, on the contrary, this *s* appears to have been retained, but before the classical forms were reached a two-fold change must have taken place.

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I. The original vowel *a* of the root *as* became *e*, giving the root *es*. Moreover, this *e* was weakened to *i* in the second syllable of the reduplicated stem *esis*, a change entirely analogous to that which takes place in *cecini*, from *cano*; the *a* in the personal ending *ma* also became *i*. We thus have the form *esismi*.

II. The full form *esismi* was then gradually shortened. The steps by which this was effected were all explained in my former paper and need not be repeated here. We noticed the disappearance of *s* before *mi* and *mus*, and the dropping of the ending *mi* with the lengthening of the preceding *i* in the first person singular. We observed also the disappearance of *s* before *t* in the third person singular. Some of these changes, natural in themselves, were undoubtedly facilitated by the analogy of the other primitive perfects in which the endings *mi*, *mus*, and *t* were not preceded by *s*. The forms of *esismi*, *esisti*, etc., became *esi*, *esisti*, *esit*, *esimus*, *esistis*, *eserunt*, which are the classical forms of the auxiliary as seen in *carp-si* for *carp-esi*, *carp-sisti*, etc. We thus reached a very simple and natural explanation of the peculiar endings of *esi* and its compounds, i. e., of all perfects in *si* and *xi*.

But how are these endings to be explained in *fui* and *cecini?* They probably have, as I have already remarked, one common origin in all Latin verbs. But what do they really represent in the forms of the auxiliary *esi?* We explained *esi* itself as shortened from *esismi;* the final *i* is therefore the remnant of the simple root *es* with the personal ending *mi*. *Isti* in *esisti* is the root with the personal ending *ti*. In the same manner the endings *it*, *imus*, *istis*, and *erunt*, all consisted originally of the personal endings added to the simple root *es*. But the union of personal endings with the simple root forms the present tense, just as the union of those endings with the reduplicated root forms the perfect.

The facts just mentioned suggest the inquiry whether *fui* and *cecini* may not contain the present of the auxiliary *es*, *sum;* whether *fui*, *fuisti*, etc., may not come from *fuismi*, *fuisti*, etc., as *esi*, *esisti*, etc., from *esismi*, *esisti*, etc., and whether in the same way *cecini*, *cecinisti*, etc., may not come

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from *cecinismi*, *cecinisti*, etc. Indeed, after what has been said, I scarcely see how it is possible to look at such forms as *fu-is-ti*, *fu-is-tis*, *fu-er-unt* (=*fu-is-unt*), and *cecin-is-ti*, *cecin-is-tis*, *cecin-er-unt* (=*cecin-is-unt*), without recognizing the auxiliary *es* as an element in the formation, as it lies there entirely undisguised between the root and the personal endings. *Erunt* (=*isunt*) is for *esunt*, the full form for *sunt*; *istis* is for *estis*, the second person plural of *sum*; and *isti* is for *esti*, the full form for *es*, the second person singular of *sum*.

But to this view a ready objection will be found in the fact that it is not supported by the analogy either of the Sanskrit perfect or of the Greek. This is not, however, a very formidable objection. We have already observed that the forms of the Latin *esi* differ so widely from those of the corresponding Sanskrit *āsa* that they must have been produced by a different treatment. Moreover the Sanskrit, the Greek, and the Latin, all have the root *bhu* (=*fu*) in common. From this root the Sanskrit forms *babhuva*, *babhuvitha*, *babhuva*, etc.; the Greek, πέφυκα, πέφυκας, πέφυκε, etc.; and the Latin, *fui*, *fuisti*, *fuit*, etc. It will require no argument to prove that these three sets of forms are not constructed on the same model. Neither of them preserves the original perfect of this root unchanged, though the Sanskrit undoubtedly comes nearer the original form than either of the other languages. The Greek πέφυκα contains an element, κ, not found in the Sanskrit or in the Latin, while the Latin, on the other hand, shows in *fu-is-ti*, *fu-is-tis*, and *fu-er-unt*, an element *s* or *is* not found in the Sanskrit or the Greek. The absence of the auxiliary, therefore, from the Sanskrit and the Greek perfects no more disproves its existence in *fui* than the absence of κ from the Sanskrit and the Latin disproves the existence of that letter in the Greek πέφυκα. The same remark applies to *cecini* and to all other perfects in *i*.

But what is the import and meaning, it may be asked, of *esmi* (=*sum*) as an element in the formation of *fui*, *cecini*, etc.? It is obviously no part of the tense-sign, as that is preserved in full in *cecini* and belonged originally to *fui*, as is shown by the earlier *fūi* (with long *u*) and *fuvi*. How then did it

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obtain a place in the Latin perfect, and what purpose was it originally intended to serve?

A brief outline of the progressive development of the Indo-European system of verbal inflections will, I trust, throw some light upon this question. Curtius, in the last edition of his able work, "Zür Chronologie der Indo-germanischen Sprachforschung," marks three distinct epochs, or stages, in the history and growth of the system of verbal inflections in the mother-tongue of the Indo-European family, from which the Latin, the Greek, and the Sanskrit alike derived their inheritance of verbal forms.

I. The first stage consisted simply in the union of a verbal root with a pronominal root or stem. Thus from the root *da* was formed *da-ta*, Latin *dat*, 'he gives.' Of course only a few of these elemental forms have come down to our time; but the Sanskrit *as-mi*, the Greek ἐστί, and the Latin *est*, may serve as illustrations.

II. During the second period, verbal roots were developed into stems or themes in various ways, especially by the addition of the determinative *a*. These stems were then inflected like the roots of the first period by the addition of pronominal roots or stems. Thus the root *bhar* became the stem *bhara*, and *bhar-ta* became *bhara-ta*. Subsequently this *a* became in the Greek *o* or *ε*, as in φέρομεν, φέρετε, and in the Latin *o*, *i*, or *u*, as in *fero*, *ferimus*, *ferunt*.

III. The third period shows us for the first time compounds of the auxiliaries *as* and *ja*: as *a-dik-sam*, which became in the Sanskrit *a-dik-sham*, and in the Greek ἔδειξα; *kama-ja-mi*, which became in the Latin *ama-o*, *amo*.

These three stages in the development of verbal forms were all reached by the mother-tongue before the Latin, the Greek, the Sanskrit, or any other known language of our family had a separate existence. If now we follow out this course one step further, we shall meet in the separate languages various periphrastic forms, as the Sanskrit *ćorojam āsa* or *ćakara*, the the Greek τετελεσμένοι εἰσί, ἔχον ἐστί (= ἔχει), and the Latin *amatum iri*, *amatus sum*.

It will be observed that such compound and periphrastic

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forms as *a-dik-sham*, ἔδειξα, *cecineram*, and *amatus sum*, all contain the copula, whose office is to connect the predicate with the subject. It may not, indeed, be easy to reproduce the original conception embodied in such a compound as *a-dik-sham*, but it may be rendered approximately 'then was I showing.' The copula ('was' in English) has its distinct sign. In the verbal forms of the previous periods the copula was not represented by any separate sign, though the relation of subject and predicate was undoubtedly recognised. Curtius justly remarks that a compound aorist like *a-dik-sham*, ἔδειξα, differs from a simple aorist very much as the Latin *tum dicens erat* differs from *tum dicens*. In other words, the former has an expression for the copula, while the latter has not.

Now tense-forms compounded of the auxiliary *as*, with the force of a copula, are important elements in the verbal systems alike of the Latin, the Greek, and the Sanskrit; but the regularity with which they have supplanted more primitive forms is preëminently marked in the Latin. In that language indeed they are found in every tense except the present.

If now we inquire what tense-forms were developed by the mother-tongue of the Indo-European family, I think that we shall find with Schleicher that even that primitive language probably had four simple tense-forms—a present, an imperfect, a perfect, and an aorist—and two compounded tense-forms—a future and an aorist. Now these forms, simple and compound, must have been the common inheritance of the Latin, the Greek, and the Sanskrit. Moreover, the tendency to form compounds of the auxiliary, as copula, which had already become distinctly marked before either of these languages had a separate existence, was afterwards still further carried out by the Greek and the Latin in their systems of verbal inflections. We must now examine the results of this tendency in the Latin tense-forms.

The Latin inherited a simple present which it retained to the last. It also inherited a simple imperfect, but it proceeded to form a compound of *fu* for the indicative, and of *es* for the subjunctive, as *ama-bam*, *ama-rem*. The simple forms of the imperfect gradually disappeared from the language, except *eram* and *essem* of the auxiliary. It inherited a compound



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future in *so* for *sio*, as is evident from the Sanskrit and the Greek (as *dasjami*, λύσω), but it created another in *bo* for *bio* from *fu*, as *ama-bo*. It also inherited a simple reduplicated perfect; but if the view set forth in this paper is correct, it formed a compound perfect by simply adding to the reduplicated root the present indicative of *es* (*esmi* = *sum*) for the perfect indicative, and the present subjunctive (*esim* = *sim*) for the perfect subjunctive. Thus we have *cecin-ismi*, which became *cecini*, as *esismi* became *esi;* and *cecin-isim*, which became *cecin-erim*, as *esisim* became *eserim*. Thus also we have *fu-ismi*, *fui; fu-isim*, *fu-erim*. In the same manner it formed from the reduplicated root, first a compound pluperfect by appending the imperfect *eram* for the indicative and *essem* for the subjunctive: *cecin-eram*, *cecin-issem; fu-eram*, *fu-issem;* and secondly a compound future perfect by appending the future *ero: cecin-ero*, *fu-ero*. Thus from the reduplicated root, or perfect stem, were formed in the indicative a perfect, a pluperfect, and a future perfect, by appending respectively the present, the imperfect, and the future of the auxiliary *es*, and in the subjunctive a perfect and a pluperfect by appending the present and imperfect subjunctive of the same auxiliary. Thus interpreted, the Latin system of verbal inflections in the tenses for completed action is perfectly symmetrical and consistent.

But it may be claimed that *eram* and *ero* are essential elements in the formation of the pluperfect and the future perfect tenses, while the present of the auxiliary in no way aids in forming the perfect tense, inasmuch as the essential idea, that of completed action, is already expressed by the reduplication. That such an objection is not really valid will, I think, be apparent from the following considerations.

1. A simple pluperfect could have been formed from the perfect stem without the auxiliary, just as the Greek imperfect was actually formed from the present stem, and just as a simple aorist, or pluperfect, was formed from the reduplicated root in the Sanskrit. A future perfect could also have been formed from the perfect stem, just as *ero* was actually formed from *es*, i. e., by simply adding *io*. Here then the auxiliary

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is not at all necessary to the formation of the tense, but is introduced for its own sake.

II. The mother-tongue of the Indo-European family very early formed a simple aorist tense directly from the root, but it subsequently formed a compound aorist by means of the auxiliary. Thus, even in that primitive age of verbal inflections, there existed side by side two forms of the same tense, an earlier form without the auxiliary or copula, and a later form with it. The Greek inherited both of these forms in its two aorists. In the same manner, in my opinion, the Latin, soon after the separation of the different branches of the family, though it already possessed a primitive perfect without the auxiliary, proceeded to form a new one with it. The cases are entirely parallel.

In this statement I of course assume, at variance with the common opinion, that the compound aorist of which I have just spoken contains the *present* and not the *imperfect* of the auxiliary. The idea of past time is expressed by the augment and need not be repeated in the auxiliary. But if this point be questioned, we may easily adduce examples in which the present of the auxiliary actually appears. That the present *sim* is an element in *cecin-erim* and *fu-erim* is a generally admitted fact; and if it be claimed that this aids in forming the mood, the obvious answer is that *sim* is in no sense a mood-sign, but a fully developed auxiliary verb; that in fact the present subjunctive of *sum* is no more necessary in the formation of the perfect subjunctive than its present indicative is in the formation of the perfect indicative. But I need not multiply illustrations or arguments upon this point, as it is generally admitted that the root *es* does appear in the ending *erunt*, for *esunt*, the full form for *sunt*, in the third person plural of the active voice of every perfect tense in the Latin language, whatever its form in other respects: *fu-erunt*, *cecin-erunt*, *dix-erunt*, *amav-erunt*. Moreover, the presence of the auxiliary is almost equally clear in the second person, singular and plural: *fu-isti*, *fu-istis*, *cecin-isti*, *cecin-istis*, *dix-isti*, *dix-istis*.

What then was the probable development of the Latin

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perfect? The language undoubtedly inherited a simple reduplicated perfect, but subsequently formed a compound one, which differed from the simple form precisely as the compound aorist differed from the primitive aorist. Like the primitive perfect, it was reduplicated; but, unlike that, it contained the auxiliary *es*. This became at length the prevailing form. The changes which it subsequently underwent in accordance with a uniform tendency in language to shorten words were precisely the same as those which have been already explained in our treatment of *esi* for *esismi*. Thus were formed *fu-i* (originally reduplicated *fufu-ismi*), *ce-cin-i*, and, in fine, all perfects in *i*. The simple primitive perfect finally disappeared in all Latin verbs, except the auxiliary *esi*, which is preserved only in compounds.

The perfect formed by appending the present of *es* to the perfect stem, must, I think, in its origin have preceded the formation of perfects in *si*, *ui*, and *vi*. At this stage in the development of verbal forms, every Latin perfect probably contained a reduplicated stem and the auxiliary *es*. From this stage the transition was easy and natural to the formation of a perfect from the present or verb-stem through the aid of the perfect of the auxiliary. It is at once apparent that the perfect of the auxiliary added to the verb-stem is entirely equivalent to the present of the auxiliary added to the perfect stem. Thus, for example, in the verb *teneo* a new form *ten-ui*, consisting of the verb-stem *ten-* and the perfect *fui*, became an exact equivalent of the older form *tetini*, consisting of the perfect stem *tetin-* and the present *esmi*. Thus at length there existed side by side two equivalent compound forms, an earlier and a later. In a few verbs both of these forms have been preserved: *tetini*, *tenui*; *pepigi*, *panxi*; *peperci*, *parsi*.

Such, it seems to me, was the origin of perfects in *si*, compounded of *esi*, and perfects in *ui* and *vi*, compounded of *fui*: *carp-si* for *carp-isi*, *dixi* for *dic-isi*; *al-ui* for *al-fui*, *ama-vi* for *ama-fui*. It is, however, often assumed that these compounds were formed to supply the place of a lost reduplication. This assumption I am inclined to regard as erroneous, at least in

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respect to compounds in *si*. All compound perfects indeed grew very naturally out of a tendency already developed in the mother-tongue, a tendency to which the Latin yielded more readily and more completely than the Greek or the Sanskrit; but those in *si*, *ui*, and *vi* became the favorite forms and thus supplanted most of the older reduplicated perfects. This view, I think, best accounts for the disappearance of the reduplication in so large a proportion of Latin verbs; for if the new forms were intended simply to supply the place of a lost reduplication, they would seldom have appeared in verbs which had not already lost it; yet compounds in *si*, *ui*, and *vi* existed even in the classical period side by side with reduplicated forms. Moreover, many archaic forms, as *faxit* (= *fecerit*), *axim* (= *egerim*), *taxis* (= *tetigeris*), *sponsis* (= *sposponderis*), *capsit* (= *ceperit*), show that compounds without reduplication existed long before the classical period in verbs which retained the reduplication or at least some trace of it throughout all periods of Latin literature.

But how were the other tenses for completed action formed? In accordance with the explanation already given of the formation of these tenses in verbs whose perfect ends in *si*, it is only necessary to add that those verbs which form the perfect indicative by adding *fui* to the verb-stem, generally form the pluperfect by adding *fueram*, the future perfect by adding *fuero*, the perfect subjunctive by adding *fuerim*, and the pluperfect by adding *fuissem*.

But a few forms in *asso*, *esso*, and *so*, and a few in *assim*, *essim*, and *sim*, require explanation. These forms are now generally admitted to belong respectively to the future perfect and the perfect subjunctive, notwithstanding Madvig's attempt at a different explanation. With a few exceptions, they are archaic forms, common in Plautus, but rare in the golden age, except in special connections.

I. Those in *so* and *sim* are readily explained: *dixim* = *dic-sisim* for *dic-isisim*; *duxim* = *duc-sisim* for *duc-isisim*; *auxim* = *aug-sisim* for *aug-isisim*. Here we have only the ordinary dropping of *is* before *s*, as in *dixti* = *dixisti*.

II. Those in *asso* and *assim*, which are very numerous, and

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those in *esso* and *essim*, which occur only in a few verbs, are generally explained as follows: *amas-so* from *amaviso* by dropping *i* and assimilating *v*, giving first *amav-so* and then *amasso*. So *rogavisit*, *rogav-sit*, *rogassit; habevisit*, *habev-sit*, *habessit*. But Pott objects to this view and maintains that *amasso* cannot come from *amavi*, but only from *amasi*. No trace of any such perfect has, however, been preserved. Indeed, Curtius and Corssen both regard the perfect in *si* as unknown to derivative verbs. The words of Curtius are: "Das Perfectum auf *si* ist den abgeleiteten Stämmen fremd"; and of Corssen: "Die lateinische Sprache bildet keine Perfecte auf *si* in den auf *ā*, *ē*, *ī* auslautenden verbal Stämmen." Corssen, however, while he thus rejects the suggestion of Pott, also takes exception to the more common explanation on the ground that *v* is nowhere else assimilated to *s*. He conjectures that *v* is dropped and *s* doubled in compensation.

But the views set forth in this paper seem to me to furnish a more natural and satisfactory explanation of these peculiar forms. We have observed that the tenses for completed action in most primitive verbs are formed by appending the corresponding tenses of *esi* to the verb-stem. Now if the future perfect and the perfect subjunctive of this auxiliary be appended to verb-stems in *a* and *e*, these archaic forms are at once produced. Thus, *ama-isiso*, *amasso* (*i* dropped); *roga-isisit*, *rogassit; habe-isisit*, *habessit*. Whether at this time the perfect indicative ended in *si* or *vi* makes not the least difference. The tense-forms *amavi*, *amaveram*, *amavero*, etc., are entirely independent of each other. They are all produced in precisely the same manner by appending the tenses of the auxiliary to the verb stem: *ama-fui*, *ama-fueram*, *ama-fuero*. *Amavero*, therefore, is not formed from *amavi* by appending *ero*, but from *ama-* by appending *fuero*, just as *amavi* is itself formed from *ama-* by appending *fui*. Accordingly the explanation of *amasso* as from *ama-isiso*, and of *amassim* as from *ama-isisim* does not at all involve a perfect *amasi*. Such a perfect may have existed, but it is not at all necessary to our explanation. The fact that different auxiliaries may be used in forming different tenses of the same verb is fully attested by such forms as *ama bam*, *ama-rem; dice-bam*, *dixi*.

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Our discussion seems to authorize the following conclusions:

I. The Latin, in common with all the cognate tongues of the Indo-European family, inherited a simple reduplicated perfect formed by appending the ordinary personal endings to the perfect stem, which was the root reduplicated. Among these primitive perfects was that of the auxiliary, originally *asasma*, which became in the Latin *esismi*, *esisti*, etc., finally shortened in the classical period to *esi*, *esisti*, *esit*, *esimus*, *esistis*, *eserunt* or *esere*. Thus were produced in this auxiliary the peculiar endings of the Latin perfect. From the stem *esis* were also formed all the tenses for completed action: *esismi*, *esisam* = (*e*)*s-eram*, *esiso* = (*e*)*sero*, *esisim* = (*e*)*serim*, *esissem*, just as from *es* were formed *esmi* = *sum*, *esam* = *eram*, etc. This, the original type of the Latin perfect, has not been preserved except in *esi*, a form used only in compound perfects in *si*: *carp-si*, *dixi*, etc.

II. The Latin, at a very remote period, formed a compound reduplicated perfect, together with all the other tenses for completed action, by appending the auxiliary *es* to the perfect stem. Thus *cecin-ismi* (= *cecini*), *cecin-eram*, *cecin-ero*, etc.; *fufu-ismi* (= *fufui*), *fufu-eram*, *fu-fuero*. But the auxiliary *fufui* finally lost the reduplication and became *fui*, *fueram*, etc. To this class belong all Latin perfects in *i*.

III. The Latin finally formed a new compound perfect, together with the other tenses for completed action, by appending the perfect of the auxiliary, together with its other tenses for completed action, to the verb stem, rarely to the present stem. Thus:

1. Most consonant stems appended the auxiliary *esi* with its other tenses for completed action: *carp-si*, *carp-s-eram*, etc.; *dix-i*, *dix-eram*.

2. *A*, *e*, and *i* stems and some consonant stems appended the auxiliary *fui* with its tenses: *amavi*, *delevi*, *audivi*, *alui*, *rapui*.

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## II.—*On an English Consonant-Mutation, present in PROOF, PROVE.*

By S. S. HALDEMAN,

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In 'proof' and 'prove' a surd consonant indicates a noun or an adjective, and a sonant one a verb, a feature which is more or less present in the following examples, extending to one hundred and fourteen pairs. The mark (*) prefixed indicates archaic or local forms.

| | | | |
|---|---|---|---|
| abuse *n.*, | abuse *v.* | dike, | dig |
| advice, | advise | drop, | dribble |
| analys-is, | analyse | duck, juke, | dodge |
| ascent, | ascend | excuse, | excuse |
| bath, | bathe | fros-t, | freeze |
| behoof, | behoove | gait, | gad |
| belief, | believe | gilt, | gild |
| bent *n.*, | bend | girth, | gird |
| brass, | braze | glass, | glaze |
| breath, | breathe | gloss, | gloze |
| bulk, | bulge | graff, | en-grave |
| calf, | calve | grass, | graze |
| *chast'ice†, | chastise' | grease, | grease |
| chief, | achieve | grief, | grieve |
| choice, | choose | grip, | grab |
| cicatrice, | cicatrise | grutch,§ | grudge |
| clack, | clang | half, | halve |
| click, | clink | halt, | hold |
| cliff, | cleave | hilt, | hold |
| close, | close | hiss, | whiz |
| cloth, | clothe | hoof, | hoov'd ‖ |
| concise *a.*, | incise | house, | house |
| crank, | cringe | kerf, | carve |
| cross, | cruise | leaf, | leave |
| delf (a mine), | delve | life, | live |
| device, | devise‡ | lip, | blab |
| diffuse, | diffuse | loath *a.*, | loathe |

† "As she from Collatinus wife
of chastice bore the bell."—*Turberville*, in Richardson.

‡ "That of this land's first conquest did devize."—*Spenser*.

§ "To whom he bore so fell a grutch,
He ne'er gave quarter t' any such."—'*Hudibras*.'

‖ "His horses hoov'd with flint."—*Henry King* (1591–1669).

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| | | | |
|---|---|---|---|
| loss, | lose | shafe‖, | shave |
| metamorphose *n.*, | -oze † | sheaf, | sheave |
| mouse, | mouse | sheath, | sheathe |
| mouth, | mouthe | shelf, | shelve |
| nip, | knab, nibble | sign, | resign |
| noose, | nooze | slip, | slive |
| ob-tuse, | con-tuse | smutch, | smudge |
| of-fice, | suf-fice | sniff, | snivel |
| paralys-is, | paralyse | sooth *a.*, | soothe |
| practice, | practise ‡ | (sprout), | browse |
| prem'iss, | premise' | staff, | stave |
| price, | appraise | stipe, | stab |
| profuse, | suffuse | strife, | strive |
| prom'ise, | com'promise | stuff, | *stive, steve |
| proof, | prove | swath, | swathe |
| purpose, | propose | teeth, | teethe |
| recipient, | receive | tenth, | ti..the |
| rap-acious, | rob | thief, | thieve |
| reef, | reeve | tractĭle, | drag |
| ref'use | refuse' | treat, | trade |
| relief, | relieve | triple, | treble |
| remiss *a.*, | remise | trōth, | betrōthe |
| rent *n.*, | rend | tussle, | touse |
| reproof, | reprove | tweak, | tweag |
| reproof, | reprieve | use, | use |
| rip, | rive | waif, | waive |
| sacrifĭce§, | sacrifice | wife, | wive |
| safe, | save | woof, | weave |
| scath, | scathe | wreath, | wreathe |
| serf, | serve | wreath, | writhe |

The pairs 'give' 'gift,' 'drive' 'drift,' do not belong here the *f* being due to the participial *t*, which is also present in 'descend' 'descent,' 'extend' 'extent,' 'portend' 'portent,' and many others. The noun 'hold' is often pronounced *holt*, and Chaucer has 'holte' for a strong-hold or castle.

The verbs 'bequeathe,' 'crave,' 'drowse,' 'lave,' 'rave,'

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† Geo. Edwards, Discourse on Birds, 1795, p. 14.

‡ "Practized."—*Spenser*, 1580.

"nought can be more disgusting to the wise,
than pride, which none but silly fools practise."
—*J. B. Gilchrist*, LL.D., 1821.

§ Compare 'orifice,' 'benefice,' 'artifice.'

. . . . . "In Dares' stead I offer this,
Eryx! accept a nobler sacrifĭce:"—*Dryden*, Æn. 5, l. 643.

‖ As in 'spoke-shafe,' a wheel-wright's implement.



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'please,' 'raise,' 'praise,' 'seize,' 'seethe,' 'soothe,' 'advertise,' and others, are not accompanied by surd nouns.

Many words are used both as nouns and verbs, without a change of form, such as 'glide,' 'rise,' 'slide,' 'slice,' 'scoff,' 'pace,' 'race,' 'revise,' 'exercise.'

In some cases a change of form would cause confusion with other words, as in 'cease' 'seize,' 'loose' 'lose,' 'bite' 'bide,' 'rip' 'rib,' 'dose' 'doze,' 'hiss' 'his,' 'lease' 'lees,' where the *z*-sound as a plural sign adds to the confusion.

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## III.—*On Begemann's Views as to the Weak Preterit of the Germanic Verbs.*

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The so-called weak preterit in the Germanic verbs has long been regarded as composed of the stem of the verb and the past tense of a strong verb from the root found in *da* in the Latin *condĕre*, ϑη in the Greek τίϑημι, and in the Sanskrit *dhâ*. This belief dates from the sharp investigations of Grimm, whose influence induced Bopp to abandon his previously adopted view of the derivation of this weak preterit from the past participle, and to accept the theory of composition. Bopp's supposition of the derivation of these preterits from the participle was doubtless suggested by the resemblance between these forms. This resemblance is marked in the regular verbs, but is striking in those verbs which form somewhat irregularly their preterit tense and past participle, viz.: the preteritive and a few others. Of the preteritive, *magan* (preterit *mahta*, participle *mahts*) may serve as an example. Of the others, *þugkjan* (preterit *thuhta*, participle *thuhts*). This resemblance is at first notice the most striking feature of these forms; and, as we have mentioned, it seemed at first to Bopp neither accidental nor incidental, but organic. The sharper sighted Grimm discovered a resemblance between

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these preterits and another form, so peculiar as to convey to his mind a notion of kindred more deeply rooted than that which was implied by the simple agreement of form between the preterit and the participle. This was the perfect agreement of the inflection endings in the dual and plural of the regular weak perfects with the endings which a strong verb of the second class from a stem ending in *d* would have in the preterit, and which the lengthened stem *bidjan* actually presents in the preterit. This resemblance becomes clear by a comparison of the preterits.

| Strong preterit, from *bidjan*. | Weak preterit, from *nasjan*. |
|---|---|
| baþ | nasida |
| bast | nasidês |
| baþ | nasida |
| bêdu | nasidêdu |
| bêduts | nasidêduts |
| bêdum | nasidêdum |
| bêduþ | nasidêduþ |
| bêdun | nasidêdum |

A resemblance so complete in the dual and plural naturally suggests an extension to the singular; and, time being given for the wearing away of the endings in the singular, what better hypothesis is there for the origin of the tense than the composition of the stem of the verb *nasjan* with the strong preterit *dad*, *dast*, *dad*, *dêdu*, *dêduts*, *dêdum*, *dêduþ*, *dêdum?* What is more natural than to refer to a stem allied with the Sanskrit *dha* and with ϑη in τίϑημι, this not wholly imaginary preterit, whose meaning, 'fixed' or 'placed,' combined with that of the stem of the verb whose preterit is to be analyzed, makes out in many cases so perfect a signification for the transitive preterit? This we know to have been Grimm's process of mind; and this explanation is so strongly supported by analogous facts in the history of language, especially as the evolutionists present that history, that the theory of composition has been practically unquestioned for fifty years.

Lately, however, the number of students in this field has increased, and the weight of those first names has somewhat lessened under the influence of discoveries and new theories, and naturally there is new investigation of principles long

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accepted. This hypothesis of Grimm's has, like others of long standing, been weighed anew. Begemann, one of the professors at the Academy for Modern Languages in Berlin, published in 1873 an able and learned pamphlet attacking the composition theory as affecting these preterits, and followed it up by a second treatise in 1874. The first pamphlet deals with the difficulties under which the composition theory labors or is said to labor, and proposes anew Bopp's first theory, that of derivation from the participle, applying it, however, to the dual and the plural, as well as to the singular. The second pamphlet deals largely with the possibility (strongly denied by many defenders of the old theory) of the derivation of an active transitive preterit from a passive participle, though many of the arguments of the first treatise are restated and newly fortified in the second.

One of the difficulties for the Grimm hypothesis of composition arises from the fact that it is at best no more than an hypothesis. As a theory to account for changes that took place in a period without literary records, it can never be anything but an hypothesis, probable enough, but never a demonstrated certainty. The same thing, however, must be said in regard to any other method of accounting for the formation of these preterits; and the question is, therefore, one of a choice between hypotheses. Which has in its favor more facts from the general field of linguistic growth and from the special field of the Germanic tongues?

It should be stated in the outset that the theory of composition advanced by Grimm and generally approved by Germanic scholars may be accepted in its outline, without committing the accepter to any one of the dozen different methods by which the details of the composition and its development have been evolved. One may fully believe in the validity of the composition theory, without accepting either Grimm's theory of an original ending *a* for the first person preterit singular of all strong verbs, or Holtzmann's original ending *dida* for the first person singular of the weak preterit, or Scherer's aorist in old German, or Grein's original *dads* for the second person singular, or Meyer's root *dadh*. Had the formation taken shape in a

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period on which literary records throw a clear light, the hypothesis either would become a demonstrated certainty, like the composition of the future and conditional in the Romance languages, or would be disproved. Conjectures in either event would be valueless. But these conjectures, worthless in such a condition, have for us now the value of possible modes of origin; and though nearly every investigator has failed at some point to provide full analogies from the Gothic or other Germanic tongues for each supposed process, yet the strength of one may perhaps in some relation be made to supplement the weakness of another; and certainly the pure and simple theory does not involve one of the crude evolutions which have been thrust upon it. It is then no overturning of the theory itself, if the bizarre methods of development which have been applied by indiscreet defenders in order to sustain it, are overthrown. Begemann has brought to this work a keen critical faculty, and the oversights and solecisms of all supporters of the composition doctrine are thoroughly exposed. An oversight, for instance, was without doubt the assertion of Bopp, that "in the second person singular of the Old High German *tâti* from *tatati* begins already the misunderstanding, and only the first and third persons, *tëta*, 'I did,' 'he did,' preserve the ancient standpoint with distinct and simple reduplication-syllable." For Bopp assumes for Gothic a stem *dad*, resting on an old reduplication of which the language is no longer conscious, and deduces from the plural *dâdun* of the Old High German a secondary root *dâd* and a present *didu*, and thus third person plural *dadadun*, contracted to *daadun*, *dâdun*. Now Begemann shows (and has, by correcting Bopp, done service for the friends of the composition theory) by a careful comparison of the preterit forms occuring in the oldest documents from Old Saxon *duan*, Anglo-Saxon *dôn*, that the forms like *dëda* and *dide*, which have been taken by some supporters of the composition theory to be early reduplicated forms, must be relatively late, and that the earlier forms were of the strong conjugation. In other words, just what the weak preterit in the Gothic verbs seems plainly to imply, viz.: that an auxiliary from the past of a strong verb

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has been received by and incorporated with the stem in *nasi-da*, *-dês*, *-da*, *-dêdu*, *-dêduts*, *-dêdum*, *-dêduþ*, *-dêdun*, is borne out by a comparison in Old Saxon and Anglo-Saxon of the earliest preterit forms of this auxiliary verb. The form *dadi* must be earlier than *dedôs* and *dâdun*, earlier than *dëdum;* and the verbal forms *dæde* and *dædon*, adduced by Grein, *should be* the remains of the strong verb in Anglo-Saxon. Weak forms supplanted these and, as in Old High German, so in Old Saxon, the second singular and all persons of the plural and the subjunctive bear the impress of the strong inflection. We have then in these dialects remains of that strong verb which the Gothic and Old Norse show us only in composition. We must thank Begemann that in rescuing *tëta* and *dëda* from over zealous advocates of the composition theory, he has established the right relation between the double forms in Old Saxon and the Old High German. We do this without in the least accepting his supposition for the origin of *tëta* in Old German. We hold that the exhibition of a reduplicated or other form of the root used to create the compound is of little importance in comparison with evidence for the composition itself; and the demonstration of an original strong preterit for the verb 'to do' in these three languages, Old Saxon, Old High German, and Anglo-Saxon (though in the latter the form rests on slender foundation from documents), is something gained for the composition theory. This may indeed prove that *nerita* is not contracted from *neri-tëta*, or even that *tëta* and *nerita* are precisely similar formations, but it also demonstrates that the strong tense *da*, *dês*, *da* (which most regard as shortened from *dad*, *dast*, *dad*), *dêdu*, *dêduts*, *dêdum*, *dêduþ*, *dêdun*, has its analogies in the sister, if not younger, dialects. Begemann seems himself to half suspect that he has helped the cause which he would oppose, for he says at the foot of page 19: "But thereby nothing is gained for the composition, for I have above shown that Old Gothic *nasidad*, *nasidast*, *nasidad*, could only become *nasidaþ*, *nasidast*, *nasidaþ*." The passage referred to, on page 9, concludes: "The rise of *nasida* from *nasidad* by the loss of *d* cannot be conceded. Also the deduction of *dês* from *dast* stands in contradiction to analogous

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forms: the verbs *qvithan*, *vairthan*, *anabiudan* have in the second person singular of the preterit *qast*, *varst*, *anabaust*. Here, and also everywhere else, has the *st* produced from a dental and *t* maintained itself. If accordingly *nasidast* had been the ground form, it would have remained uninjured. But if one would even grant the loss of the *t*, the transition from the created *das* (for *dast*) to the actually existing *dês* would be incomprehensible." That is, the analogies of the Gothic, as they are known to us, do not favor such a change. That is all of the "unbegreiflich" which the change involves. Begemann goes on from the passage just quoted to add: "Since now, however, the forms *nasida*, *nasidês*, *nasida*, actually exist in harmony with Old High German *nerita*, *neritôs*, *nerita*, we must unconditionally abandon the idea of composition for the singular." On page 15 of the introduction to his treatise "Zur Bedeutung des schwachen Präteritums" he adduces in order the arguments which have led him to reject the composition theory. The first is that "nowhere outside of the Gothic are the slightest traces of a composition to be discovered." In another place (p. 32) he says "surely we must presuppose everywhere (that is, in all numbers) composition and accordingly mutilation in the singular, or throughout connection with the participle and enlargement in the dual, plural, and subjunctive"! This, then, is the argument: "*Nasida* (Gothic) and *nerita* (Old High German) belong together." Again: "If *nasidêdum* is a compound, so is *nasida*, and *nerita* must also be one." Again: "The theory is false, because there is not a trace of the composition outside of the Gothic." Why should not the relation work both ways? If resemblance in form between *nerita* and *nasida* can be used against the composition theory in respect to *nasida*, because *nerita* is claimed to be no compound, why cannot the same evidence be used for composition in *nerita*, when *nasida* is claimed to be a compound, as the traces of composition in dual and plural indicate to many? This first argument begs the whole question. Begemann's own exhibition of a strong preterit from the verbs *duan* O. S., *dôn* A. S., *thuon* O. H. G., corroborates the assumed existence of an early strong preterit

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*dad* in Gothic, whose dual and plural are perfectly presented in the regular verbs. If *nasidêdum* is a composed form, Begemann himself admits that *nasida* must be composed. If *nasida* is composed, admitting that *nerita* "belongs with it," he ought to concede composition for *nerita*. Grimm held, and many now hold, that the singular *nerita* and the plurals *neritumês*, *neritut*, *neritun* show composition. It is a subjective dictum that there is no trace of a composition outside of the Gothic; and Begemann himself acknowledges that this argument by itself alone could decide nothing. Let me note in the argument that he seems to imply that there is something very like a trace of composition in the Gothic. This first argument properly stated covers the same ground as the third (p. 15 of the second treatise), but it involves much more. The third reason for rejecting the composition is the "impossibility of explaining the forms *nasida* Goth., *nerita* O. H. G., by composition." If, as the first reason declares, there is "no trace of composition outside of the Gothic," why is a single Old High German form *nerita* picked out and held up as an especial hindrance to the acceptation of the composition theory? It ought not to be. The difficulty is in the Gothic singular *nasida;* and if that difficulty were once removed, if in accordance with known laws of Gothic formation the singular *nasidad*, *nasidast*, *nasidad*, became *nasida*, *nasidês*, *nasida*, there would be traces enough of composition "outside of the Gothic." The first and third reasons are then to be reduced to the simple reiteration that it is "unbegreiflich" how *nasidad*, *nasidast*, *nasidad*, could become *nasida*, *nasidês*, *nasida*.

It is true that we find no Gothic forms older than the *nasida*, *-dês*, *-da*. Begemann holds that O. H. G. *nerita*, *-tôs*, *-ta*, are identical with them, as the *dagê* of the Gothic genitive plural strong declension is *takô* in Old High German. From the identity of these forms he infers that we must have the primeval Germanic form not so much back of these forms as in them. It is much the same line of thought when he rejects the ordinary view of the "lautverschiebung" and declares that the surd *p* in Old High German *puochâ* is older than the sonant

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*b* in Gothic *boka*. Surely it is more in accordance with the processes of language as they appear in the Indo-Germanic families, to infer rather that we have not in the singular *nasida* and *nerita* the primeval Germanic form than that we have. Attrition, mutilation, phonetic decay (whatever we call the process), would lead us to expect a *modification* of the form if composed of a stem and a strong preterit, and it is rather a surprise that no such modified form appears in the Gothic plural, than that there is such a mutilation in the Gothic singular. However, the correspondence of the endings with those of all strong preterits in the dual and plural may have produced from resemblance a tendency to continuance, and thus preserved them, while the final sonant *d* or aspirate þ seen in the strong preterit of the supposed stem *did* (as it is elsewhere found without an immediately preceding consonant only in the preterit of *bidjan*) had little or no class feeling to maintain it and might easily be lost. It is in accordance with the very nature of violent mutilations that they take place before or rather behind all literary record of them. When once the literary record begins, the conservative force is greatly augmented, and it by no means follows that the earliest documents show us the primeval forms.

The second reason which Begemann gives for rejecting the composition and assuming origin from the participle, namely, that in all the Germanic languages since the earliest times the closest formal relation has existed between the preterit and the participle of weak verbs, certainly has a serious aspect. It was this close relation, as has been noted, which induced Bopp at first to derive the preterit from the participle. For this close similarity but three possible methods of origin can be assigned: it may be accidental, or incidental, or organic. Considering the number and completeness of the agreements, not merely in the regular verbs of each class in Gothic, but also in the preteritive verbs and in those omitting the connecting vowel in the preterit, and likewise in other Germanic languages in cases where participles exist, an accidental resemblance cannot be assumed as accounting for *all* the agreements. There remain two other possibilities.



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The resemblance of form is, partly at least, either incidental or organic; that is, either it is the result of assimilations between the forms, or the one is derived from the other. Either of these suppositions would account for the resemblance. Under the influence of either the derivation of the preterit from the participle or the assimilation of the preterit to the participle, the coincidence of stem-form might be thus complete. The derivation of the participle from the preterit is not to be thought of, as the participle is the descendant of—rather the same as—the Sanskrit participle in *ta*, Greek *το*, *τος*, Latin *to*, *tus*. Begemann, in view of this uniform resemblance, holds the doctrine of assimilation to be unreasonable, and the doctrine of organic development of preterit from participle to be the only adequate solution of the form. In reality his second, fourth, fifth, sixth, and seventh arguments for his view are simply varying presentations of this one fact of striking resemblance between the two forms. His fourth argument is the impossibility of the origin of the primeval preterits *mahta*, *brahta*, *þahta*, etc., from the hypothetical ground-forms *magda*, *braggda*, *þagkda*. Of course the alternative thought is, that they can be perfectly accounted for by derivation from the participle. Grimm, Bopp, Schleicher, Leo Meyer, Moritz Heyne, and Holtzmann have all given an account of processes possible to form these preteritive preterits from the stem of the verb and the ending *da*. These explanations were independently conceived and are different from each other. Begemann reviews them and finds each account inadequate or unsupported by analogies, and some, notably that of Moritz Heyne, absurd. It is easy thus to throw contempt on the theory; but the theory is not responsible for the blunders made in its defence.

It is one of Begemann's points against the theory of composition that in the Gothic and the Old Norse this supposed auxiliary (*da*) does not exist in an independent form; and in his judgment its use as part of a supposed preterit compound and its existence in the substansive *dêds* Goth., *dâd* O. S., *tât* O. H. G., ought to have kept it alive if it originally existed. But not contented with the verb's non-

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existence in Gothic and Old Norse, he devises an original non-existence for it in the earliest forms of High German and Saxon dialects, and supposes it to have been derived in these languages from the substantive *tât* O. H. G., *dâd* O. S., after the separation of the two groups. Begemann's reasoning that its use as an auxiliary in the preterit would have preserved the verb, is for the Gothic and Old Norse. But the verb's existence in the Saxon and High German group weakens a little this argument. Therefore the gratuitous supposition of a late origin must abolish its early existence in the latter group. It did not occur to Begemann that the participles which in his judgment have generated the preterits must, by his argument against the original existence of a strong verb (*do*) in Gothic, be preserved. Where is the participle, for instance, that created *vissa*, the preterit of *vait?* Not in the Gothic, though the substantive *vissei* in Gothic presents a parallel to *dêds*. How could the participle generate the preterit and perish? If it could, why might not also the strong preterit of the verb 'to do' and the verb itself perish in spite of having been used to form the weak preterits?

Not satisfied with magnifying the actual sound-difficulties, which for us make the transition of the stems in the preterits of some of the preteritive verbs (when combined with the suffix *da* or *dad*) to their present form strange, Begemann invents difficulties in the case of *gamotan*, *vitan*, *kaupatjan*, by supposing that the strong preterit second person singular ended in *st* instead of *t*. The ending *st* as in *bast* (*bapt*), *vaist* (*vaitt*), has made the change from *gamot-da* or *gamot-ta* to *gamos-ta* seem natural. What support is there for the theory of a Gothic second singular ending *st*? the single anomalous reduplicated form *saisost*. Only this verb and dental stems have *st* in the second singular preterit. If the final letter of dental stems is dropped before Begemann's imaginary *st*, why should every other final stem-letter be able to turn out the *s*? When Begemann endeavors to brace himself up by the Old Norse usage, he finds no solid support. Bezzenberger ("Zeitschrift für Deutsche Philologie," vol. 5, p. 474) has given illustrations enough to prove that *z* in the second singu-

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lar of the Old Norse preterit is often a graphic representation of *s* and not a combination of a dental with the *s* of a personal ending *st*.

Begemann's fifth argument against the composition theory, that the plurals belonging to the oldest preterits *mahta*, *brahta* (*mahtêdum*, *brahtêdum*), can never have had a *dêdum* in their earliest shape, is not a whit different from the third. It is anew the statement that the resemblance between participle and preterit is organic. When, however, he adduces the enigmatical *iddjêdum* as an argument for his view, he seems to believe, because he has one form in which the part of the verb most suggestive of composition no longer begins with a lingual mute, that he is justified in claiming it as a new argument for the derivation of preterit from participle. Were the parent participle here, or could its form be undeniably assumed from the other Germanic languages as coinciding in stem with *iddja*, we might concede force to the argument. But as the participle should end in *t*, at least in a lingual mute, it seems necessary to get rid of the *j*, and accordingly in one place the *j* is compelled to become inorganic, "ein ableitendes *j*." Nevertheless the argument as a whole professes great respect for the age and pedigree of the *j*, though rather more honor is paid to *d*, perhaps because most of the advocates of the composition theory have regarded the *dd* as as inorganic before and generated by the *j*. Müllenhoff makes the form *iddja* come from Sanskrit *ija*, *ijâja*, and supports the inorganic evolution or assumption of *dd* before *j* by the Gothic genitive *tvaddjê* and the substantive *vaddjus*. Begemann begins a long way off with his intrenchments to lay siege to the enemy's camp, namely with the Sanskrit comparative, which he assumes was formed by *idj*; so also was it in the Greek, ἡδιδjων, ἡδιjων, ἡδίων, ἥδιον, and in Latin *mav-idjor*, *suav-idjor*, *suav-ijor*, *suavĭor*, *suavĭor*; Gothic *sut-idj-an*, *sut-izj-an*, *sut-iz-a*(*n*). To the side of the Gothic *tvaddjê*, the Old Norse *tveggja* is summoned; both (it is claimed) can have had a nasal form as forerunner; one was *tvandjê*, the other *tvengja*; both of these can easily have come from *tvangdjê*, and a Gothic *tvandjê* for *tvangdjê* is compared with Latin

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*quintus* for *quinctus* and has therefore "an absolutely certain analogue." *Fidvôr*, in Begemann's opinion, is perhaps from *fingdvor*, to which the corresponding Lithuanian form *keturi* certainly does not directly point. *Frijôn*, *fijan*, and the present optative of the Gothic *sein* (*sijan*) are also adduced to prove that there is a tendency to drop the *j*, though the greater number of cases in which it is preserved are said to show that it was "original." Therefore in *iddja* we must not suppose that a hypothetical *ija* has been loaded down with an inorganic *dd*. And at last Begemann tells us that he has the "boldness" to deny that the root *i* in Latin *ivi* (for instance) ever existed alone, and asserts that *d* has always belonged with it and that it was *idvi* formerly in Latin. Consequently, the *eode* of Anglo-Saxon belongs with *iddja*, and the mystery of the latter form is not merely cleared up, but its existence and descent disprove the composition theory. Begemann is by no means the first to connect Gothic *iddja* and Anglo-Saxon *eode*, and Grein's connection of the two words, deriving the Gothic from *idjan* or *ithjan*, and making *iddja* and *iddjêdum* transpositions for *idida* and *ididedum*, seems simpler than Begemann's primeval combination *dj*. Grein's theory (suggested also by Grimm) is mentioned in a note by Begemann, but we do not find any refutation of it in either treatise. Grein and Begemann are not very far apart in respect to this root. If the root is *id* and the *j* "ableitend," it is only in the evolution of the perfect that they differ, and Grein's hypothesis is worth just as much *for* the composition as Begemann's *against* it. Certainly no great argument can be drawn from *iddja* against the composition theory, for it is quite as explicable by this theory as by a reference to an unknown participle.

If, as Begemann says, *iddja* has been a "Schmerzenskind" to some of the composition champions, we must also concede to him a long parturition therewith, nor can we admit that his demonstrations are so convincing as to justify the statements on page 20 of the introduction to his second treatise. His poetical words are: "Men have played evil tricks with the poor innocent *iddja;* but why does it have the boldness

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not to be willing to adapt itself to the theory? For that it must atone in the straight jacket. Yet linguistic facts do not allow themselves to be adapted to measuring rules; *iddja* remains *iddja* and *iddjêdum* remains *iddjêdum;* the endings *a* and *êdum* do not allow their true force to be explained away; they exist to all eternity. This is the most brilliant confirmation of my view and at the same time a witness against the assumed *dêdum* not to be killed." "Linguistic facts do not allow themselves to be adapted to measuring rules"; yet an anomalous *iddja* without any generating participle shall be claimed as the "most brilliant confirmation" of the theory that the weak preterit is the offspring of the participle. "Linguistic facts do not allow themselves to be adapted to measuring rules"; but the anomalous form *saisost* may dictate a second singular ending to the preterits of all strong verbs, and what was "in Old Norse only an occasional usage" shall, departing from this single form, he claimed as "eine durchgreifende Regel" in Gothic. "Linguistic facts do not allow themselves to be adapted to measuring rules"; but the *ddj* in *iddja* may suggest *dj* as a newly discovered method of comparison, and impose it on entire classes of Indo-Germanic comparatives!

But the seventh and crowning reason for the derivation of the weak preterit from the participle is the "quite particularly weighty fact that by the derivation from the participle all difficulties present themselves as quite natural appearances, and in general all is in the fairest order." So it seems to Begemann, but he admits the difficulty of the element *ed* inserted according to his theory before the personal endings of the dual and plural. He calls this difficulty "ein unschuldiger waisenknabe," "an innocent orphan boy," in comparison with the difficulties that beset the composition theory. Why he did not call it a girl (it would have been a more poetic picture), I do not know; but he calls it innocent, because in his judgment the mistakes of the advocates of the composition theory are flagrant; and he calls it an orphan because he does not wish to acknowledge it as his. It is a foundling whose father must be made responsible for it, and is an

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insuperable hindrance to Begemann's withdrawing in triumph. Such a child unprovided and unaccounted for is an uncomfortable fact in his domestic economy. Begemann suggests its identification with the termination in *faheths*, but it is simply an accidental agreement of form without any support from analogy or meaning; indeed we understand him to claim that the meaning of the active preterit is deducible from the participle. Besides, not merely its appearance, but its appearance in dual and plural alone, just where it would belong on the correctness of that form of the composition theory which makes the *da* a strong preterit from the root *did*, is likewise a serious difficulty for Begemann to confront. Not to emphasize the fact that this increment, on our author's theory, would be anomalous in the Germanic languages, why should it appear simply in dual and plural? If the answer be that it is according to the analogy of strong preterits, we ask: Why then just the form *ed*? Does not this analogy with the strong preterits point to some intimate connection? and what will acount for the *ed* but an actual strong preterit with that very form in dual and plural? Nor is the disappearance of the *ed* (if it really is not present) in Old High German and the kindred dialects, as Begemann claims, against its original existence as part of the form. Why not also assume that the dual cannot exist in Gothic, as it does not occur in the other Germanic dialects? If the dual fell out in Old High German why not also the *ed* (or better the syllable following it), a part used for the subordinate purpose of inflection? It is no argument to assert that if *neritum* in Old High German had once been *neritâtum*, the *tâ* would never have fallen out. Such a claim is against the teaching of compounds and derivatives in the Indo-Germanic family. Why not say that the Latin and Celtic could not have formed futures by adding *bhu* to the root of the present (*predicabit*, *predchibid*), because the full form of the root is not retained? Why not deny that perfects in Latin and Celtic (*mansimus*, *rogensam*) are formed with the root *as?* Why not claim that *fuo* could never become part of a Latin perfect, and the present and imperfect of *habeo* in the Romance languages could never

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become in a mutilated form the endings of the future and conditional? Nor does it meet this objection to insist that the Germanic languages had another "betonungs-princip"; that the Old French *punir-avóns* is essentially different from Old German *neri-tâtum;* for the termination *ons* is the same for many first plurals, and for the French ear it must have been as necessary to discriminate between *avóns* and *soyóns*, as for the German ear between *tâtun* and *nâmun.*

One other difficulty in regard to the derivation of the weak preterit from the participle has been that of deriving an active, generally transitive, form from one commonly having a passive meaning. To remove this difficulty, which was but slightly considered in Begemann's first treatise, is the object of his more recent pamphlet, "Zur Bedeutung des schwachen Präteritums der Germanischen Sprachen." This treatise, like the other, shows great learning, and the collection of facts from the domain of the Indo-Germanic languages in regard to the relation of the active and the passive voices and the meaning of the past participle is valuable. Starting from the acute discussion of Dr. von der Gabelentz in the seventh volume of the proceedings of the Royal Saxon Scientific Society, who shows that the passive voice is rather a luxury than a necessity of language, Begemann endeavors to exhibit the evolution of the passive from the active. The first half of his treatise is devoted to the establishment of certain propositions. Passivity developes itself from activity through the medium of reflexiveness. Reflexiveness is expressed formally, or results from the conception ("vorstellung"), and remains unmarked. In the verb, the usage is various in this matter. In the noun, reflexiveness lies only in the conception. The first two of these propositions are virtually involved in Dr. von der Gablentz's discussion and illustrations of the passive in the Indo-Germanic family (pp. 527–535). Ingenuity and power in their fuller development cannot be denied to Begemann, but it is worthy of note that the analysis of any form favoring even remotely the composition theory meets summary condemnation from our author: thus, the aorist passive ἐτέθην, which is held by some grammarians to be a compound of the

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stem and the aorist ἔθην with the meaning 'I placed' (so that ἐτέθην would mean 'I placed to place myself,' 'I had myself placed,' or 'I was placed'), is pronounced to be simply a lengthened form of the aorist in ην. That the passive was developed from the active is possible. Many participles are cited in the second section of this treatise (pp. 92–124) from various languages of the Indo-Germanic group, in which an active meaning still inheres. From the Greek among others are mentioned τλητός, δυνατός, ἀδύνατος, ἑρπετός, λωβητός. From the Latin *potus*, *pransus*, *cenatus*, *peritus*, are familiar illustrations. When we come to the Gothic, Begemann's past participles with active meaning are few compared with those occurring in Greek, and into some of these few the activity is infused rather than inherent. Taking for instance *þaurfts*, the past participle of *þaurban*, which has the two meanings of 'needful' and 'useful' (if they are two), the activity of the latter 'that can be used' is not so prominent as to call for any explanation, or to go very far in accounting for the origin of active transitive preterits from past participles. However near one another active and passive may once have been, absolute original identity could not prove that, after they had once separated and the forms had received definite significations so opposed in nature as are the active and passive generally in the earliest records of our Indo-Germanic family, new forms of opposing meaning could be developed from either voice without any new element. This (if we understand the conditions) is the genesis which we are asked to accept, and this, even granting an age for the beginnings of Germanic speech surpassing that of the more eastern languages of the family, cannot become more than a doubtful possibility. It is also to be noted that the number of adjective-participles or participial adjectives having a meaning looking towards activity is much more numerous in the Middle High German than in the Old or the Gothic, though Begemann accounts for this by the comparatively abundant literary material of the Middle High German period.

It is from the highly interesting development of a participial perfect in the Iranian languages that Begemann derives his



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main analogies for the assumed development in the Germanic languages. The facts exhibiting this development are mostly taken from the works of Spiegel, and are clearly presented in the third section of our author's second pamphlet. A brief outline of the facts shows, even in the old Persian and Bactrian, the past participle assuming verbal functions with both active and passive meaning, though in the latter case the auxiliary 'to be' is commonly used with the participle, while in the former the participle is used alone. By the side of these forms the old tenses of past time exist and indeed greatly preponderate. But in the younger Hûzvaresh the old forms of past tenses have been completely supplanted by the past participle. The meaning of the participle is still either active or passive. In the latter case, as before, the auxiliary is commonly found, and often another auxiliary is added to the former. But number and gender have disappeared from the participle form. The person is ascertained from the connection or indicated by a pronoun. The auxiliary, if present, of course denotes it. In the somewhat younger Parsi there are the same relations, but this progress—that the participle when used for the first person singular has assumed the personal ending (Bopp regarded it as a form of the verb 'to be'), which is wanting, if elsewhere indicated. In the new Persian the development is completed. Separate auxiliaries are used for the active and the passive forms, but the old simple participle stem is used only as an active, and has adopted, after the fashion of the first singular in Parsi, personal endings for each person, except the third singular which remains in the stem-form. Striking as the facts are, they are not new, but have long been familiar to the students of Indo-Germanic speech, and most familiar to those who have most firmly believed in the composition of the Germanic weak preterit. Bopp's "Comparative Grammar" records the facts, though not with Spiegel's minuteness. If these facts show the possibility of the derivation of a preterit from the past participle, it is to be noted that the participle maintained and exhibited from the first the active meaning which Begemann is obliged to assume for the Germanic participle. It is true

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that in English active transitive verbs are in use from Latin past participles. It is true that in Middle and Old German present forms of transitive verbs have been in some cases derived from earlier participial or substantive forms, but to insist on the derivation of *all* transitive weak preterits from past participles whose early active meaning cannot be established, is to ask belief for something which the Iranian participle and its evolution cannot make probable. Nor does the illustration of the development of a preterit from a Hungarian participle (in a language that belongs to an entirely foreign family), though it is in its nature more analogous to the hypothetical development in the Germanic group, bring much support to the theory. Much nearer to the Germanic than the Persian even is the Slavo-Lithuanian branch of the Indo-Germanic family. It is the connecting link (if we may accept the statements of its expounders) between the Germanic and the Aryan members, both by grammar and word-fund. From the Lithuanian the composition theory receives a strong confirmation. Its imperfect of customary action is composed of the stem of the verb and the form *davan*. Whether this form be from the root *dha*, 'to place,' or not, there can be no doubt that this imperfect is formed by a composition of the stem with a past tense. However this intimate relationship between the Slavo-Lithuanian and the Germanic languages may be explained, whether by the influence, in a period later than the development of both languages, of Germanic authority over the Slavo-Lithuanian family, or by a community of the two stems at a period previous to the perfect development of either language, the *suk-davan* of the Lithuanian belongs with the *sôk-i-da* of the Gothic. The probability of the development of the Germanic weak preterit from the stem of the verb combined with the strong preterit of a verb from root *dha*, 'to place' or 'to do,' can hardly be doubted by one who gives proper weight to the formation of the Lithuanian imperfect and the relation of the languages of this group to the Germanic.

How then is the resemblance between the preterit and the past participle to be explained? If it be not accidental nor

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organic, it must be incidental, it must have come from assimilation. Bopp's idea of a "Schutzbündniss," a "defensive alliance," has a poetical sound and is perhaps a fanciful presentation, but there must be truth behind it. Especially in the preteritive verbs, verbs of such great scope and repeated usage, there would be a constant tendency to assimilate a newer preterit *kunda* to an established form *kunþs*, *magda* to *mahts*, *þaurbda* to *þaurfts;* and in cases where no participle had continued, analogy might have great force. In the case of the regular verbs, the resemblance is more apparent than real. Otherwise why should the termination *a* of the preterit go over into Old High German, and maintain itself so firmly, whereas the *a* of the present becomes *u*? Certainly there was something in that *a* of the preterit besides a simple personal ending or the *a* of a participial stem. This theory of assimilation finds analogies enough in the development of languages. Not to turn aside from these preteritive verbs, we see in English that the *l* in 'would' has forced its way into the preterit of 'can.' Into the present of *will* in old English the *o* of the preterit forced its way and produced a present *wol*, *wole*, which we have in 'I won't,' *I wol not*. That in the primitive period such an assimilation should take place, in case the meanings did not greatly differ, is quite conceivable. It is probably on this very ground of assimilation that we are to account for the loss of the final consonant in the singular, that is, the assimilation of the personal endings of the preterit to those of the present. Nor is there anything surprising in a double assimilation, an assimilation of personal ending to personal ending between preterit and present, and of preterit to participle in stem-form. That *nasidad* should become *nasida* by the side of *nasja*, and *nasidast* should become *nasidas* or *nasidês* by the side of *nasjis* is natural, and the theory of assimilation to the present, and a lengthening of the *a* in the second person singular seems more reasonable than Delbrück's ("Zeitschrift für Deutsche Philologie," vol. I., p. 128) assumption of a strong preterit *dad* resting upon *dadád* with the accent in *dádast* on the reduplication syllable after a Sanskrit analogy. To this assimilation the similarity

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of the singular personal endings of the present in both strong and weak verbs might contribute, and the subordinate relation of the second part of the compound would invalidate any argument for permanence of form in the terminations derived from the strong preterit and hence sustained by a class feeling. The permanence in Old High German of the Gothic *a*, *ês*, *a*, as *a*, *ôs*, *a*, while the present ending *a* of both strong and weak verbs is reduced to *u*, is an evidence that the *a*, *ês*, *a* is something more than a simple personal ending. How *neritâtum* could become *neritum* may be to some inexplicable. We do not so regard it, and Seiler's explanation on p. 455 of "Beiträge zur Geschichte der deutschen Sprache and Literatur" has much in its favor. That such a change did take place will be probable to him who carefully weighs the following considerations.

1. The Gothic dual and plural *nasidêdu*, *nasidêdum*, presuppose a singular *nasidad* compounded of the stem *nasi* and a strong preterit *dad*. This singular we have in the form *nasi-da*, *-dês*, *-da*. Corresponding with this, identical with it, we have *neri-ta*, *-tôs*, *-ta*, in Old High German.

2. The increment in the Gothic dual and plural cannot be accounted for on the supposition that the preterit is derived from the participle. Least of all can we thus explain the particular form *ed*, which is identical with the syllable corresponding in strong preterits derived from a stem ending in *d*.

3. The loss of a part of the stem or ending of the auxiliary, or a contraction or mutilation of the appended verb in the Old High German plural is natural, especially as the loss of the Gothic dual in the other Germanic languages shows an increasing tendency to disregard the fulness of the old inflectional forms.

4. The composed forms in Slavo-Lithuanic, the imperfect in *davan* and the participle in *damas*, nullify any probability of the derivation of the Germanic weak preterit from the past participle which might be deduced from facts in the Persian and Hungarian languages, as the Slavo-Lithuanian is the connecting link between the Germanic and the Aryan and much nearer the Germanic than the Iranian, and the Hunga-

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rian (which belongs to the Finnish class) is still farther removed from the Germanic.

5. To render assurance still surer, it is noted that the transitive meaning of the Iranian participle finds no analogue in the Germanic.

6. The persistence of what are called the personal endings of the singular in the weak preterit in Old High German and Old Norse, involves fuller vowels and stronger elements than those of the ordinary personal endings (in the present for instance) will account for.

7. For the close resemblance of the stem-form in past participle and preterit, the theory of assimilation in the more striking cases is adequate. The close resemblance may then be incidental without excluding the possibility that in less striking cases, as in *nasida*, it is accidental. The accidental resemblance may have promoted the incidental.

8. The anomalous form *iddja* connected by etymologists doubtfully with Anglo-Saxon *eode* can just as well, even better, be regarded as a transposed form for *idida* and claimed as harmonious with the composition theory, than made a main foundation of Begemann's view, especially as no generating participle can be exhibited. It is not the enigmatical exceptions, but the prevailing regularities, that are most valuable in discovering a principle of form-genesis.

Even if we accept Begemann's ingenious explanation of the forms characterized by the *rückumlaut*, and regard them as of equal age or older than the regular weak forms, *santa* for instance as equally old with *sentita*, this does not establish the derivation of the preterit from the participle. Begemann's full and doubtless accurate collections of forms certainly indicate an age for the forms with the *rückumlaut* no less than that of the regular forms; but if these forms are even older than the more regular ones, when both occur, and if they agree with the participle, nothing justifies us in claiming that they are not compounded or in regarding them as an argument against composition. That the Old High German *dursta* by the side of Gothic *þaursida* is from an obsolete present *durran* or *dursan*, and is of greater age than the Old Saxon *thurstida*,

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even if it is proved, does not demonstrate that it itself is not a compound. On the contrary the advocate of the composition theory has the same right as Begemann to suppose two preterits developed at different times from or analogous to different stems, and is *not* obliged to contract Old High German *heftitatun*, corresponding to Gothic *haftidêdun*, into the West German *haftun* in order to enlarge it again to *heftitun*. The results of Begemann's investigations into the relations of these forms, even if correct, do no more than convict some advocates of the composition theory of inaccuracy in respect to the *time* of development of the forms. All that he has proved may be brought into harmony with the composition theory.

The object of this article causes us to stop short of any examination of Begemann's views in regard to the *ablaut*, the *lautverschiebung*, and the personal endings of the Indo-Germanic verb. But it may be remarked that our author is nothing if not revolutionary, and we may be thankful for the discussion of these old questions. We expect that this attempt at revolution, like every other that rests on any partially legitimate protest, will result in a readjustment of some relations between contending parties, but are confident that this bold assault on the composition theory, as applied to the weak preterits, will only show that the foundations cannot be shaken.

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## IV.—*On Some Forms of Greek Conditional Sentences*

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I did not happen to be present at the meeting of the Association at Easton, and I do not, therefore, know whether the paper which Professor Goodwin read on the Forms of the Greek Conditional Sentence was subjected to any criticism at the time. On reading it, however, lately, I was struck by a certain statement in it which appeared to me questionable, and I accordingly submitted the point I refer to to some examination; not, I confess, as thorough as I could wish, but still sufficient to confirm me in the opinion I at first formed; and I should be glad, therefore, to place the matter before you in order that the facts may be tested by the judgment and reading of other members of the Association.

It is well known that Professor Goodwin, in his book on the Greek Moods and Tenses, and also in his Grammar, advances the opinion that there is no *essential* difference between the expression of a condition by ἐάν with the Subjunctive, and the expression of the same by εἰ with the Optative. He has felt the extreme difficulty of *defining* the exact implication of one of these as contrasted with that of the other; and has been driven accordingly to the conviction that such difference as there is consists only in the degree in which a certain quality which he calls "vividness" attaches to one or to the other. He finds that a condition may be expressed with a low degree of vividness by εἰ with the Optative; with a greater degree of it by ἐάν with the Subjunctive; while it is possible to express the same condition with a still greater degree of vividness by εἰ with the future Indicative; these three kinds of expression presenting, as it were, a positive, a comparative, and a superlative degree of vividness to the choice of the writer. He says ('Transactions,' p. 70): "The Optative in ordinary protasis is merely a vaguer or less vivid form than the Subjunctive for stating a future supposition, bearing a relation to the Subjunctive somewhat similar to that

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which the Subjunctive itself bears to the future Indicative. Thus we have three forms which may be used to express a future condition, differing essentially only in the vividness with which they state the supposition: *εἰ γενήσεται, if it shall happen; ἐὰν γένηται, if it happens* (i. e. *shall happen*); and *εἰ γένοιτο, if it should happen.*" And he remarks elsewhere that it evidently makes little difference in English whether we say "if he shall do this, it will be well," or "if he do this, it will be well," or "if he should do this, it would be well."

I think that a large amount of the assent which the Professor's views on this point have received is due to the fact that, when he places these forms in close connection with each other, he makes use of such vague expressions by way of illustration. "Do this" may mean anything, *possible*, *probable*, *likely*, or *impossible;* and since we are consciously or unconsciously aware of this, we feel that each of the forms of condition quoted would under certain circumstances be appropriate; and we do not, therefore, deny in our thoughts the substantial equivalence of the expressions, though it would be found that in practice they are used by no means without a conscious or unconscious discrimination. If this is true in English, I think it can be shown that it is far more true in Greek. But to show this we must not take such an example as *εἰ τοῦτο γένοιτο, καλῶς ἂν ἔχοι* to contrast with *ἐὰν τοῦτο γένηται* (or *εἰ τοῦτο γενήσεται*), *καλῶς ἕξει*, but must try to find actual sentences of which the material character is so distinct that the speaker or writer must have been conscious of it. And here I may say that I think the paper read at the Hartford meeting of the Association by Professor Sewall, though I agree in general with its conclusions, is less convincing than it might be, from the fact that the examples chosen are in several instances such as might with propriety be stated in more than one way. For example: in Dem. Phil. i. 25, *εἰ γὰρ ἔροιτό τις ὑμᾶς, εἰρήνην ἄγετε, ὦ ἄνδρες Ἀθηναῖοι; μὰ Δί' οὐχ ἡμεῖς γε, εἴποιτ' ἄν*, the same sense might have been expressed—not, I grant, so properly, but still without absurdity—by *ἢν γὰρ ἔρηταί τις, ... φήσετε.* And in the passage from the 29th section of the same speech, which



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Professor Sewall himself presents in two forms, a very slight change in the aspect in which the matter is regarded would make either mode appropriate. I propose to give one or two illustrations of conditional sentences in what I look upon as matter suitable to decide the question whether there is or is not any essential distinction of import between hypothetical sentences employing ἐάν with the Subjunctive, and those which exhibit εἰ with the Optative; and I will then come to the particular statement in Professor Goodwin's paper which attracted my attention and set me on this method of inquiry. I will, however, first state the four forms of particular suppositions together, in what I conceive to be their proper gradation, and will characterize each by a descriptive epithet and symbol which may hereafter facilitate reference.

The Hypothetical Period, i. e. the condition (or Protasis) together with the conclusion (or Apodosis), in all cases asserts the dependence of the conclusion upon the condition. Then we have:

Form α, or the *Logical* form, in which nothing more is implied than this logical dependence: εἰ τοῦτο γενήσεται, καλῶς ἕξει.

Form β, or the *Expectant* form, in which, beside the logical dependence, is implied an *anticipation* of the possible realization of the condition: ἐὰν τοῦτο γένηται, καλῶς ἕξει.

Form γ, or the *Ideal* form, in which, beside the logical dependence, is implied an *imagination* of the possible realization of the condition: εἰ τοῦτο γένοιτο, καλῶς ἂν ἔχοι.

Form δ, or the *Unreal* form, in which, beside the logical dependence, is implied a *negation* of the possible realization of the condition: εἰ τοῦτο ἐγένετο, καλῶς ἂν ἔσχεν.*

I will take first a passage which Professor Goodwin quotes, I think, in both his books. At the beginning of the Agamemnon of Aeschylus, the watchman on the roof of the palace at Mycenae indicates his fear that on the return of Agamemnon

* Of course the examples given are used for illustration merely, and are not intended to establish the correctness of the import attributed to each. I should say that I borrow the terms "Logical," "Ideal," "Unreal," from Professor Gildersleeve's Latin Grammar.

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he will not find things within the palace just as he would like to have them. But he dares not speak out himself (v. 36):

> *τὰ δ' ἄλλα σιγῶ· βοῦς ἐπὶ γλώσσης μέγας*
> *βέβηκεν· οἶκος δ' αὐτός, εἰ φθογγὴν λάβοι,*
> *σαφέσταθ' ἂν λέξειεν.*

Now in this case will the Professor assert that it would have been possible for the watchman to have expressed himself in the Expectant Form or Form $\beta$? thus:

> *οἶκος δ' αὐτός, ἢν φθογγὴν λάβῃ,*
> *λέξει σαφέσταθ'.*

He makes a supposition in regard to the future, but it is in a matter which, he must have been conscious, rendered the realization of it impossible; and he found in Greek a form of stating his supposition which conveyed the impression that it was one wholly ideal, and therefore employed it. Here in English too we must say: "If the house itself should (*or* were to) find a voice, it would speak most clearly," and in Latin we must say: "*Si ipsa domus vocem capiat, planissime loquatur*"; and in each language the substitution of the forms which are asserted to differ only in "vividness" from the Ideal form must be felt to be wholly inadmissible. I may quote here PLAT. Protag. 361 A, as it contains a supposition precisely like that of the watchman, and expressed, of course, in the same way: *καί μοι δοκεῖ ἡμῶν ἡ ἄρτι ἔξοδος τῶν λόγων ὥσπερ ἄνθρωπος κατηγορεῖν τε καὶ καταγελᾶν, καὶ εἰ φωνὴν λάβοι, εἰπεῖν ἂν ὅτι Ἄτοποί γ' ἐστέ, ὦ Σώκρατές τε καὶ Πρωταγόρα.* Could Socrates possibly have said, *καὶ ἢν φωνὴν λάβῃ, ἐρεῖν?* *

Again, in the Clouds of Aristophanes, after Strepsiades, in despair of inducing his son to place himself under the instruction of Socrates, has decided (*invita Minerva*) to go to school

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* It may be worth while here to illustrate the Latin usage in such cases of *sermocinatio.*

CIC. Cat. i. 19: *Haec si tecum, ut dixi, patria loquatur, nonne impetrare debeat?* and so in § 27 without Apodosis: *Si mecum patria sic loquatur.*

Auct. ad Her. iv. 66: *Si nunc haec urbs invictissima vocem emittat, non hoc pacto loquatur? . . . si nunc L. ille Brutus reviviscat et hic ante pedes vestros adsit, non hac utatur oratione?* It is true that in Div. in Caec. 19, Cicero says: *Sicilia tota, si una voce loqueretur, hoc diceret, . . . si universa, ut dixi, provincia loqui posset, hac voce uteretur.* But he here substitutes, not the form which Professor Goodwin regards as essentially the same as that employed in the former cases, but the Latin

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himself, Socrates submits him to an examination which only reveals the hopeless imbecility of the old man. After much questioning and severe denunciation of his pupil's stupidity, Socrates insists that he shall set his own brains to work and find out for himself some device by which he may hope to get rid of the fatal necessity of paying his debts, to attain which end was the purpose he had in view in enrolling himself as a member of the school. At last the old man cries out that he has it: that if he could buy a Thessalian enchantress and draw down the moon from the sky, and keep her locked up like a mirror in a close box—"Then what?" says Socrates; and his answer is (v. 754):

> *εἰ μηκέτ' ἀνατέλλοι σελήνη μηδαμοῦ*
> *οὐκ ἂν ἀποδοίην τοὺς τόκους.*

Here again I ask: Does any one suppose, if Strepsiades had stated his plan thus:

> *ἢν μηκέτ' ἀνατέλλῃ σελήνη μηδαμοῦ*
> *οὔπως ἀποδώσω τοὺς τόκους,*

that Socrates would have found himself able to tolerate the old man's arrogant stupidity for some fifty lines longer, as he does? That whole passage is full of conditions expressed by *εἰ* with the optative; but I select, as before, this one as being made in a matter which the speaker must have been conscious was unalterable. I will quote shortly two or three more passages which seem to me to resist as strenuously as the ones already cited a change into the forms supposed to be essentially equivalent, which I suggest as possible alterations.

Aesch. Pers. 431:

> *κακῶν δὲ πλῆθος, οὐδ' ἂν εἰ δέκ' ἤματα*
> *στιχηγοροίην, οὐκ ἂν ἐκπλήσαιμί σοι.*

Could the messenger have spoken thus?

> *κακῶν δ' ἐλεινὸν οὔποτ', ἢν δέκ' ἤματα*
> *στιχηγορῶ σοι, πλῆθος ἐκπλήσω λόγοις.*

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equivalent of Form *δ*, which the character of the supposition clearly entitled him to do, just as in the converse way, in Ter. And. ii. i. 10, Charinus says (in the Ideal form): *Tu si hic sis, aliter sentias;* when the sense would have justified the Unreal form *esses—sentires;* since, as Madvig says, by a turn of rhetoric an impossible thing is represented as if it might take place.

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PLAT. Rep. ii. 359 C: *εἴη δ' ἂν ἡ ἐξουσία ἣν λέγω τοιάδε μάλιστα, εἰ αὐτοῖς γένοιτο οἵαν ποτέ φασι δύναμιν τῷ Γύγου τοῦ Λυδοῦ προγόνῳ γενέσθαι.*

Could Plato have made Glaucon state his illustration thus? *ἔσται δ' ἐξουσία . . . . ἣν αὐτοῖς γένηται.*

PLAT. Euthyd. 299 D: *εἴη ἂν εὐδαιμονέστατος εἰ ἔχοι χρυσίου μὲν τρία τάλαντα ἐν τῇ γαστρί, τάλαντον δ' ἐν τῷ κρανίῳ, στατῆρα δὲ χρυσοῦ ἐν ἑκατέρῳ τῶφθαλμῷ.*

Could this have been written thus? *ἔσται εὐδαιμονέστατος ἣν ἔχῃ . . .*

A passage in the Phaedo, 72 B C, which is too long to quote, contains a number of imaginary conditions expressed in Form γ which could not possibly be converted into Form β without being felt to be incompatible with the argument. Compare also Phaedrus 245 D. The necessity of the employment of Form γ to express conditions of this character is implied, moreover, by such passages as that in ARIST. Rhet. iii. 10, 7: *ὥσπερ Περικλῆς ἔφη, Τὴν νεότητα ἀπολομένην ἐν τῷ πολέμῳ οὕτως ἠφανίσθαι ἐκ τῆς πόλεως, ὥσπερ εἴ τις τὸ ἔαρ ἐκ τοῦ ἐνιαυτοῦ ἐξέλοι,* and such as the hopeless wish of the slave in ARIST. Pax 21, *πόθεν ἂν πριαίμην ῥῖνα μὴ τετρημένην*; though in these the condition is only suggested.

I come now to the particular statement in Professor Goodwin's paper which I venture to think erroneous. He quotes two or three times the English proverbial expression, "If the sky falls, we shall catch larks"; and on one occasion he says (p. 64) that if we translate it into Greek we must use *ἐάν* and the Subjunctive (Form β). Now I grant, of course, that the nearest Greek equivalent to that collection of English words, no regard being had to the matter, would be the form he names. But if any one who used the English line were asked to explain his meaning he would certainly interpret it by "if the sky should (*or* were to) fall, we should catch larks," and not by "if the sky shall fall, we shall catch larks." And it seems to me that the form the expression has taken in English has been determined by the use that is made of it. For it is, I think, always employed with the purpose of making it clear to some one that he has been flattering himself with a hope which depends upon conditions practically impossible, or

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excessively improbable; and therefore the repartee is expressed for rhetorical effect in precisely the same form as that in which the hope was couched. However this may be, I feel convinced that it would never have been expressed by a Greek (unless indeed he was speaking after the manner of a prophet) in the form which the Professor assumes to be the correct one; and I think this will be evident to all, if the line be rendered into Greek in the three modes open to us for future suppositions. Thus:

Form α:

*εἰ δὴ πεσεῖται τοὐρανοῦ γ' ὁ κύτταρος,*
*τὠρνίθια ληψόμεσθα.*

Form β:

*ἢν δὴ πέσωσιν οὐρανοί, μάλ' εὐχερῶς*
*τὠρνίθια ληψόμεσθα.*

Form γ:

*εἰ δὴ πέσοιεν οὐρανοί, μάλ' εὐχερῶς*
*τὠρνίθι' ἂν λάβοιμεν.*

Now it seems to me that, of these, Form γ alone expresses what is really implied by the English proverb; that Form α might be employed for the same rhetorical purpose as that which has, in my opinion, determined the form of the English expression; but that Form β involves a consciousness of the possibility of realization which would have prevented any Greek from using it, unless indeed he was speaking as a prophet.

The conclusion which I draw from this examination is, that when a future supposition is made in such a matter as compels the consciousness that it cannot be realized, εἰ with the Optative (Form γ) is the form *necessarily* employed. This region is that governed by the unchanging laws of the physical universe; and, while on such a question I wish to speak with all possible deference to the greater learning and wider reading of others, I venture to express a strong opinion that no case can be adduced from the best writers in which a future supposition demanding for its fulfilment *a violation of physical laws* is expressed by ἐάν with the Subjunctive (Form β). I do not affirm that none such can be found with εἰ and the future

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Indicative (Form α); for, as I have before said, I consider that when the Indicative is used in both clauses of the Hypothetical Period, all consideration of the matter is left out of sight, and the possibility of the realization of the condition is neither imagined nor anticipated; and, therefore, as any sort of future supposition may conceivably be expressed by εἰ with the future Indicative, it is possible that some passages may be adduced which really suppose a violation of physical laws and yet have not the Optative. I have not, however, myself lighted upon any such. In regard, then, to future suppositions, I assume as a fixed limit on the side of improbability a supposition of the violation of natural law, and this, I believe, is always expressed by εἰ with the Optative (Form γ).* Then in cases which come short of this, exactly in proportion as the writer or speaker wishes to leave his supposition in the region of the ideal, whether from a consciousness of its extreme improbability or from a modest and courteous understatement or withdrawal of his own opinion, in that proportion is he likely to use this same form; while in all cases when he either feels or wishes to express his belief that his supposition will be realized, or that at least *time will show* whether his anticipation is well grounded or not, he will employ ἐάν with the Subjunctive (Form β). If this conclusion is correct, it must, I conceive, be agreed that the forms in question differ from each other, not in the degree merely in which they possess vividness or any other quality, but in kind and essentially.

It is manifest that upon the view here maintained, there must be a large number of cases which admit of being stated in both ways without any very important, or at least any very striking, difference. One of these Professor Goodwin in his

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* An example of a supposition, not indeed physically impossible, but so improbable that it may practically be regarded as impossible, is in Xen. Anab. iii. 2, 34. *οἶδα γὰρ ὅτι καὶ Μυσοῖς βασιλεὺς πολλοὺς μὲν ἡγεμόνας ἂν δοίη, πολλοὺς δ' ἂν ὁμήρους τοῦ ἀδόλως ἐκπέμψειν· καὶ ὁδοποιήσειέ γ' ἂν αὐτοῖς, καὶ εἰ σὺν τεθρίπποις βούλοιντο ἀπιέναι.* It is not credible that if Xenophon had said, *ὁδοποιήσει γε αὐτοῖς, καὶ ἐὰν σὺν τεθρίπποις βούλωνται ἀπιέναι,* his hearers would have merely felt a greater amount of vividness in the statement of the supposition. It is plainly inconceivable that he could have so expressed himself.

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paper (p. 70) refers to, by way of showing that "the essential distinction is merely one of vividness of expression or distinctness in the form of the supposition, entirely apart from any difference of the speaker's opinion." As this passage seems to me a good one to illustrate what I have just said, I shall venture to quote it at length. It is in Dem. Phil. i., § 14 (p. 43). He has been urging upon the Athenians the necessity of action; he asks them what they are waiting for; what they expect to hear; and then he says: *τέθνηκε Φίλιππος; οὐ μὰ Δί', ἀλλ' ἀσθενεῖ. τί δ' ὑμῖν διαφέρει; καὶ γὰρ ἂν οὗτός τι πάθῃ, ταχέως ὑμεῖς ἕτερον Φίλιππον ποιήσετε, ἄνπερ οὕτω προσέχητε τοῖς πράγμασι τὸν νοῦν· οὐδὲ γὰρ οὗτος παρὰ τὴν αὑτοῦ ῥώμην τοσοῦτον ἐπηύξηται ὅσον παρὰ τὴν ἡμετέραν ἀμέλειαν. καίτοι καὶ τοῦτο· εἴ τι πάθοι καὶ τὰ τῆς τύχης ἡμῖν ὑπάρξαι, ἥπερ ἀεὶ βέλτιον ἢ ἡμεῖς ἡμῶν αὐτῶν ἐπιμελούμεθα, καὶ τοῦτ' ἐξεργάσαιτο, ἴσθ' ὅτι πλησίον μὲν ὄντες ἅπασιν ἂν τοῖς πράγμασι τεταραγμένοις ἐπιστάντες ὅπως βούλεσθε διοικήσαισθε.* Professor Goodwin asks in reference to this passage: "Did Demosthenes imply that there was any nearer prospect of decision on the question of Philip's death when he referred to it in the words *ἂν οὗτός τι πάθῃ*, than when he repeated his supposition in the very next sentence in the form *εἴ τι πάθοι?*" I should answer: Certainly not; but the reason of his passing from one form of the condition to the other is perfectly clear notwithstanding. Demosthenes has just referred to the report of Philip's sickness; and with this consideration in his mind, with the consciousness of the existence of a cause adequate to produce a certain effect, he naturally makes use of that form of the Hypothetical Period which suggests that the speaker has in view "an *anticipation* of the possible realization" of the condition: "If this sickness shall really prove one unto death, you will, I fear, with your supine inactivity soon make another Philip for yourselves; for it is more through your inertness than through his own strength that he has grown so great." Demosthenes then looks at the matter in a more general way, without any notion of the occurrence of Philip's death as being actually not unlikely, and contemplates it merely as an ideal matter, with merely "an *imagination* of the possible realization" of his supposition, and in

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consequence employs a different form of the Hypothetical Period: "And yet look at this: if he were to die, if fortune were to play into your hands in this way, how easily would you, if, as I advise, you were on the spot with a competent force when affairs there were in confusion, manage matters to suit yourselves." It appears to me that the distinction here suggested is one which lies on the face of the Greek, and which accords perfectly with the import of these forms as manifested in numberless other passages.

In these remarks on the thought implied by the two forms of condition in question, I do not imagine that I have attributed to them any other import than that *intended* to be expressed by the phraseology which has been employed by the writers on Greek Grammar referred to by Professor Goodwin. I conceive that I have only suggested a means of testing whether there does really exist between them a distinction in essence and fundamental, or whether they differ from each other merely in regard to "vividness" of expression. This word seems to me to be sadly in need of elucidation; and I shall be glad if what I have said may induce Professor Goodwin to add to the great services he has already rendered to students of Greek by clearing up the obscurity which I find in it.



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## V.—*On Verb-Reduplication as a Means of Expressing Completed Action.*

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It is a matter of no little surprise that in all that has been written upon Indo-European Philology during the last thirty years, we can nowhere discover any full treatment of Verb-Reduplication. In the "Comparative Grammar" of Bopp, the "Compendium" of Schleicher, the "Moods and Tenses" of Curtius, and the "Doppelung" of Pott, we may find attempted and partial explanations of it, but nowhere can be found any full and satisfactory discussion of the rise, the function, and the history of this form. The tense-formations of later growth have been quite critically analyzed, and their origin and primitive significance have been determined with considerable accuracy; yet so little has hitherto been done upon this form by those best fitted to tell us of its history, and so many elements of yet doubtful origin enter into its composition, that it is with no little diffidence that we venture to lay before this Association the results of our examination of the subject.

It is not our purpose to discuss the whole subject of verb-reduplication, but to speak only of the *genesis*, the *history*, and the *decay* of reduplication as a means of expressing completed action, alluding to the general subject of reduplication only so far as it may serve to elucidate our more limited theme. For the sake of greater clearness we may at the outset be permitted to state what we hope in this paper to accomplish. We shall endeavor *first*, to explain the origin of this reduplicated form, and how it came to possess the signification of completed action; *secondly*, to note its rapid growth and extension; *thirdly*, to trace its subsequent history and decay, that is, to show to what extent this form lost its original signification of completed action and to what extent the form itself decayed; and *fourthly*, to show by what new forms it was afterwards wholly or in part supplanted. We shall be compelled to limit ourselves to a part

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of the Indo-European field, and shall, therefore, choose as the basis of this paper those branches of our family which best illustrate our subject—the Sanskrit, the Greek, and the Latin.

*First*, then, let us enquire into the *origin* of verb-reduplication. In all languages we may find abundant illustrations of the principle that the repetition of a root, stem, or word adds emphasis to the expression—a principle which extends to all parts of speech. In Sanskrit, for example, the root *lū* (Gr. λύ-ω) means 'to sever,' and *lolū* for *lulū*, 'to sever completely.' Even in adverbs and prepositions we find examples of it: e. g. *upari* (Gr. ὑπέρ) means 'up' or 'above,' and *uparyupari* (for *upari upari*) means 'higher and higher,' or 'wholly up,' or 'above'; while the word *adhas* on the contrary means 'down,' and *adhas adhas*, or by euphonic combination *adhōdhas*, means 'wholly down.' We discover an illustration of the same principle in the Latin personal pronouns, *meme*, *tete*, *sese*; and in the general relative *quisquis*, corresponding to the Sankrit *yas yas*, which, however, is usually accompanied by the correlative *sas sas* (as in Nalus v. 12, *yaṃ yaṃ hi dadṛiśe teshān tan tam meme Nalan nṛipam*, 'for whomsoever of these she looked upon, this one she thought to be prince Nalus'); also in the relative adjectives *qualisqualis*, *quantusquantus*, *quotquot*. We give these examples because the whole word is in each case repeated, and because the principle involved is evident. Hundreds of words might be easily adduced at the basis of which lies the same principle, words which, however, have undergone euphonic changes, so that they are not readily recognized as reduplications, and of which the original intensive force has been lost. In all such cases the original purpose evidently was to give increased emphasis to the expression.

Very early the primitive Aryan people began to employ this method of strengthening their verbs. In the mother tongue the reduplication consisted of nothing less than a repetition of the whole root; but in course of time the form began to decay, and in the different branches of the family we find only representative elements of the root repeated. The original reduplication, e. g., of *vid* 'to see' was *vid vid*, and to

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this was added the root of the personal pronoun *ma*, restricting the action of the verb to the first person. From this radical stage it passed into the agglutinative and became united as *vidvidma*. *Now* the first syllable may be called the reduplication, the second the root, and the pronoun the termination. The language afterwards passed into the inflectional stage, in which further changes and modifications were made, modifications which tended to distinguish the primary root, as the bearer of the meaning of the word, from the reduplication and the termination. Thus *vidvidma* became, by strengthening the root and by curtailing the reduplication and termination, *vi-vaid-a*. Thus the reduplication dwindled down to those representative syllables which we find in Sanskrit, Greek, and Latin.

This reduplicated form did not at the outset assume the functions of a perfect, i. e., a tense expressing completed action. It was simply a present, existing side by side with the simple present, but expressing, however, intensive action. This is the origin and explanation of those reduplicated presents which we find in Sanskrit, Greek, and Latin: e. g., Skr. *dadāmi*, Gr. δίδωμι; Skr. *dadhāmi*, Gr. τίθημι; Skr. *jajanmi*, Gr. γίγνομαι, Lat. *gigno*. All such were without doubt originally intensive verbs. Even in later times, when the Sanskrit formed new intensives, it always did so by reduplication, as if still recognizing this primitive method. The Greek also has not a few intensives of later origin formed in the same manner, as e. g. from the root φαν 'to shine' we get the Epic φανφαν or παμφαν, 'to shine brightly,' (παμφαίνησι, Il. v. 6); from πνυ 'to breathe,' ποιπνύω 'to puff' or 'breathe with exertion' (Il. 8, 219). We can readily see how these old intensive presents in the mother tongue would gradually assume the idea of completed action. Bopp says, and his remark is quoted and approved by Curtius in his Moods and Tenses: "Die Reduplicationssylbe bezweckt bloss eine Steigerung des Begriffs, gibt der Wurzel einen Nachdruck, der von dem Sprachgeist als Typus des Gewordenen, Vollendeten, im Gegensatze zu dem erst im Werden Begriffenen, noch nicht zum Ziele Gelangten, aufgefasst wird" (Vergleichende Gram-

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matik, s. 749). Schleicher in his Compendium says substantially the same thing: "Die function der verdoppelung ist stäts im algemeinen die der steigerung, welche aber ser verschidene, später gesonderte beziehungen umfasst, so z. b. die intensive und iterative beziehung, welche sich später in den verbis intensivis entwickelte; dise beziehung ist den reduplicierten aoristen noch deutlich warnembar. Die beziehung der dauer bezeichnet die wurzelverdoppelung in den reduplicierten praesensstämen; die der vollenteden handlung im perfectum" (Compend. der Ind. Ger. Spr. s. 716). Although they recognize this change of signification from intensive to completed action, yet they do not attempt to explain how this change may have occurred. Although Bopp's statement that "reduplication gives emphasis to the root which the spirit of the language regarded as a type of that which is done," enables us to conceive how this change may have come about, and though the mere mention of the authorities already cited may be sufficient on this point, yet we may be permitted to offer one or two suggestions in further elucidation of it.

In the first place, reduplication may imply completion from the fact that the repetition of an act implies that it has been already once done. We might illustrate this from the repetition of ἄλλος in such frequent use in Greek: e. g. ἄλλος ἄλλο λέγει, literally, 'another says another thing,' which implies that some one has already said something, and hence is properly rendered 'one says one thing, another says another thing.' This case is somewhat different, to be sure, but may it not serve to illustrate how a repetition may imply that the act has already been done, and thus connect with the repetition of a verbal-root the idea of a completed act? For the assertion that an act is taking place a second time is virtually an assertion that it has already taken place once.

Again, intensity of action, the original signification of this reduplication, also implies completion. No element of action is more indicative of completion than intensity. Whenever we see a man bending to a task with the utmost intensity, we say the work is as good as done. In the case of the verb, may the mind not have passed from the action itself over to

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the result, to the completed act suggested by this intensity, and thus the verbs of completed action have been derived gradually from these intensive verbs?

This then we take to be the origin of this reduplicated tense, a tense denoting present completed action, or as we call it a perfect tense. Moreover, this was, we assume, one of the earliest, if not the very earliest form made use of by the primitive Aryan people for expressing the relation of time, for no other tense is so simple in its structure nor gives evidence of a greater antiquity, except perhaps the uncompounded aorist, the so-called second aorist in Greek, the aorist in *am* in Sanskrit. All the other tenses bear evidence of later formation, many even of having been formed on separate Indian, Hellenic, or Italian ground. This form, however, must have become fully established as a tense for denoting completed action before the original separation of the family, since we find it bearing this signification in the earliest literary records of each of the branches, Old Indian, Old Bactrian, Grecian, Latin, German, and in the Celtic (remnants). The Letto-sclavic alone has preserved no trace of it. This form, moreover, must have already passed through the radical and agglutinative into the inflectional stage before the separation; for in the oldest records of each of the branches we find it already reduced to the same or nearly the same form that it presents in the classical periods.

Let us now briefly trace its subsequent history in the Sanskrit, the Greek, and the Latin.

I. In *Sanskrit.* The Sanskrit inherited from the mother tribe the above method of forming the perfect tense, a tense which had already become distinguished in both form and meaning from the class of intensives from which it took its rise. In regard to it two assumptions may with apparent safety be made. *First*, in the earliest period of the history of the Sanskrit-speaking people this reduplicated form was the only one used for expressing completed action. Other forms sprang up later, as we shall see, but they all bear evidence of having been formed on Indian ground. *Secondly*, at this early period also this form had probably the signification of completed

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action only, and did not until later assume those other functions which it performed in its subsequent history. For, had it already taken on its later offices while the language was one, we should expect to find evidence of such use in the early Greek also. Very early, however, it began to lose gradually its own proper signification, and with decay of form dwindled down in meaning to a mere past tense in narration. This had gone on to such an extent that, although the reduplicated form was preserved in constant use in the latest literary period, yet its prevailing signification in the classical era was not that of a perfect but of an aorist. The three preterit tenses—the imperfect, the aorist, and the perfect—seem to have been handled very capriciously, and in their use no apparent distinctions seem to have been observed.

We next have to ask what new forms arose to assume the functions cast off by the reduplicated perfect? For never since the Aryan people first formed the conception of completed action have they once surrendered it, but they have always had some form, either synthetic or analytic, by which to express it. The Hindus supplied its place by several new formations, and to these we wish now to direct your attention. For sake of clearness they may be spoken of under six different heads.

1. Very early there arose what Sanskrit grammarians call the Periphrastic Perfect, formed by making of the verb root an abstract noun in *ām*, and affixing to this the reduplicated perfect of one of the auxiliary verbs, *as* 'to be,' *bhu* 'to be,' or *kṛi* 'to make' or 'to do.' Thus of *īś* 'to rule' is formed the abstract noun *īśām*, and to this is added the perfect *āsa*, *bahūva*, or *ćakāra*, giving *īśāmāsa*, *īśāmbabhūva*, or *īśāmćakāra*, the first two of which may be rendered 'I have been ruling,' and the last 'I have done ruling' or 'I did rule,' using the verb 'to do' as the Germanic branch did at a later date for the formation of a similar compound past tense. Here in each case the idea of a perfect lies in the reduplicated auxiliary verb. This was the method of forming the perfect of roots beginning with a long vowel and of those of more than one syllable, including derivatives, such as causals, desidera-

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tives and frequentatives. The purpose was evidently to avoid the ambiguity connected with the reduplication of a long initial vowel, and the difficulty of reduplicating polysyllabic stems. We may trace this form back to its very origin—a thing we are rarely able to do in the history of a verbal form. It occurs for the first time in the Atharvan, confessedly by far the most modern of the four Vedas, and here it occurs but once (*gamayām ćakāra*, A. V. 18, 2, 27). Wherever those verbs, which in later Sanskrit require this form, are found in the earlier Vedas, they always have the simple reduplicated perfects.

2. Usually when the completion of an action is to be expressed we find an analytic form, a perfect participle in *tas* (Greek τος, Latin *tus*) used with the present of the verb *as* 'to be' (Greek ἐσ-τί), and the agent expressed by the instrumental case. Thus to say 'Thou hast seen Nalus' we should have *Nalas tvayā dṛishṭas asti*, or by euphonic combination, *Nalas tvayā dṛishṭo 'sti*, 'Nalus by thee having been seen is,' equivalent to the Latin *Nalus a te visus est*. The auxiliary, however, is seldom expressed, and we find the above in Nalus ix. 29, *Nalas tvayā dṛishṭas*. This form, as well as that in *tus* in Latin, probably acquired the signification of a perfect somewhat in the following way. The demonstrative pronominal ending *tas* added to the root of a verb expresses the result of its action and implies that the action is already finished. Thus *dictum* in Latin expresses the result of the root *dic*, and *dictum est* means 'there exists the result of the root *dic*,' or in other words 'the saying has already occurred,' or 'it has been said.' Again *amatus est* means 'he is a loved object,' a result of the action of the root *am*, implying that the action has already been exerted, i. e. some one has loved him and he is now an object loved, or 'he has been loved.' And how often we meet these perfects passive when we are in doubt whether to translate them as presents or perfects, 'he is loved' or 'he has been loved.' They both amount to the same thing. In this manner do we explain the origin of the perfect passive in Sanskrit. This analytic perfect is in constant use in classical Sanskrit in every species of composition.

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This perfect passive participle occurs, in fact, nearly as many times as all other verbal forms put together, used not only where it supplies the place of the indicative perfect, but as often where the context requires the auxiliary of other moods. The precise tense and mood is often left to be inferred from the context, making the whole structure of the sentence loose and indefinite, and often, to the Greek and Latin scholar, provokingly so. Indeed it is calculated to astonish one, after having spent many weeks, perhaps months, in endeavoring to master the numerous classifications and moods and tenses of the Sanskrit verb, to find how few are the forms he is destined to meet in his reading. The whole treatment of the verb, the very soul of a language, is bald indeed when placed beside that of the Greek and Latin, and in fact the whole language, as a mode of expressing thought, will not suffer a moment's comparison with either of these languages; and it is surprising that western scholars, misapprehending the true sources of its value to philology, have, in imitation of the extravagant expressions of that eminent Orientalist, Sir William Jones, asserted again and again its superiority in this respect to the classic tongues of Greece and Rome.

3. The perfect is rendered frequently by this same participle in *tas* in agreement with the subject of the verb—a construction which is not unknown to the Greek and Latin; but in the excessive use of compounds in Sanskrit, in which nouns, adjectives, prepositions, conjunctions, and participles are all dovetailed together, this use of the participle is so frequent, where in Greek and Latin we should find a perfect, that it is deemed worthy of a separate mention.

4. With neuter verbs this same participle is used in impersonal constructions. Thus to say 'Thou hast gone' we have *gatan tvayā asti*, 'it has been gone by thee,' or usually without the auxiliary, as in the Episode Savitrī, v. 19, *gatan tvayā*. This use is similar to such Latin expressions as *ventum est.*

5. With neuter verbs the passive use of this participle is limited to the foregoing impersonal construction; but, unlike the Latin, these same participles in *tas*, of these same neuter



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verbs, are often employed personally with an active meaning. Thus this same expression, 'Thou hast gone,' may be rendered personally, *gatas asi* or by euphony *gato 'si*, equivalent and similar to the German *du bist gegangen*, as in Nalus xii. 13, we read: *kva nu rājan gato 'si? quone, rex! profectus es?* 'whither, O king, hast thou gone?' The last four forms are, it will be observed, of like origin, and are perfects by virtue of the participles, as already explained.

6. One other form remains to be noticed, and it is a case worthy of careful analysis. It is formed by a perfect active participle and the present of the auxiliary verb *as* 'to be.' This participle of the verb *kṛi* 'to do,' is *kṛitavān*, and *kṛitavān asmi* means 'I have done,' as in Urvasī we find *api dṛishṭavān asi mama priyām*, 'art thou having seen my beloved?' or 'hast thou seen my beloved?' Let us now analyze this form and see by virtue of what elements it has acquired this signification. It is composed of three elements, *kṛi* the verb root, *tas* the ending added to form the perfect passive participle, and *vān*. This latter comes from the suffix *vant*, nominative singular masculine *vants*, denoting 'possession,' which by the rules of euphony becomes *vān*, a change not unlike that by which in Greek the participle λυοντς by omission and vicarious protraction becomes λύων. This is often added to nouns to form adjectives of possession, e. g. *dhana* 'riches,' *dhanavān* 'one possessing riches,' 'a rich man.' Adding to this the stem of the perfect passive participle we get *kṛitavān*, which originally signified precisely what the individual elements of which it is composed mean, namely, the present possession of the object in the condition specified by the participle. But this participle in *tas*, as we have already shown, implies a past action viewed as completed, and the statement of the possession of an object in the condition of completion denoted by the participle came in time to be accepted as expressing the completed act by which it was brought into that condition. The basis upon which a perfect tense has been reared is not the element of possession, as some would seem to imply (see Schleicher's Compendium, § 218, ed. 1870), but rather, just as in the second class discussed, the idea residing in the par-

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ticiple. The same is true of those modern analytic perfect formations made up of the perfect passive participle and an auxiliary verb denoting possession; and *kṛitavān asmi* might be thus rendered *factum habeo*, *je l'ai fait*, *ich habe es gethan*, and *I have done it*. There is no mysterious virtue in this auxiliary *have* by the influence of which this phrase becomes a perfect. There has been in each case a transfer of the centre of gravity from the declaration of the condition of completion inherent in the participle to the declaration of the antecedent act implied in that condition. When once this formation had become established as an expression of completed action, and the step by which it attained this position had been forgotten, then the auxiliary laid aside its original functions as a separate part of speech, and, becoming a mere formative element, assumed the burden of representing the perfect tense, and by its aid there were formed from analogy other classes of perfects where etymological analysis would find only nonsense, as *uktavān asmi* 'I have said,' where there is no idea of possession, and as *bhūtavān asmi* 'I have been,' where not action, but simply state or condition is denoted.

Thus we have seven (or really four distinct) formations for expressing completed action in Sanskrit. We would not be understood to assert that all these changes took place chronologically as we have treated of them. Whether the reduplicated perfect first began to decay, to lose its signification, and these analytic forms in consequence sprang up to supply its place, or whether these analytic forms began to rise and usurp the functions of the reduplicated form, and this in consequence began to yield the field—which was the cause and which the effect, which the antecedent and which the consequent, we do not pretend to say; it is more probable that the two processes went slowly on side by side and are not to be sundered. We simply state the facts as found recorded in the literary records left us by the Sanskrit speaking people, classifying as we have simply for convenience and a more perspicuous presentation of the subject.

II. In *Greek*. The whole history of this form in Greek

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may be told in a very few words. The Greek inherited from the mother-tongue the method of forming a tense for completed action by reduplication and preserved it intact to a greater extent than any other branch of the family. Just two remarks are all that is called for under this head.

1. The Greek perfect has shown remarkable tenacity in holding to the idea of completed action. In this respect it stands unique in the history of the Indo-European verb. It has kept strictly within its own province, and has not, like the Sanskrit, dwindled down to a mere historic past, entering thus the domain of the aorist and assuming its functions.

2. Not only has the Greek reduplicated perfect kept its own province, but, on the other hand, it has allowed no other forms to enter and rob it of any of its functions, as the Sanskrit, which has been, as we saw, robbed of all its original possessions. Throughout all the periods of its history, from the earliest Epic to the latest Attic forms, it knows no other means of expressing completed action. Even the circumlocutory forms used in the moods of the middle and passive voice, and sometimes for euphonic reasons elsewhere, are, unlike the similar analytic perfects in Sanskrit and Latin, formed with a reduplicated participle in which lies the idea of completed action. Whatever then may be the explanations of the new forms in κ, and those with σ in the Middle Voice, we yet feel confident in stating that no element ever entered into the Greek verb to denote completed action except the reduplication originating as already explained.

III. In *Latin.* The Latin also, after its separation from the mother tongue, continued to form its perfects in the primitive way by reduplication. Unlike the Sanskrit and the Greek, however, where the form remained intact, here in Italic speech the form itself began early to decay, so that in the literary language we find only remnants of it. The verbs in which these remnants are found may be divided into four classes.

1. Some less than thirty verbs still retain the old reduplication, as *peperi*, *tutudi.*

2. About an equal number have a short stem vowel

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lengthened in the perfect, the result of a contracted reduplication: as *lĕgo*, *lĕlĕgi*, *lĕlĭgi*, *lĕĭgi*, *lēgi*; *vĕnio*, *vĕvĕni*, *vĕvĭni*, *vĕĭni*, *vēni*.

3. Somewhat less than fifty have the vowel unchanged in the perfect, being already long: as *cūdo*, *cūdi*; *īco*, *īci*.

4. The compounds of the above classes, which suffer some euphonic changes, complete the list of perfects in *i*. All of these are perfects by virtue of an original reduplication.

Let us notice now the new formations which sprang up to assume the functions of the perfect when this method had fallen out of use. We may speak of three classes.

1. Perfects in *si*, as *scrip-si*, *rexi* (*reg-si*), etc., about one hundred in number. Bopp was the first to explain these as compounds of *esi*, a perfect of the root *es*, 'to be.' This view has been quite generally accepted by scholars, but none have attempted to show by virtue of what this *esi*, and in consequence the forms compounded of it, became perfects. In his later writings Bopp himself struck these forms out of his list of perfects, but tried in vain, as it seems to us, to find some intimate relation between them and the Sanskrit aorist in *sam*, *sīs*, *sīt*. No attempt had been made to give a complete and satisfactory explanation of this form until last year, when, before this Association, Professor Harkness, in his critical paper "On the Formation of the Tenses for Completed Action in the Latin Finite Verb," presented an extended analysis of it based upon the theory that *esi* is itself a reduplicated perfect. No other theory that has been yet put forth can account for all the elements entering into this difficult and almost inexplicable form. Accepting this view, we explain the perfect in *si* as a perfect by virtue of the auxiliary, which is itself a reduplicated perfect of the root *es* 'to be'; and thus, as far as the element of tense is concerned, this form is not unlike the periphrastic perfect in *āsa* in Sanskrit.

2. Perfects in *vi* and *ui*, as *amavi* and *monui*, are regarded by all scholars whose authority we are accustomed to follow, as compounds of the auxiliary verb *fui*, and need only this mention from us. These again are perfects because the auxiliary is a perfect belonging to the *i* class and formerly

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reduplicated as already explained. These also, you observe, as far as the tense element is concerned, are not unlike the Sanskrit periphrastic perfects in *babhūva*, root *bhu*, Greek φυ, Latin *fu*.

3. The foregoing complete the list of synthetic perfects in Latin; but later there arose certain analytic forms which may be mentioned in this connection. (1). In the passive voice the perfect is expressed by the perfect passive participle in *tus* and the present of the verb *es* 'to be,' as *amatus est* or (of neuter verbs) *ventum est*. Here the perfect signification was acquired in the same manner as in the corresponding forms in *tas* in Sanskrit as already explained. In this expression a transfer of thought has taken place similar to that which the analytic forms denoting possession underwent. In the one case we have the predication of the present possession of an object in a certain condition, in the other the predication of the present existence of a subject in a similar condition, said condition in both cases implying the previous action. In neither case has the auxiliary—'to have' or 'to be'—had any influence whatsoever in raising the form to the rank of a perfect. As far as the auxiliaries are concerned, both forms would have remained a *present*. The basis upon which the idea of completed action has been reared is the condition denoted by the participle. Starting from the same point, both expressions by a like process have reached the same goal. This process has been more fully illustrated under the Sanskrit forms in *vān*. As to the question whether the Latin ever possessed a synthetic perfect passive, which was afterwards supplanted by this analytic form, it cannot be established by any proof drawn from Roman literature. As, however, the Sanskrit and the Greek possess such forms, we should infer that they existed in the mother-tongue before the separation, and that there was probably a time in the history of Italic speech when such a form was in use; but no trace of it has been left.

(2). There also arose, in both the active and the passive voice, a so-called periphrastic perfect, formed by the perfect *fui*, and in the active the future active participle, as *amaturus fui*, in the passive the gerundive participle, as *amandus fui*.

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These participles are used with the verb *esse* throughout all its moods and tenses. The perfects are perfects by virtue of the auxiliary.

(3). Still one other form appears, the prototype of the modern analytic form with *avoir* in French, made up of the perfect passive participle in agreement with the object and the present of the verb *habēre*. Thus in Cic. Div. 2, 70, 145, we read: *innumerabilia, quae collecta habent*, 'which they have collected.' This form was used, though sparingly, in all the periods of Latin literature and its rise may be explained precisely as that of those in *vān* in Sanskrit, to which it is similar.

In regard to the chronological development of these different perfect formations in Latin, we cannot, of course, determine with any degree of accuracy, as we have not here so extended a literary history as in the Sanskrit, throughout which we may trace the rise of new forms. In all periods of the literature we find all these forms used side by side, and without any apparent increase or decrease in the use of any one of them. The reduplicated forms are, without doubt, the most primitive; those in *si* seem to contain evidences of antiquity which entitle them to the second place, though *secundus longo intervallo;* those in *vi* and *ui* probably arose next, and the analytic forms would naturally be developed last.

In connection with the Latin perfect, we should call attention to the fact that all these forms have, besides the signification of the perfect, also that of an aorist. Whether the Latin originally possessed an aorist form like its two sister branches and afterwards lost it, and the perfect gradually assumed its functions, cannot be determined from the material at our command. From its earliest to its latest literary records, the Latin uses this tense with this two-fold signification. There is no doubt but that an aorist form had already become established in the mother-tongue before the breaking up of the family, and that the different branches at the separation carried away uses of it; but all, except the Sanskrit and the Greek, very early lost it.

*Resumé.* Of these three languages which received by inheritance the reduplicated perfect, we see then that: I. The

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Sanskrit, while preserving the form in use, has lost entirely its original meaning, and has developed three new and distinct forms: 1. The periphrastic perfects with *āsa*, *babhūva*, and *ćakāra*. 2. The analytic perfects with the perfect passive participle in *tas*. 3. The analytic perfects with the perfect active participle in *vān*.

II. The Latin has lost the form almost entirely, and has retained the signification only in part, while five new forms have appeared.: 1. Perfects in *si*. 2. Perfects in *vi* and *ui*. 3. The analytic perfects with the perfect passive participle in *tus*. 4. The analytic periphrastic perfects with the future participles and a perfect of the auxiliary. 5. The analytic perfects with the perfect passive participle and the auxiliary *habēre*.

III. The Greek discovers a most remarkable history, showing its superiority here as in all other parts of its verb. Throughout a long literary career it has preserved both its form and its signification entire.

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## VI.—*A Grammatical Analysis of the Old English Poem, "The Owl and the Nightingale."*

By L. A. SHERMAN,

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It is generally agreed that the author of this poem has left us his name in the character of the righteous umpire, Nichole of Guildeford; but when he lived it is impossible to determine within a century. The name of King Henry is once mentioned (line 1091), showing, in connection with other facts, that the poem cannot well have been written before the reign of the second sovereign of that title. For fixing the other limit to the possible period of its composition, but little is offered, either of suggestion or of proof. The number of words borrowed from the French does not exceed thirty. The author is no less a Saxon in sympathies than by birth, for he quotes only Alfred, and that no less than eleven times. But, on the other hand, he calls himself *Maister*, and is apparently a priest. The prejudice against the Saxons must, therefore, by his time, have largely disappeared.

The following analysis has been made from the edition of Stratmann, which shows the readings of the only two MSS. of the poem which have yet been found. The material has been taken from his emended text, except in instances where he has deviated from the reading of both MSS. to insert a form theoretically more original or correct. The peculiar forms admitted in such cases will be distinguished, those from the older Cotton MS. by C., the Oxford by A. Forms not in parenthesis are the reading of both.

### NOUNS.

The Strong and Weak Declensions of the Anglo-Saxon are well preserved. The former differs but slightly from its primitive. The latter has shortened *-an* to *-e*.



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STRONG DECLENSION.

*Masculines.*

| SING. | PLURAL. |
|---|---|
| N. dom | N. briddes |
| G. domes | G. ——— |
| D. dome | D. briddes |
| Ac. dom | Ac. briddes |

An ephelkystic *-e* is found in the nominative singular *swikelhede*, and the accusative singular forms *beore*, *godhede*, *hihte*, *hunde*, and *woȝe*.

The dative sing. occurs a few times the same in form as the acc., having lost the final *-e*. The nominative plural without *-s* is not found with words known to be masculine, but is seen in two instances of nouns doubtfully so: *Theȝ appel trendli from þon treowe.—Thar two ileove . . liggeþ.*

The genitive plural ends in *-e* or *-ene*: *þan elles hwar beon deovelene fere.*

A few datives or accusatives plural occur with the ending *e*: *And oþer clene stede þu schunest.—Hwane mon hoȝeþ of his scheve.—Ac na þe les mid alle his wrenche.—Ich habbe at wude treon wel grete mid þicke boȝe.*

In some instances further the number is doubtful, the nouns being possibly used generically or collectively: *The faucun ilefde his bridde.—Al þat þu miht mid clivre smiten.* (Both these nouns show the regular plural in *-s*.)—*And þe totorveþ and tobuneþ mid stave and stone.—Thu canst feler wike.—Hwar þu miht over smale fuȝele.*

In one instance a form occurs which is apparently a strong dative plural in *-en*: *That is bischopen muchel schome* (l. 1761).

Nouns of anomalous declension in Anglo-Saxon have the following forms:

| SING. | | | PLURAL. | |
|---|---|---|---|---|
| N. broþer | G. freondes | D. foto | D. teþ | Ac. freond |

*Man* is thus inflected:

| SING. | PLURAL. |
|---|---|
| N. man | N. men |
| G. mannes | G. manne |
| D. manne, men | D. manne, men |
| Ac. man, manne | Ac. men |

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### *Feminines.*

The differences of case-formation will be best understood from the following comparative exhibit of words occurring in two or more cases.

| SINGULAR. | | | | PLURAL. | | | |
|---|---|---|---|---|---|---|---|
| N. | G. | D. | Ac. | N. | G. | D. | Ac. |
| amansing | | amansinge | | | | | |
| bliss, blisse | | blisse | blisse | | | | |
| cheste | | cheste | cheste | | | | |
| | | dede | dede | | | dede | |
| | | lavedi | | lavedies | | | |
| | milne | milne | | | | | |
| niht | | nihte | niht | nihtes | | | nihtes |
| stefne | | stefne | stefne | | | | |
| sunne | | sunne | | | | sunnen | sunne |
| ule | ule | ule | ule | | | | |
| wiht | | | | wihtes, wihte | | | |
| | | | | | wise | | |
| | worldes, worlde | | | | | | |

An inorganic *-e* is seen in the nominatives *blisse*, *cheste*, and *stefne*.

Of feminines of the A. S. anomalous declension there are the following forms:

| SINGULAR. | | | PLURAL. | | |
|---|---|---|---|---|---|
| G. | D. | Ac. | N. | D. | Ac. |
| bokes | boke | bok | mus | muse | mus |
| | | burȝ | | | |
| | | turf | | | |

### *Neuters.*

Those of most frequent occurrence are the following:

| SINGULAR. | | | | PLURAL. | | | |
|---|---|---|---|---|---|---|---|
| N. | G. | D. | Ac. | N. | G. | D. | Ac. |
| child | | | | children | | childre | |
| cun | | cunne | | | cunne | | |
| flesch | flesches | flesche | flesch | | | | |
| hors | | horse | | | | horse | |
| hus | huses | huse | huse | huses | | | |
| maide | | maide | maide | maide | | | |
| nest | | nestè | neste | | | | |
| | | rise | | ris | | rise | |
| þing | | þinge | þing | þinge | | þinge | þing |
| wif | wives | wive | wif | wives | | | wive |
| word | | worde, word | word | wordes | | worde | wordes |
| wundor | | | | | wundre | | |

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An inorganic *-e* occurs once in the nominative: *Ne mai his strengþe hit ischilde, þat hit nabuȝþ þe lutle childe* (here added perhaps for rhyme only). In the accusative sing. it is seen in *huse, bile, imunde. Thinge* as a nom. plural occurs once only: *The mon mot . . . wite inoh of hwiche þinge cume.* There is one other similar form: *Wepne beoþ gode griþ to halde.*

From a comparative view of these paradigms we see the distinction of gender as expressed by form well nigh destroyed; neuters, and feminines as well, taking the masculine *-es* in the plural, and the feminines also in the genitive singular. In the dative singular the formative *-e* may be omitted, to the accusative it may be added, showing that the difference between the dative and accusative idea was about obliterated. In the mas. plural the same unification of these cases is noticed, but is strangely wanting in the neuters, no dative plural neuter in *-es* appearing in the poem.

WEAK DECLENSION.

All cases in both numbers show the ending *-e*, and for all genders. The following words occur, all of the A. S. weak declension.

| SINGULAR. | | | | PLURAL. | | | |
|---|---|---|---|---|---|---|---|
| N. | G. | D. | Ac. | N. | G. | D. | Ac. |
| ape | | | | | | | |
| | | | | | | attercope | |
| | | | | blostme | | | |
| | | burne | | | | | |
| | chirche | chirche | | | | | |
| | | | | crowe | | | |
| | | deme | | | | | |
| | | dwole | dweole | | | | |
| | | | | fere | | | |
| | | | fihte | | | | |
| | | | fode | | | | fiȝe |
| | | folde | | | | | |
| | | frogge | | | | | |
| | | gare | | | | | |
| | | grome | grame | | | | |
| hare | | | | | | | |
| harp | | harpe | harpe | | | | |
| heorte | heorte | heorte | heorte | | | | |
| | | | | hine | | | |
| | | | hoȝe | | | | |
| | | | imene | | | | |
| iwune | | | | | | | |
| lilie | | | | | | | |
| | | lichome | | | | | |

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| SINGULAR. | | | | PLURAL. | | | |
|---|---|---|---|---|---|---|---|
| N. | G. | D. | Ac. | N. | G. | D. | Ac |
| | | linde | | | | | |
| | | loke | | | | | |
| | | masse | masse | | | | |
| necke | | | | | | | |
| | | nome | | | | | |
| | | | | | | netle | |
| oxe | | | | | | | |
| | | onde | | | | | |
| rose | | | | | | | |
| | | | | schine | | | |
| | | side | | | | | |
| sop-sage | | | | | | | |
| | | | spure | | | | |
| | | | dai-steorre | | | steorre | steorre |
| | | sticke | | | | | |
| | | sweore | | | | | |
| | | | teone | | | | |
| bed-time | | time | | | | | |
| tunge | | tunge | | | | | |
| | | þrote | þrote | | | | |
| | | weole | | | | | |
| | | wille | | | | | |
| | | wlate | | | | | |
| wrecche | | | | | | | |
| wrenne | | | | | | | |

The neuter *eȝ*, 'eye,' has the following forms: acc. sing. *eȝ*, nom pl. *eȝen*, *eȝe*, dat. pl. *eȝen*, *eȝe*, acc. pl. *eȝen*, *eȝe*: *Theos ule heold hire eȝen neoþerward.—That ut berste bo þin eȝe.*

The neuter *treow*, 'tree,' has: nom. pl. *treon*, dat. pl. *treo* and *treowe*, acc. pl. *treon*. This word is always strong in A. S., and perhaps received its weak endings from identity of form with *treow*, 'faith,' which is weak.

The paucity of genitive forms in both declensions is partly due to the use of the dat. with *of* in its place: *Thu bodest ferde of manne oþer þeoves rune.—Hwi niltu singen to men of Galeweie.*

## THE ADJECTIVE.

The adjective receives the endings of both the strong and the weak declension, or their representatives. The strong retains its distinctive features only in the nominative and genitive singular, the other cases being for both declensions marked in common by *-e*, or in the strong stand without endings. The weak shows *-e* in all cases.

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STRONG DECLENSION.

*Singular.*

*Nominative.*—The adjective closely follows the Anglo-Saxon nominative form, and in only two or three cases shows *-e* where it would not be found in Saxon: *For ȝif aht man is hire ibedde.—Uvel strengþe is lutel wurþ.* With added *-e*: *In one bure þat hire was boþe stronge and sure.—Hwanne snow liþ wide.*

*Genitive.*—The regular A. S. ending *-es* is found only in the pronominal *oþres*, which occurs four times: *To oþres mannes bedde.* Two instances occur of weak forms substituted for strong: *In so gode kinges londe.—Of sume freondes rure.*

*Dative.*—The dative takes *-e*: *It was iseid in olde laȝe.—Bi þeostre nihte.—Of selliche wisdome.* The pronominals *al* and *oþer* are frequently indeclinable, except *oþres* in the gen. sing., as above.

*Accusative.*—The masculine adjectives show the bare stem, like the nominative, but nearly as frequently take *-e* or the regular *-ne*: *He schuneþ þat hine ful wot.—Al þat werieþ linenne cloþ.—Ich wot hwo schal fulne deþ afonge.* The only adjectives which take *-ne* are *ful*, *god*, *riht*, and *sum*.

The feminine adjectives take *-e*, but may omit it: *Ich habbe gode answare.—For me hi halt loþlich and fule.* Of cases like the latter there are not more than three or four.

Neuters, bare stem, two or three times *-e*: *Ich habbe bile stif and stronge.*

*Plural.*

The genitive plural with full ending *-re* is found in two* adjectives only: *Hit is alre wundre mest.* This form occurs eight times. *The laverd . . . fareþ ut on þare beire neode.* Simple *-e* is also found. *Hit is a wise monne dom.—Hit is gode monne iwune.*

The nom., dat., and acc. pl. end alike in *-e* without exception, except *sum*, and *oþer*, and *al*, as above. *Theȝ eni god man to heom come, so hwile dude sum from Rome.* In a single

* Add the numeral *two*, gen. pl. *tweire*. Cf. *numerals* at end of pronoun.

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apparent exception, *Gode clivres scharp and longe*, the final *-e* of *scharpe*, was probably not written because it was not pronounced before the following initial vowel. *Eni* and one are followed by strong forms: *Eni god man.*

### WEAK DECLENSION.

The adjective takes *-e* for all cases in both numbers.

*Nominative.—That ilke best.—Þeos riche men.—That gode wif.—Alle þine wordes.*

*Dative.—Hire wise tunge.—Thare longe tale.*

*Accusative.—Let þane lutle fuȝle nime.—Ne mai þat þridde no man bringe.*

### COMPARISON.

The scheme is scanty.

| POSITIVE. | COMP. | SUPERL. |
|---|---|---|
| blipe | blipure, (blipur, C.) | |
| god | betere, (beter, A.) bet, 21-23. | |
| briȝt | briȝter | |
| fair | fairer | |
| glad | gladdre | |
| gret | grettere | hecst |
| icunde | icunder | |
| lutel | lesse | |
| milde | mildre | |
| muchel | more, mo | mest |
| neh | | necst |
| rad | raddere | |
| soft | | softest |
| strong | strengur | |
| uvel | worse | worst |
| | *Adverbs.* | |
| wel | bet | best |
| er | erur | erest |
| faste | fastre | |
| heȝe | herre | |
| ilome | | ilomest |
| late | later | |
| longe | lenger, leng | |
| | les | |
| muchel | more, mo | mest |
| rape | rapere, (rapre A.) | |
| uvele | worse | |

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The comparative forms are sometimes indeclinable, sometimes take *-e* for all cases: *Of brihter heowe, of fairer bleo.—For I am wisure þan he.—Grettere is þin heved.* Of the superlatives *worst* is a weak form: *And eiþer seide . . . þat alre worste þat hi wuste.*

PRONOUNS.

*Personal.*

SINGULAR.

| | |
|---|---|
| N. Ich, ic, i (ihc, ih, C.) | N. þu |
| G. min, mi, (my seolve, A.) | G. þin, þi |
| D. me | D. þe |
| Ac. me | Ac. þe |

DUAL.

N. ———
G. unker
D. and Ac. ———

PLURAL.

| | |
|---|---|
| N. we (we A.) | N. ȝe (ye, A.) |
| G. ure | G. ower (oure, eure, eur, A.) |
| D. us | D. ow, eu (ou, C.) |
| Ac. us | Ac. ow |

SINGULAR.

| *Masculine.* | *Feminine.* | *Neuter.* |
|---|---|---|
| N. he | N. heo, ho, he | N. hit |
| G. his | G. hire | G. his |
| D. him (heom, A ; hom, C.) | D. hire | D. him |
| Ac. hine, him, heom | Ac. hi, heo, hire | Ac. hit |

PLURAL.

N. hi, heo
G. heore, hire (here, hore, C.)
D. heom, him (hom, C.)
Ac. hi, heom, heo (hom, C.)

The form *ho*, nom. fem., occurs once in A. (936), but is frequent in C., where *eo* is generally written *o*. *He* for *heo*, fem. nom. sing., is twice found alike in both A. and C. (1381 and 1560), once in A. when not in C. (19), and six times in C. when not in A. (141, 393, 401, 469, 936, 1638).

The genitives are used as possessives, once or twice particularly: *Hwi neltu . . . schewi hweþer unker beo.—And mai ure eiþer hwat he wile, mid rihte segge.* They are found indeclinable, even when connected with a plural noun, and with about the same frequency as the inflected forms. The

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latter differ from the former only by an appended *-e*, found even in the nominative sing. but rarely. *Min* and *þin*, however, show traces of the older declension: *Ich an wel, cwaþ þe nihtengale, Ac, wrenne, noht for þire tale, Ac do for mire lahfulnesse.*

THE ARTICLE.

(A. S. *se, seo, þät.*).

SINGULAR.

| *Masculine.* | *Feminine.* | *Neuter.* |
|---|---|---|
| N. þe | N. —— | N. þat |
| G. þes, þas | G. þare | G. þes, þas |
| D. þan, þen | D. þare | D. þon |
| Ac. þane | Ac. þare | Ac. þat |

PLURAL.

N. þeo (in one occurrence, þo C., heo, A. [843] )
G. þare? (140; c. f. lines 3 and 4)
D. þan
Ac. ——

*The* is only used once pronominally: *And þe oþer ne can sweng bute anne, and þe is god wiþ eche manne.* It is used generally instead of the declined forms, as an article, like the modern *the*. It occurs about forty times as an instrumental in such cases as *Heo was þe gladre for the rise*, and is used seven times as a relative: *Swo heo doþ . . . þe bute neste goþ to brode.* Elsewhere the indeclinable *þat* performs the office of the relative, and shares that of the article. The other inflected forms are used as pronouns and as articles with about equal frequency.

The nom. pl. *þeo* is found but once alike in both A. and C. (1675).

A. S. *þes, þeos, þis.*

SINGULAR.

| *Masculine.* | *Feminine.* | *Neuter.* |
|---|---|---|
| N. þes | N. þeos (þos, C.) | N. —— |
| G. —— | G. —— | G. —— |
| D. þis | D. —— | D. þisse |
| Ac. þis | Ac. (þeos, A., þos, C.) | Ac. þis |

PLURAL.

N. þeos (þos, C.)
G. ——
D. ——
Ac. þeos, (þos, C.)



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*Hwo.*

| MASCULINE. | NEUTER. |
|---|---|
| N. hwo (wo, wa, C.) | N. hwat |
| D. hwam | D. hwan (wan, C.) |
| | Ac. hwat (what, wat, C.) |

There is no appearance of this pronoun as a relative.

The following pronominals occur: *Al; ech; eni; euch; eiþer* (*aiþer*, C., *eyþer*, A.); *ever euch*, *evrich; azen*, *ozen; oþer; hweþer; hwich*, *hwuch; ihwat; swich*, *swuch*, *þilk*. Once the combination *eni man so ever*, (1474).

These cases of the use of *seolf* are found: *The sulve sottes.—Thu sulf. The sulve pope.—Him sulve.—My seolve* (A., *mi*, C).—*Heom seolve.—The seolve.—The seolve coc.*

*An* shows the forms *a*, *an*, *on*, *one*, *no* in the nominative and accusative. They are used without reference to gender: *An ule and one nightingale.* With masculine accusatives *anne* (*nanne*,) is found a few times: *Sum blind mon þat nanne rihtne wei ne con.* In the dative *ore* is found a few times, but is used apparently without regard to the gender of the following noun: *In ore waste þicke hegge.*

Only these numerals are met with:

| | | |
|---|---|---|
| N. tweie, two | | |
| G. tweire | þridde | hundred |
| D. twam | | |

THE VERB.

*Voice.*—The passive voice is formed by joining *beon* with the past participle. In place of *beon*, *wurþan* is used a few times: *I schal do þat þi speche wurþ forwode.—Nu hit schal wurþe wel isene.*

*Mood.*—There are four moods, the Indicative, Subjunctive, Imperative, and Infinitive. For the latter the gerund may be employed, that is to say, the infinitive may or may not be introduced by *to*: *Hit is unriht and gret sothede*, To *misdon one gode menne, and his ibedde from him spanne.—Thu þohtest . . mid faire worde me biswike.*

*Tense.*—The present and preterit tenses are formed by inflection, the perfect and pluperfect by the aid of *habbe* and *hadde*, and the future by *schal* or *wille* with the infinitive.

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There are two conjugations, the Strong and the Weak. The former forms the preterit by change of the root vowel, the latter by the addition of *-de* or *-te* to the verb-stem as affixes.

The past participles end, for the strong verbs in *-en* or *-e*; for the weak in *-ed* or *-d* (*-t*).

STRONG CONJUGATION.

*Endings.*

INDICATIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Plural.* | *Sing.* | *Plural.* |
| 1. -e —— | 1. -eth | 1. —— | 1. —— |
| 2. -est, -st | 2. -eth | 2. -e | 2. —— |
| 3. -eth, -th | 3. -eth | 3. —— | 3. -en, -e |

SUBJUNCTIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| Sing. -e | Plural, -en, -e | Sing. -e | Plural, -e |

IMPERATIVE.

Sing. —, e  Plural, -eth, -e*

INFINITIVE.

-en, -e

PARTICIPLES.

Pres. -inde  Past, -en, -e

The verbal forms following are given according to the class of their primitives in the Anglo-Saxon. The classification is that of Heyne.

*Reduplicational Verbs.*

SCHEME.

| *Class.* | *Pres.* | *Pret.* | *P. Part.* |
|---|---|---|---|
| 1 | a | eo | a |
| 2 | ae, e | e | ae |
| 3 | ā | ē | ā |
| 4 | — | | |
| 5 | o | eo | o |

*Class* 1.

| *Inf.* | *Pres. Ind.* | *Pret.* | *Past Partic.* |
|---|---|---|---|
| falle | falleth | —— | ifallen |
| holde | holdest | heold | iholde |

* When nominative pronoun follows.

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*Class* 2.

| *Inf.* | *Pres. Ind.* | *Pret.* | *Past Partic.* |
|---|---|---|---|
| lete | leteth | let | — |
| rede | rede | raddest | — |

*Ich nolde don that thu me raddest.*

*Class* 3.

| | | | |
|---|---|---|---|
| — | croweth | — | — |
| — | (hattest, passive) | — | — |
| sowe | soweth | — | isowe |

*Class* 5.

| | | | |
|---|---|---|---|
| blowe | — | — | iblowe |
| wepen | wepeth | — | — |

## *Ablaut Verbs.*

SCHEME.

| *Class.* | *Pres.* | *Pret.* | *Pret. Pl.* | *P. P.* |
|---|---|---|---|---|
| 1 | i | a | u | u |
| 2 | i, e | a, ä | e | u, o |
| 3 | i | å | i | i |
| 4 | eo | ea | u | o |
| 5 | a | o | o | a |

*Class* 1.

| *Inf.* | *Pres. Ind.* | *Pret.* | *Pl.* | *P. P.* |
|---|---|---|---|---|
| — | — | — | — | ibunde |
| bringe | bringeth | (brohte) | — | (ibroht) |
| berne | forbernst | — | — | — |
| clinge | clinge (2d sub. s) | — | — | — |
| — | drinketh | — | — | — |
| findan | findest | — | — | ifunde |
| — | ginneth | — | — | — |
| — | — | ȝal | — | — |
| singen / singe | singeth | song | sungen | — |
| springe | springeth | — | — | isprunge |
| — | thringe (3d sub. s) | — | — | — |
| — | — | — | — | ibolȝe |
| — | — | — | — | idorve |
| fihte | fiht | faht | — | — |
| helpe | helpeth | — | — | — |
| urne | urneth | — | — | — |
| — | — | swal | — | — |
| weorpe | weorpeth | warp | — | — |
| wurthe | wurth | — | — | iworthe |
| — | forbredeth | — | — | forbrode |
| ligge | liggeth | — | — | — |
| | berste (pl. sub.) | — | — | — |

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*Class* 2.

| *Inf.* | *Pres. Ind.* | *Pret.* | *Pl.* | *P. P.* |
|---|---|---|---|---|
| cume | cumeth | com | —— | icume |
| nimen | nimeth | nom | —— | inume |
| —— | —— | —— | —— | ibore |
| breke | —— | —— | —— | ibroke |
| —— | bereth | —— | —— | —— |
| ete | eteth | —— | —— | —— |
| ȝive | ȝiveth | ȝaf | —— | iȝive |
| —— | cwath | —— | —— | icwede |
| ——(seon) | sith | seȝ | —— | isene |
| —— | —— | underȝat | —— | —— |
| speke | spæcth | speke, (sub.) | —— | ispeke |
| stele | —— | stal | —— | —— |
| —— | awreke (sub., pl.) | —— | —— | —— |

*Class* 3.

| | | | | |
|---|---|---|---|---|
| abide | abideth | abod | —— | —— |
| abite | —— | —— | —— | —— |
| chide | chid | (chidde) | | |
| drive | driveth | —— | —— | —— |
| grede | gredeth | (gredde) | —— | igræd |
| —— | rideth | —— | —— | —— |
| —— | schrichest | —— | —— | —— |
| slide | —— | —— | —— | —— |
| —— | sniuth | —— | —— | —— |
| slide | —— | —— | —— | —— |
| smite | —— | —— | —— | —— |
| —— | stigth | —— | —— | —— |
| —— | swiketh | —— | —— | —— |
| —— | tihth | —— | —— | —— |
| wite | witest | —— | —— | —— |
| —— | writ | wrot | —— | —— |

*Class* 4.

| | | | | |
|---|---|---|---|---|
| beode | beod | bede (2d sing.) | —— | —— |
| cheose | —— | —— | —— | —— |
| creope | creopth | —— | —— | —— |
| fleo | flihth | —— | —— | —— |
| leosen | forl-eost | forles | —— | —— |
| —— | luteth | —— | —— | —— |
| —— | bi-luketh | —— | —— | —— |
| teo | —— | —— | —— | bi-toȝe |

*Class* 5.

| | | | | |
|---|---|---|---|---|
| drahe | draheth | —— | —— | idraȝe |
| fare | fareth | for | —— | ifare |
| —— | to-schaketh | —— | —— | —— |
| —— | —— | —— | —— | islaȝe |
| spanne | —— | —— | —— | —— |
| —— | stont | stod | —— | —— |

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## *Paradigm.*

*Singen*, to sing.

INDICATIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. singe | 1. —— | 1. —— | 1. —— |
| 2. singest | 2. —— | 2. sunge | 2. —— |
| 3. singeth | 3. singeth | 3. song | 3. sungen |

SUBJUNCTIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. singe | 1. —— | 1. sunge | —— |
| 2. singe | 2. —— | 2. —— | —— |
| 3. singe | 3. singe | 3. —— | —— |

IMPERATIVE.

No forms.

INFINITIVE.

singen, singe

PARTICIPLE.

*Pres.* singinde *Pret.* ——

---

IMPERATIVE.

*leten*, to let.

*Sing.* 2. lat *Pl.* 2. lateth, lete we

### WEAK CONJUGATION.

Of these verbs there are two classes. Those belonging to the first class affix the endings *-de*, *-te*, without change of the root. The second class changes the root-vowel.

The following are the most important verbs of both classes :

*Class* 1.

| *Inf.* | *Pres. Indic.* | *Pret.* | *P. P.* |
|---|---|---|---|
| lere | lere | lerdest | —— |
| ihere | ihereth | iherde | iherd |
| —— | —— | ferde | —— |
| —— | hupth | hupte | —— |
| sette | —— | sette | —— |

*Class* 2.

| | | | |
|---|---|---|---|
| buggen | —— | a-bohte | |
| don, do | deth, 3d s., doth 3d pl. | dude | ido |
| bi-thenche | thencth | thohte | ithoht |
| thinche | thincth | thuhte | —— |

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No preterital form has been observed which adds *-ede*, i. e., shows a connecting vowel between root and preterit sign.

### *Beon.*

INDICATIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. am | 1. —— | 1. was | 1. —— |
| 2. art | 2. —— | 2. were | 2. —— |
| 3. is (beoth) | 3. beoth (both, C.) | 3. was | 3. were |

SUBJUNCTIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. —— | 1. —— | 1. were | 1. —— |
| 2. beo | 2. —— | 2. were | 2. —— |
| 3. beo | 3. beon, beo | 3. were | 3. weren, were |

IMPERATIVE.

| *Sing.* | *Pl.* |
|---|---|
| 2. beo | 2. beoth |

INFINITIVE.

beon, beo

The form *beoþ*, for the 3d sing. of the pres. indic., occurs five times: *Hwone þi lesing beoþ unwroȝe* (848. The other examples may be found in lines 296, 670, 1385, 1468).

PRETERITIVE VERBS.

| *An.* | *Ah.* |
|---|---|
| PRES. INDIC. | PRES. INDIC. |
| 1. sing. (an, C.; unne, A.) Line 1739. | 3. sing. āh (auh, A.) Line 1471. |

### *Can.*

INDICATIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. can | 1. —— | 1. —— | 1. —— |
| 2. canst | 2. —— | 2. —— | 2. —— |
| 3. can | 3. { cunne, can (1324) | 3. cuthe | 3. cuthe |

SUBJUNCTIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. cunne | —— | 1. —— | —— |
| 2. —— | —— | 2. —— | —— |
| 3. cunne | —— | 3. cuthe | —— |

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## *Dar.*

| INDICATIVE. | | SUBJUNCTIVE. | |
|---|---|---|---|
| *Present.* | | | |
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. dar | 1. —— | 1. —— | —— |
| 2. darst | 2. —— | 2. —— | —— |
| 3. dar | 3. dar | 3. durre | —— |

Preterit wanting for both modes.

## *Mai.*

INDICATIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. mai, may | 1. —— | 1. mihte | 1. —— |
| 2. miht, maist | 2. —— | 2. mihtest | 2. —— |
| 3. mai | 3. muȝe | 3. mihte | 3. mihte |

(For the 2d sing. present C. has also *miȝt*, *miȝst*, while A. shows *maist*, *mist*, *myht*).

SUBJUNCTIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. muȝe | 1. muȝe | 1. —— | 1. mihte |
| 2. —— | 2. —— | 2. —— | 2. —— |
| 3. muȝe | 3. muȝe | 3. —— | 3. —— |

## *Mot.*

INDICATIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. —— | 1. —— | 1. —— | —— |
| 2. most | 2. —— | 2. —— | —— |
| 3. mot (mod, C.) | 3. mote | 3. moste | —— |

*Mote*, 2d sing. pres. once occurs (9872): *Ever mote þu ȝolle and wepen.*

SUBJUNCTIVE.

| *Present.* | | |
|---|---|---|
| *Sing.* | *Pl.* | |
| 1. mote | 1. —— | |
| 2. mote | 2. —— | Preterit wanting. |
| 3. —— | 3. moten | |

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## *Schal.*

INDICATIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. schal | 1. —— | 1. scholde | —— |
| 2. schalt | 2. schule | 2. scholdest | —— |
| 3. schal | 3. schule, schulle, (schulleth, A. 1133) | 3. scholde (solde, C.) | —— |

SUBJUNCTIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. schulle, schille | 1. —— | 1. scholde, schulde | 1. scholde |
| 2. —— | 2. —— | 2. —— | 2. scholde |
| 3. schule | 3. schule | 3. scholde | 3. scholde, (solde, C.) |

## *Thearf.*

INDICATIVE.

*Present.*

*Sing.* 3. tharf

## *Willan.*

INDICATIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. wille, wile, nelle | 1. —— | 1. nolde | —— |
| 2. wilt, nelt, wult | 2. wulle | 2. woldest | —— |
| 3. —— | 3. willeth | 3. —— | —— |

SUBJUNCTIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. —— | 1. —— | 1. wolde | 1. —— |
| 2. wille | 2. —— | 2. —— | 2. —— |
| 3. wille | 3. wille | 3. wolde | 3. wolde |

## *Witan.*

INDICATIVE.

| *Present.* | | *Preterit.* | |
|---|---|---|---|
| *Sing.* | *Pl.* | *Sing.* | *Pl.* |
| 1. wat, wot | 1. —— | 1. wiste | 1. —— |
| 2. { wost, witest, nustest | 2. nute | 2. —— | 2. wiste |
| 3. wot | 3. nuteth, (nute, C.) | 3. wiste | 3. —— |

SUBJUNCTIVE.

| *Present.* | |
|---|---|
| *Sing.* | *Pl.* |
| 1. —— | —— |
| 2. wite | —— |
| 3. —— | —— |

Preterit wanting.



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INFINITIVE.

witen, wite.

---

A few first persons occur of the pres. indic. sing., in which the verb-stem, ending in a vowel, does not take *-e*; as *Ich warni men; Therof ich wundri; Ich beginne on heh*, and *endi laȝe*. But the *-e* may be assumed: *Thanne ich pleie* and *singe*.

In the second person sing. of the same tense and mode such combinations as *etestu*, *witestu*, are frequent.

The third person also shows contracted forms, as *stiȝþ*, *spenþ*, *wenþ*. When the verbal root ends in a dental it often stands without the ending -þ, or even without showing, by euphonic alterations, that it was ever added: *Hwan he cumeþ ham eft to his wive. . . . He chid and gred swuch he beo wod, and ne bringþ hom non oþer god.—For he nis noþer ȝeþ ue wis, þat longe abid þar him neod nis.* So *beod*, *bid*, *þuster*, *understond*, *wend*, etc. In other instances euphonic changes prove previous contraction: *The ȝeorne bit and sikeþ sore.* So *arist*, *falt*, *mist*, and *west* (*wecsþ*).

Of the present participle there is one example: *Wenest þu hi bringe so lihtliche To godes riche al singinde.*

The past participle of strong verbs, with the infinitive of both classes, has very generally weakened the final *-en* to *-e*, as will be noticed in the examples given above. The past participle of weak verbs shows once or twice only the appended *-e* of declension: *Heo hadde þe speche so feor forþ iladde.* In no case is this *-e* found added to a strong participle with the *-en* retained.

Noticeable are a few adjectives formed in modern fashion through the medium of denominative verbs, or in other words by adding *-ed: Thi bile is stif and scharp and hoked, Riht so an owel þat is croked.—Artu ihoded?—Theo þe haveþ bile ihoked and clivres wel icroked.*

## THE ADVERB.

### *Use of the Negatives.*

The simple negative *ne*, unaccompanied by another negative

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particle in the same sentence, occurs ninety-five times. It was observed doubled in the same sentence twice only.

*Ne*, *na*, or *ne*, *no* are found forty-one times.

*Ne*, *noht*, occur forty times.

*Ne*, *never*, twenty-eight times.

*Noht*, alone, eight times.

*Nowhit*, once.

*Ne*, *noþer* are found five times.

Three negatives, two of them compound words, are not infrequent: *Nis noþer noht þi lif ne þi blod*, 'Neither thy life nor thy blood amount to aught.'

PREPOSITIONS.

*A* is found frequently, thirty-five occurrences: *And song a feole cunne wise.*

*Buve* = above, is found twice.

*To fore* = before, is also found twice.

CONJUNCTIONS.

The following are common: *Ac*, thirty-five times; *boþ*, *and*; *bute*; *oþer*; *oþer*, *oþer* (*Hwone ich iseo arise feorre, Oþer dairim oþer dai-steorre*). *Ne* (nor); *noþer*, *ne*, six times; *hweþer*, *þe*, five times.

GENDER.

Nouns in the main retain the gender of the same in Anglo-Saxon. The following are the correspondences and exceptions of those nouns to their A.-S. primitives, which appear in connection with the personal pronoun of the third person, or with the masculine accusative ending *-ne* of the adjective.

*Masculines.*

All masculine proper names, and the noun *man*, are of course represented by *he* in all cases.

CORRESPONDENCES.

| *Natural (with Epicenes).* | *Grammatical.* |
|---|---|
| Faucun (French word, mas.) he.—Havec, he.—Hare, he.—Cheorl, he.—Cat, he.—Fox, he.—Thes hundes (gen.)—Fuȝol, him, (m. or n.,) but thane (acc. sing.)—Coc, he. | Drem, he.—Song, he.—Red, hine.—Wrenche, godne.—Deth, fulne.—Wei, rihtne.—Lust, he.—Dom, rihtne.—Cwed, sumne. |

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## *Feminines.*

| *Natural (with Epicenes).* | *Grammatical.* |
|---|---|
| The words *ule* and *nightingale* are always represented by *heo, hire,* or *hi,* save in two instances, where *he,* doubtless by a blunder of the copyist, is employed (cf. personal pronoun). Mose, hire.—Henne, heo.—Lilie, hire.—Rose, hire.—Lefdie, hi. | Stefne, heo.—Murȝthe, heo.—Heorte, heo.—Luve, heo.—Speche, theos. |

## *Neuters.*

| | |
|---|---|
| Word, hit.—Flesch, hit.—Child, hit.—Gome, hit.—Bispel, hit.—Unriht, hit—Neste, hit.—Thing, hit.—Wit, hit.—Blod, hit.—Lond, hit. | Hors, hit. |

### DISAGREEMENTS.

Stoc, m., hit.—Swikeldom, m., hit.—Either ure (O. and N.) he.—Murȝthe, f. hit.—Lepe, m., hit.—Wif, n. heo, hire.—Harm, m., hit.—Geongling, m., hit, heo.—Wrenne, m., heo.—Brid, m., hit.—Maide, heo.—Stunde, sumne.—Brother, hit; (but see connection, 118).

---

NOTE.—It has been found impracticable in the printing to represent uniformly the use in the text of the characters for the *th* and *w*. The MS. A. generally shows *w*, but C. the A.-S. character. Both, however, employ þ.

An erratum occurs on page 30 of the Proceedings, line 6 from bottom: *þah* should be *þat*.

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# PROCEEDINGS.

## SEVENTH ANNUAL SESSION,

### HELD AT NEWPORT, R. I., JULY, 1875.

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# AMERICAN PHILOLOGICAL ASSOCIATION.

NEWPORT, R. I., Tuesday, July 13, 1875.

The Seventh Annual Session was called to order at 3 o'clock P. M., in the hall of the Rogers High School, by the President, Dr. J. Hammond Trumbull, of Hartford, Conn.

An address of welcome was made by the Hon. Samuel Powel, Chairman of the Local Committee, to which the President replied.

The Secretary presented his report, announcing that the persons whose names follow had been elected members of the Association:

Professor John Binney, Berkeley Divinity School, Middletown, Conn.; Mr. W. F. Bradbury, High School, Cambridge, Mass.; Mrs. N. W. DeMunn, Providence, R. I.; President J. M. Gregory, Illinois Industrial University, Champaign, Ill.; Professor George O. Holbrooke, Trinity College, Hartford, Conn.; Mr. Albert H. Hoyt, Boston, Mass.; Mr. J. C. M. Johnston, New Haven, Conn.; Professor D. B. King, Lafayette College, Easton, Penn.; General Albert G. Lawrence, Newport, R. I.; Mr. U. W. Lawton, Jackson, Mich.; Mr. D. P. Lindsley, Andover, Mass.; Professor J. J. Manatt, Denison University, Granville, O.; Professor John Meigs, Lafayette College, Easton, Penn.; Mr. Augustus C. Merriam, Columbia College, New York City; Rev. S. M. Newman, Taunton, Mass.; Mr. C. M. O'Keefe, 45 Willoughby St., Brooklyn, N. Y.; Mr. Wm. T. Peck, High School, Providence, R. I.; Mr. Leonard W. Richardson, Trinity College, Hartford, Conn.; Professor W. G. Richardson, Central University, Richmond, Ky.; Dr. Julius J. Sachs, New York City; Professor Francis W. Tustin, University at Lewisburg, Penn.; Mr. G. H. White, Amherst College, Amherst, Mass.

The Treasurer presented his report, showing the receipts and expenditures of the past year to be as follow:

RECEIPTS.

| | |
|---|---|
| Balance in treasury, July 15, 1874, | $417.98 |
| Fees of 26 new members, | 130.00 |
| Annual assessments, | 545.00 |
| Interest, | 50.32 |
| Donation from citizens of Hartford, | 84.27 |
| Sales of Transactions, | 97.38 |
| | $1,324.95 |

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EXPENDITURES.

| | |
|---|---|
| Printing Transactions, 1873, | $267.75 |
| Printing Proceedings, 1874, | 164.90 |
| Postage, express, and stationery, | 24.70 |
| Secretary's expenses, | 35.50 |
| | $492.85 |
| Balance in treasury, | 832.10 |
| | $1,324.95 |

An investment of $500 is not included in the balances of this abstract.

On motion, Professor Charles H. Brigham and Mr. Charles J. Buckingham were appointed Auditors of the Treasurer's report.

On motion, it was

*Resolved*, That the Association gratefully acknowledge the receipt of $84.27, the generous contribution of citizens of Hartford; and that 50 copies of the volume of Transactions recently published be placed at the disposal of the Secretary for distribution to contributors to the fund of the Local Committee at Hartford.

Professor S. S. Haldeman, of the University of Pennsylvania, Philadelphia, Penn., read a paper on "An English Consonant-mutation, Present in 'proof, prove.'"

In 'proof' and 'prove,' a surd consonant indicates a noun or an adjective, and a sonant indicates a verb. More than one hundred examples of a similar interchange were given: e. g., 'advice' and 'advise,' 'bath' and 'bathe,' 'belief' and 'believe,' 'gilt' and 'gild,' 'grip' and 'grab,' 'practice' and 'practise' ('practize' in Spencer), 'purpose' and 'propose,' 'teeth' and teethe,' 'wife' and 'wife.' Such pairs as 'give' and 'gift' do not belong here, the *f* being due to the participial *t*, which is also present in 'descent' (from 'descend') and many others. Some verbs, as 'bequeathe,' 'crave,' are not accompanied by surd nouns. Many words are used as both nouns and verbs without a change of form: as 'slide,' 'scoff,' 'exercise.' In some cases a change of form would cause confusion with other words: as in 'cease' and 'seize,' 'loose' and 'lose,' 'dose' and 'doze.'

The Secretary read a paper by Professor Edward S. Holden, of the United States Naval Observatory, Washington, D. C., on "The Number of Words Used in Speaking and Writing English."

For my purpose I define a *word* to be a symbol printed in capital letters in Webster's Dictionary, edition of 1852.

In turning over the leaves of a dictionary one meets with three classes of words: 1st, those which one is certain truly belong to him and are constantly used in writing and speech; 2d, those which one might use in writing or very formal conversation, but which it requires a moment's consideration to determine to include or not to include in one's vocabulary; and 3d, those rare or extraordinary words which one unhesitatingly rejects. It is to be noted, however, that technical words

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are not all in this last class, although a large part of this class is composed of them. In counting the number of words in the dictionary which are properly to be included as in habitual use, one's natural tendency is to include too many of the second class spoken of, that is, too many words whose meaning is perfectly well understood, which would be intelligible if met with in reading, and which yet might not be used in a life-time. I have sedulously endeavored to avoid this tendency; and, indeed, I have gone over many of the pages previously examined, finding not more than one *per cent.* of words wrongly marked as my own.

In the unillustrated edition of Webster's Dictionary (1852) there are 1281 pages of defined words. By actual count, 33 selected pages were found to contain 2383 words, giving an average of 72.2 words to a page, and making the estimated number in the whole book 92,488. Then in sixteen different places, so selected as to give as nearly as possible an average number of words, the number used was found to be 1599 out of an estimated total of 4420. This would give 33,456 words in my vocabulary.

An estimate, based on Mrs. Clarke's Concordance to Shakespeare shows that his vocabulary (with the important omission of all verbs which are spelled like nouns) contained over 24,000 words. Similar estimates give over 17,000 for the number of words used by Milton in his Poems, about 7200 for the number in the Authorized Version of the Bible, and about 12,000 for the number of those which occur in the Anglo-Saxon Chronicle.

The estimate made by the Hon. George P. Marsh, that an intelligent man uses in speaking and writing less than 10,000 words, is based on a definition of a *word* different from that which I have adopted. He counts as one all forms which have the same simple or stem, making, for instance, 'lover,' 'loveless,' and 'lovely,' only one word; I have counted all the forms which occur in the list that is given in the dictionary.

A resolution of the Directors of the Redwood Library and Athenæum, extending the privileges of their rooms to the members of the Association, was presented, and the thanks of the Association were returned for the same.

A recess was then taken until 8 o'clock.

## Evening Session.

The Association met in the Unitarian Church, Professor S. S. Haldeman, Vice-President, in the Chair.

The Annual Address was delivered by the President, Dr. J. Hammond Trumbull.

The true student of knowledge is ready to acknowledge himself, with Paul, a "debtor both to the Greeks and to the barbarians." No apology seems necessary for calling you, for a time, away from the beaten paths of classical philology to the vast, half-explored regions on the frontier of language, where are heard only the strange tones and uncouth idioms of savages. Hundreds of these idioms are scarcely known even by name, to linguistic scholars. Yet there is not one of them that might not, if thoroughly investigated, make some valuable contribution

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to the science of language. American scholars cannot too often be reminded how rapidly tribe after tribe is passing away; how many American dialects have, in the last two centuries, disappeared, leaving no trace; how little has been done and how much remains to do for even a provisional classification of all the languages of the western continent. To the so-called "dead languages" of the old world, letters and the art of writing assured survival. The speech of Homer and Aeschylus, of Virgil and Cicero—however marred by modern utterance—is immortal. The language of Egypt is as enduring as her pyramids: Thoth, the god of letters, watched over its long sleep, until in the fullness of time came the unsealing. The Semitic empires of Mesopotamia, and even (if we accept M. Lenormant's determination of the Accadian) their Turanian predecessors, are yet speaking by their incised records. But to an *unwritten* language, when it dies, comes no possibility of resurrection.

The number and variety of American languages seem, at first view, more remarkable than the approximation to uniformity in plan of thought or general structure which establishes among them all a certain family likeness. No accurate enumeration of these languages has been or can be made. Their number has been variously estimated; and one estimate is as good as another, where none can rest on sufficient data.

Is there any bond of union between these innumerable languages, which seem to be radically unlike? Are there characteristic features testifying to the original unity of all, or which at least may serve to distinguish them all, as a class, from languages of the eastern world? The answer must be less confidently given now than it was fifty years ago. As the range of observation widens, broad generalizations are seen to be hazardous. Scholars must be content to rest for the present in Mr. Gallatin's conclusion, that though he perceived and was satisfied of the similarity of character in the structure of all known American languages, he could not define with precision the general features common to all. No morphological classification yet proposed finds a place for these languages to the exclusion of all others. Many of them are as truly *inflective* as the Semitic or the Indo-European. No definition of an inflectional language has been found which can exclude the Algonkin while including the Hebrew. The modification of the root by varying vocalization is as well-marked a feature of the one language as of the other. The inflection by internal change, which makes Arabic *qâtil* 'killing' from *qatala* 'he kills,' is of precisely the same character as that which in the Chippewa (an Algonkin dialect) makes *nēshiwed* 'killing,' from *nìshiwi* 'he kills.' Their separation as a class cannot be established by morphological characteristics. There is a general likeness, but it is in their plan of thought, not in their methods of combining the elements of words or annexing formatives to roots. It is the constant *tendency* to synthesis, rather than the means by which its expression is effected, which characterizes American speech. This tendency is found in all American languages, and, so far as is known, is found in the same degree nowhere else. It manifests itself as plainly in a primary verb as in the 'agglutination' of a dozen syllables.

It may almost be affirmed that Indian speech, pronouns and a few particles excepted, is all *verb*. Every word may be conjugated by moods and tenses, every so-called noun has its preterit and future, its indicative and subjunctive modal qualifications; and every synthesis, however cumbrous, may be regarded as a conjugation-form of a compound verb. The subjective element is as dominant

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in Algonkin as in Aryan speech. The Indian's first thought is self, his next of those 'like' or 'unlike' himself. His impressions of the outer world are received through his desires and appetites. External objects are conceived in their relation to self. His name for *man* is 'like self,' for *woman* 'one who follows,' for *father* 'one from whom self comes' (literally, 'I am *from* him'), for the preternatural 'something beyond' self—*manito*—and this word, very generally employed by missionaries as a name for God, in Algonkin dialects, is in fact formed as a *verb*, from a *participial* of an earlier verb of which the root signifies 'to go beyond,' 'to exceed.'

As every so-called adjective or noun may be conjugated as a verb, from which verb may be formed again, nouns designating the actor, the action, the instrument, etc., and as the formation of every such verb-noun is regular, so that every new name is self-defining, there is absolutely no limit to the possible enlargement of any Indian language. The ease with which, in the principal North American tongues, new words have been framed for new objects and idèas—the formation being always in strict accordance with structural laws—gives ample proof that these languages "have within themselves the power of progressive improvement, whenever required by an advance in knowledge and civilization."

In the devious mazes of American linguistics, it is easy to lose one's way and forget the time. Returning homeward, to say something about a language in which members of the Association have a more direct and selfish interest than in the Algonkin—a language which, in spite of the predictions of Noah Webster, that a "future separation of the *American* tongue was necessary," Americans still love to call *English*—the subject of the proposed reform of spelling was discussed.

There are indications of increased interest in this subject. The popular mind seems awake, as never before, to appreciation of the difficulties, eccentricities, and absurdities of the present standard-English cacography. The remarks of Professor March, in his address to the Association, last year, have been extensively copied, and apparently meet very general approval. Professor Whitney's discussion of the question "How shall we spell?" has helped expose the weakness of the stereotyped objections urged against reform. Legislators are beginning to look at the subject from the economic point of view, as related to popular education, and are considering how much bad spelling costs the country per annum. A bill is now before the legislature of Connecticut for the appointment of a commission to inquire and report as to the expediency of employing a reformed orthography in printing the laws and journals. The "spelling matches" which, last winter, became epidemic, had their influence, by bringing more clearly to popular apprehension the anomalies of the current orthography, and disposed many to admit (with Mr. A. J. Ellis) that "to spell English is the most difficult of human attainments."

Among scholars, there is little difference of opinion on the main question, Is reform of the present spelling *desirable?* The objection that reform would obscure etymology, is not urged by real etymologists. "Our common spelling is often an untrustworthy guide to etymology," as Professor Hadley averred; and Professor Max Müller's declaration that, "if our spelling followed the pronunciation of words, it would in reality be of greater help to the critical student of language than the present uncertain and unscientific mode of writing," receives the nearly unanimous assent of English scholars.

Equally unfounded is the objection that words, when decently spelled, would

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lose their "historic interest." The modern orthography is, superlatively, *un*-historical. Instead of guiding us to, it draws us from, the "well of English undefyled." The only history it can be trusted to teach, begins with the publication of Johnson's dictionary.

The greatest obstacle to reform is the want of agreement among scholars as to the best mode of effecting it. What seems an improvement to one, is regarded by another as an undesirable innovation, or, perhaps, as a new deformity. Few men are without a pet orthographical prejudice or two, and the more unreasonable these are, the more obstinately they are held fast.

Perhaps the most that can be hoped for, at present, is some approximation to general agreement, as to the words, or classes of words, for which an amended spelling may be adopted, concurrent with that which is now in use. A list of words "in reference to which present usage in the United States or in England sanctions more than one way of spelling," is prefixed to Webster's and Worcester's dictionaries. A similar list, prepared under judicious limitations, exhibiting side by side the present and a reformed spelling—and an agreement of prominent scholars, in England and America, that the use of either form shall be recognized as allowable spelling—would go far towards ensuring the success of reform.

It is in compliance with suggestions repeatedly made, and from various quarters, that this subject has been brought to the consideration of the Association. It is for you to decide whether it is advisable to take any action for promoting and directing the popular movement for reformed orthography.

On motion, the thanks of the Association were offered to the President for his address.

The Association thereupon stood adjourned to 9 o'clock Wednesday morning.

### Wednesday, July 14—Morning Session.

The Association met at the High School at 9 o'clock, the President in the chair.

The Secretary reported the election of new members:

Rev. Samuel J. Andrews, Hartford, Conn.; Rev. Homer T. Fuller, St. Johnsbury, Vt.; Professor Richard T. Greener, University of South Carolina, Columbia, S. C.

The Auditors reported that they found the Treasurer's report correct, and it was, on motion, accepted.

On motion, it was

*Resolved*, That a committee of three be appointed by the President, to whom shall be referred so much of his annual address as treats of a reformation of English spelling.

Professor Francis A. March, Professor S. S. Haldeman, and Professor Lewis R. Packard, were appointed such committee.

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Dr. George R. Entler, of Franklin, N. Y., read a paper on "A Comparative View of the Language of Deuteronomy and Jeremiah."

The purpose of the paper was to make a comparison of the grammatical forms, syntactical arrangement, and style of the two books, and to show that the results of such a comparison were opposed to a theory that they both had the same author. For instance: נְאֻם יְהוָה never occurs in Deuteronomy, but is found frequently in Jeremiah; the phrase יְהוָה צְבָאוֹת never occurs in the whole Pentateuch as expressive of the true God, but is employed often in Jeremiah. Also נְאֻם יְהוָה appears in Jeremiah at the beginning of a verse, while אָמַר יְהוָה stands at the end of a verse. The reason assigned for the interchange of these two words is based on their meanings. The former means 'to murmur,' 'to mutter,' 'to speak in a low voice,' being especially used of the supernatural voice which was supposed to whisper oracles in the ear of the prophet. It corresponds to the Greek *μύω* 'to be closed,' 'to be shut,' especially used of the lips and eyes, which is connected with the Sanskrit root *mu* 'to bend,' *mukas*, Latin *mutus*, *musso*, *mutio*. It corresponds also to the Arabic *nama* 'to speak in a low voice.' Gesenius, in his "Thesaurus," controverts Fürst, who derives it from the Sanskrit *nam* 'to bend.' Benfey gives one meaning of the word as 'to sound,' but says that there are no authoritative references. The verb אָמַר corresponds to the Greek *φημί*, which belongs to the root ΦA, whence come also *φάος*, *φαίνω*, and means 'to bring to light,' 'to utter,' 'to say.' Hence אָמַר introduces what is to be said (Numb. v. 12; vi. 2; xv. 2). This explains the use of the infinitive לֵאמֹר (LXX. *λέγων*, *λέγοντες*), as a formula of quotation, particularly after verbs of announcing. The phrase חֵלֶק יַעֲקֹב, a title of God, never occurs in Deuteronomy. בְּתוּלַת יִשְׂרָאֵל in Deuteronomy means an Israelitish virgin (A. V., 'damsel,' Luther and DeWette, '*Dirne*'); in Jeremiah it means the whole people. The phrase עַמִּי is found twenty-eight times in Jeremiah; it occurs also in all of the books of the Pentateuch, except Deuteronomy. So also terms applied to the land and people of Israel are different in the two books. Also, the worship of idols or of strange gods is never forbidden in Deuteronomy under a prohibition of "offering incense" to them, which is often found in the prophet. Jeremiah says, "the Levites, the priests"; in Deuteronomy we find "the priests, the sons of Levi." In Jeremiah the use of the infinitive absolute followed by the finite verb with the conjunction וְ is of frequent occurrence and characteristic; in Deuteronomy it is very rare. In Jeremiah the article with the preposition לְ stands several times instead of the accusative-sign אֵת. Aramaic words, meanings, inflexions, terminations, and constructions are common in Jeremiah, but altogether wanting in Deuteronomy, except in chapters xxxii. and xxxiii. The parallelisms between the two books may be accounted for by the prophet's familiarity with the earlier writings, and his quotations from them and references to them.

After giving an analysis of many expressions used by both writers, the speaker noticed the contrast between Jeremiah and Isaiah. He spoke of the retiring disposition of Jeremiah, and of his likeness to Martin Luther in two respects, dis-



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trust of himself and melancholy, which latter was natural to one who experienced the decay of all hopes for the restoration of national prosperity, and who was accused by those whom he wished to serve. The speaker drew a parallel between Dante and Jeremiah. Both combated authorized teachers of religion, and both were sustained by the hope of blessedness which shall hereafter prevail on earth.

Professor Albert Harkness, of Brown University, Providence, R. I., read a paper on "The Formation of the Tenses for Completed Action in the Latin Verb."

*Esi, carpsi, cecini, fui, alui,* and *amavi,* represent all the varieties of Perfect Formation known to the Latin language. They are inflected as follows:

| | | |
|---|---|---|
| 1. es-i, | 2. carp-s-i, | 3. cecin-i, |
| -isti, | -s-isti, | -isti, |
| -it, | -s-it, | -it, |
| -imus, | -s-imus, | -imus, |
| -istis, | -s-istis, | -istis, |
| -erunt *or* -ere. | -s-erunt *or* -s-ere. | -erunt *or* -ere. |
| 4. fu-i, | 5. al-u-i, | 6. ama-v-i, |
| -isti, | -u-isti, | -v-isti, |
| -it, | -u-it, | -v-it, |
| -imus, | -u-imus, | -v-imus, |
| -istis, | -u-istis, | -v-istis, |
| -erunt *or* -ere. | -u-erunt *or* -u-ere. | -v-erunt *or* -v-ere. |

The most cursory examination of these forms reveals the fact that the endings *i, isti, it,* etc., on the one hand present the most remarkable peculiarities, entirely without a parallel in any other tense in the language, while on the other hand they preserve the most unvarying uniformity throughout all classes of Latin verbs, being precisely the same in the latest derivative as in the earliest primitive. This fact renders it almost certain that they have a common origin in all Latin verbs.

But only three of our representative examples are really independent forms. The others are compounds of auxiliaries—*carp-si* of *esi, al-ui* and *ama-vi* of *fui.* We may therefore dismiss these compounds for the present from our discussion. Moreover *esi* has been already examined in a previous paper, in which we reached the conclusion that it was derived from *asasma,* the original of the Sanskrit *āsa.* In the Latin, *asasma, asasta,* etc., became *esismi, esisti,* etc., finally shortened in the classical period to *esi, esisti, esit, esimus, esistis, eserunt* or *esere.* The steps by which this was effected were all explained. We noticed the disappearance of *s* before *mi* and *mus,* the dropping of the ending *mi* with the lengthening of the preceding *i* in the first person singular. We observed also the disappearance of *s* before *t* in the third person singular. We thus reached in that paper a very simple and natural explanation of the peculiar endings of the Latin perfect in the auxiliary *esi* and its compounds; i. e., in all perfects in *si* and *xi.*

But how are these endings to be explained in *fui* and *cecini*? In *esi,* shortened from *esismi,* the final *i,* as we have already seen, is the remnant of the simple root *es,* with the personal ending *mi.* In the same manner the endings *isti, it, imus, istis,* and *erunt,* all consisted originally of the personal endings added to the root

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*es.* If, then, these endings have a common origin in all Latin verbs, it follows as a matter of course that *fui* and *cecini* are formed from *fu-ismi* and *cecin-ismi*, as *esi* is formed from *esismi;* i. e., that they contain the present of the auxiliary *es, esmi* = *sum.* Indeed I scarcely see how it is possible to look at such forms as *fu-is-ti, fu-is-tis, fu-er-unt* = *fu-is-unt* and *cecin-is-ti, cecin-is-tis, cecin-er-unt* = *cecin-is-unt,* without recognizing the root *es* as an element in the formation, as it lies there entirely undisguised between the principal root and the personal endings.

The fact that this view is not directly supported by the analogy of the perfect formations in the Sanskrit and Greek is a matter of little importance, inasmuch as it is fully supported by the analogy of an entire class of other tense-forms in both those languages. In the use of the auxiliary, *cecini,* as explained above, is entirely analogous to the Sanskrit *a-dik-sham,* and the Greek ἔδειξα.

Our discussion seems to authorize the following conclusions:

I. The Latin, in common with all the cognate tongues of the Indo-European family, inherited a reduplicated perfect, formed by appending the ordinary personal endings to the perfect stem, which was the root reduplicated. Among these primitive perfects was that of the auxiliary, originally *asasma,* which became in the Latin *esismi, esisti,* etc., finally shortened in the classical period to *esi, esisti, esit,* etc. Thus were produced in the auxiliary the peculiar endings of the Latin perfect. This, the original type of the Latin perfect, has been preserved only in *esi.*

II. At a very remote period the Latin formed a compound reduplicated perfect by appending the auxiliary *es* to the perfect stem. Thus, *cecin-ismi, cecini, cecin-eram, cecin-ero,* etc. To this class belong all Latin perfects in *i.*

III. The Latin finally formed a new compound perfect by appending the perfect of the auxiliary to the verb-stem, rarely to the present-stem. Thus:

1. Most consonant stems appended the auxiliary *esi:* as *carp-si, carp-s-eram.*

2. *A, e,* and *i* stems, with some consonant stems, appended the auxiliary *fui:* as, *ama-vi, dele-vi, audi-vi, al-ui.*

Professor W. G. Richardson, of Central University, Richmond, Kentucky, read the next paper, on "Statistics as to Latin Pronunciation in American Colleges and Universities."

Last winter the Bureau of Education, at Washington, (Gen. John Eaton, Jr., Commissioner,) instituted some inquiries with the view of ascertaining the usage of American Colleges. Two hundred and forty-nine colleges had responded. The speaker had been charged with tabulating the results of this correspondence, so as to give, as far as possible, the present status of Latin Orthoëpy in this country as well as in England, Germany, and France. For the information of Latinists, and with the concurrence of the Bureau, he presented the following statistics. He expressed the hope that his paper would not re-open the vexed question of orthoëpy. As a representative of the Bureau, he preferred to preserve an entirely neutral attitude, and to prepare a report which should impartially present every phase of the subject.

The pronunciation of Latin is here classified according to well-known principles, as "English," "Continental," or "Latin" (the last word being used in the same sense as "Roman"). The two hundred and forty-nine colleges are here arranged according to the location, and then according to the pronuncia-

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tion adopted. Of the whole number, 37 per cent. use the "English," 32 per cent. the "Continental," and 31 per cent. the "Latin."

| | |
|---|---|
| **NEW ENGLAND STATES.** | |
| English, | 10 |
| Continental, | 1 |
| Latin, | 2 |
| **MIDDLE STATES.** | |
| English, | 14 |
| Continental, | 11 |
| Latin, | 16 |
| **SOUTHERN STATES.** | |
| English, | 19 |
| Continental, | 18 |
| Latin, | 16 |
| **WESTERN STATES.** | |
| English, | 48 |
| Continental, | 42 |
| Latin, | 37 |
| **PACIFIC STATES.** | |
| English, | 4 |
| Continental, | 7 |
| Latin, | 4 |

Many colleges which are here classified as using the "English" or the "Continental" pronunciation, also employ the "Latin" in teaching archaic forms or for philological purposes.

Mr. Alonzo Williams, of the Friends' School, Providence, R. I., next presented a paper on "Verb-Reduplication as a Means of Expressing Completed Action."

It was the author's endeavor, first, to explain the origin of this form, and how it came to possess the signification of completed action; secondly, to trace its subsequent history and decay, i. e., to what extent the form lost its original signification of completed action, and to what extent the form itself decayed; and, in connection with this, to show by what new forms it was supplanted.

I. Genesis. In all languages we find illustrations of the principle that repetition of a root adds emphasis to the expression; Sanskrit *lù* 'to cut,' *lolùya* 'cutting much'; Latin *meme, tete, sese*, etc. Very early the primitive Aryan people began to employ this method of strengthening their verbs. By the side of the simple present arose the reduplicated present, expressing intensive action. This is the origin of those old reduplicated presents in Sanskrit, Greek, and Latin; all were originally intensive verbs. Indeed the Sanskrit in its latest literary period continued to form intensives, iteratives, and frequentatives, after this primitive method, by reduplication, and the Greek has a few examples of a similar kind. This form gradually assumed the signification of completed action, the mere repetition suggesting the idea that the action has been already once performed.

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Thus arose one of the earliest tense-forms, denoting completed action, a perfect. This must have become fully established before the separation, as we find it bearing this signification in old Indian, Bactrian, Greek, Latin, German, Celtic. The Lettosclavic alone has lost all traces of it.

II. Subsequent History. 1. Sanskrit. In the earliest period this form possessed no other meaning than that of completed action, and this was the only form bearing this signification. Very early, however, it began to lose this meaning, and with decay of *form* dwindled down in *meaning* to a mere past tense in narration. New forms arose to supply its place. (*a*) Periphrastic perfects in *âsa, babhûva,* and *cakâra,* which *are* perfects because of the reduplicated auxiliaries. (*b*) The analytic forms made up of the present of *as,* 'to be,' and a perfect passive participle, are perfects by implication. This participle in *tas,* added to the root, expresses the result of the action, and implies that the action is already completed. (*c*) The combination of the present of the auxiliary and a perfect active participle not only expresses the result of the action, but attributes to the subject the possession of the completed action; thus *uktavân asmi* is not unlike the Latin *dictum habeo* and the English 'have said.'

2. Greek. Throughout its literary history it has preserved the form and signification entire, and no new forms have arisen to rob it of its functions.

3. Latin. Very early the form began to decay. Only about twenty-seven reduplicated forms remain, but many others show traces of it. All in *i* originally contained it. Several new forms arose. (*a*) Those in *si,* as *scrip-si,* contain probably a reduplicated *es,* 'to be.' (*b*) Those in *vi* and *ui* contain the perfect *fui.* (*c*) Of the analytic forms, the periphrastic forms in active and passive are combined with a perfect of the auxiliary; those in *tus* in the passive may be explained like those in *tas* in Sanskrit; those with *habeo* as the similar form in Sanskrit. All these forms, besides their proper signification of completed action, have taken on also the function of a simple past tense in narration.

A recess was taken till afternoon.

## Afternoon Session.

On re-assembling, it was, on motion,

*Resolved,* That a Committee of five be appointed by the Chair to recommend a suitable time and place for the next meeting; and that a further Committee of five be appointed by the Chair to nominate officers for the next year.

The President appointed as the Committee on that part of his address which had reference to a reformation of English spelling, Professor Francis A. March, Professor S. S. Haldeman, and Professor Lewis R. Packard.

Mr. C. M. O'Keefe, of Brooklyn, N. Y., read a paper on "The Proper Names in the First Sentence of Cæsar's Commentaries."

He stated that when, in 1807, the foundation of a scientific and genealogical classification of the human languages was laid by Frederick von Schlegel in his

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*Sprache und Weisheit der Indier*, and the Indo-Germanic family was defined, no conception was formed by the learned German as to the place of the Gaelic in that group. Five years subsequently, however, Pritchard published his "Researches into the Physical History of Mankind," in which the connection of the Celtic with the Indo-European family was adumbrated if not determined. And in 1832 another work—now wholly useless—by the same author, "The Eastern Origin of the Celtic Nations," placed their affinity beyond dispute. In 1837, Pictet's valuable treatise, "*De l'affinité des langues Celtiques avec le Sanskrit*," proved the advantages possessed by Gaelic over the kindred dialects of Wales and Brittany. Pictet was followed by Professor Bopp, who published in 1837 "*Die Celtischen Sprachen*," and he again by J. Kasper Zeuss, whose "*Grammatica Celtica*," on which he spent fourteen years, amazed and delighted the republic of letters. Basing their opinion on the researches of these profound scholars, as well as on Diefenbach and Jacob Grimm, such men as Arnold in England, and Anthon in America, and Thierry in France, considered the word *Gallus* and *Gael* as identical. But this view is not accurate. The word *Gael* signifies an Irishman.* As the word 'Jew' is derived from a Hebrew patriarch named Judah, so the word 'Gael' is supposed to be derived from a primæval progenitor of the Irish race named *Gaedhil*. If they had asked an illiterate peasant who spoke the vernacular what was the meaning of the word *Gael*, he would have told them that it signifies 'a kinsman,' while *Gal* or *Gaul* means a foreigner. Nothing could be more at variance in meaning than these two words. In the Welsh and Breton the word *Gal* signifies 'foreign.' One of the many commentators on "Ossian's Poems" asserts that the ancient Irish were so barbarous as to apply to themselves and to their enemies one and the same name. But on the other hand, it has been reasonably maintained that no people, however rude and ignorant, ever confounded their nationality with that of their foes—that it is not only unexampled, but utterly impossible; and that between himself and the stranger he fights and kills, the warrior of the rudest tribe makes a marked phonetic distinction. This is a very plausible objection which Arnold, Anthon, and Thierry should have considered. On this subject a learned writer says: "Finding thus that the word (*Gal*) means 'foreign' in all the languages where any form of it occurs, the Editor holds until further proof be adduced * * * that the ancient Celtic inhabitants of modern France and northern Italy did never call themselves *Galli* at all; but that, *Gallus* perhaps meaning in old Latin what *Gal* means in Gaelic, the old Itali called their invaders from beyond the Alps *Galli*, because they were strangers; and that the name continued to be applied to the people to whom it had been most particularly given after it had lost its primitive and more extended meaning. So the Anglo-Saxon *Wallisc*—or the English 'Welsh'—has lost its more general signification and it is now forced as a national name upon the Cymri whether they will have it themselves or not."

Now the true explanation lies in the fact that when the Irish were at home in their sea-encircled Erin, they termed themselves *Gaeil*. But when they went abroad, when they invaded what they called *Lochlin*—the continent of Europe—they ceased to be simply *Gaeil;* they became *Gal-Gaeil*—'foreign Irishmen.' The Gaeil inhabiting Alba—the Highlanders of Scotland—may be called *Gal-Gaeil*. This compound epithet occurs in the "Annals of the Four Masters,"

* See "Manuscript Materials of Irish History," by E. O'Curry, volume i., page 3.

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and is explained in a note by Donovan as signifying "piratical Irishmen." It occurs likewise with the same signification in Smerwick's "History of the Clans of Scotland." The Gal-Gacil were roamers of the deep—knights-errant of the ocean, who sallied forth from their island-citadel in search of adventure, gold, and renown. Under Hugony Mor such adventurers with the name of Celts overran Western Europe. The Roman writers having this compound epithet before them, naturally took the first and as naturally rejected the second member as redundant and superfluous. They pluralized *Gal* and termed them *Galli*, which they certainly were in that place. As to the other member, it is a remarkable fact—which has never previously been noticed—that the name foreigners impose on the Teutonic race, which they themselves recognize with reluctance and pronounce with difficulty—the name of 'German'—is identical in meaning with *Gael*. *Germanus* is a translation of the word *Gael*, or, if you will, *Gael* is a translation of *Germanus*. *Gael* signifies 'near akin, closely allied, come of the same stock,' and I need not tell you that *Germanus* has the same signification. Speaking of the *Germani* the Delphin editors say: "*Sic forte a Romanis dicti, quod mutuis auxiliis se juvarent, et communi quodam fœdere essent conjuncti.*" That is: "They received this name from the Romans because they rendered mutual help to one another and were linked together in the bonds of a common confederacy." Strabo in his fourth book, as translated by Pelloutier ("*Histoire des Celtes*," tome 1, page 34,) says: "The Germans resemble the Gauls; their features and customs are similar, and they feed on the same aliments. I am therefore persuaded that in calling them Germans, the Romans meant to convey that they are kinsmen and relations of the Gauls." The words which Pelloutier quotes (*ibid.*) from Dionysius Halicarnasseus, "*quelques Celtes que l'on appelle Germains*," may be translated, "some Celts who are termed Gaels." I have not time or space to show how well the writers of Classic antiquity understood a language which is utterly unknown to modern scholars (Arnold's "Rome," volume i., page 200); I mean the venerable vernacular of Ireland. But knowing that language they naturally and inevitably termed the Irish *Germani*—that is *Gael*. Anthon says, "the term *Galli* is only 'Gael' Latinized." No; it is not the term *Galli*; it is the term *Germani* which is the equivalent of *Gael*.

From this it seems obvious that when Strabo says the Germans were "true Celts," Strabo was right. Speaking on this subject, Arnold says in his "History of Rome," "Dionysius divided the country of the Celts (Κελτική) into two great divisions, which he calls Gaul and Germany" (XIV. 2. Fragm. Mai). Strabo describes the Germans as the most perfect and genuine specimens of the peculiarities of the Gaulish race, and says that the Romans called them Germani, "true," "genuine," to intimate that they were genuine Celts.

We read in a fragment of the *Ephemerides* that Cæsar, in the confusion and tumult of a hand to hand engagement, and mounted on a "termagant steed," was suddenly captured by a Gaulish warrior, who—likewise a horseman—putting his brawny hand on his shoulder, made him prisoner. At that moment the Gaul heard a fellow soldier—possibly a superior officer—exclaim, "*Is Cæsar e*": "*He is Cæsar.*" But he mistook the words; in the disorder and clamor of the combatants, he fancied the speaker to exclaim, "Cast him free—liberate him." Now what words were those which so closely resembled the name of the illustrious Roman? They were these: *caith saer e*, "Cast him free." *Caith* is the second person, imperative mood of the verb *caithim*, 'to fling, to cast,' and *e* signifies 'him.' It is a personal pronoun equivalent to *eum* in Latin. "Throw him

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loose." "*Hoc autem ipse Cæsar,*" says Servius, "*in Ephemeride sua dicit, ubi propriam commemorat felicitatem.*" According to Servius the words used were: "*Cæcos Cæsar.*" This would be written in modern Irish, *Caoc* 'oh! blind man,' *is* 'he is,' *Cæsar* 'Cæsar.'

From this incident, as well as from the geographical nomenclature of the country, and the "Formulas of Marcellus," translated by Jacob Grimm, it appears that the soldiers whom Cæsar encountered were *Gal-Gaeil*—an Irish-speaking people residing in a foreign country. What Leopold Contzen ("*Wanderungen der Kelten,*" p. 92) says of the sacerdotal order is equally applicable to the military caste: "*Von hier war es nach Gallien verpflanzet*"; for this reason: "*In Irland hat sich druidische Lehre am langsten gehalten.*"

The letter *t* in the imperative *caith*, though mute at present, was unquestionably sounded at one time. But when was that? Not when Cæsar was captured by an Irish warrior on a field of Gallic battle. Not 1800 years ago. To find the period when the *t* was sounded we must go back 1800 additional years, to a time—very possibly—when the temple of Belus was not yet mirrored in the waters of the Euphrates, when the sandy desert of Karnak was yet unadorned by the form of a Sphinx. It appears to me that if the *t* were sounded Cæsar would have lost his life on this occasion. The javelin of a Celt might have changed the destinies of the world. But if this be so, it seems evident that Irish scribes have preserved this *t* for more than 2,000 years. "It is a proof of the resistance given by Irish Ollaves and bards to the linguistic corruptions of the vulgar."

The next paper was presented by the Rev. Carl W. Ernst, of Providence, R. I., on "The Structure of the German Sentence."

Such knowledge as people have of language may be divided into three classes: empiric or historic knowledge, scientific knowledge, and philosophic knowledge. The first of these, and especially that knowledge which we have of our mother-tongue, we derive from experience, in an historic and evolutionary manner, by listening to words, whatever they are, by the energy of practice, and by cultivating speech as a fine art. Few people rise beyond this experimental knowledge of language. And it is all-sufficient for purposes outside of ourselves. Fine illustrations we find in the courts of law, in the houses of trading and true business, among those who have something to say. We know a language scientifically in so far as we know it consciously; objectively, in so far as we perceive the living laws which pervade it, though not seen by common eyes. Philosophic knowledge of language is empiric, scientific, and more: it is an art-knowledge, and completely satisfying the subjective requirement. Some minds cannot rise to the full dignity of a dialect; other minds, less circumscribed, go beyond it. But every mind must be absolutely satisfied, must cease to doubt or to believe that imperfect knowledge is unavoidable. We know English and German philosophically when we know them completely, organically, when they give full answer to our last questions. And whatever we know philosophically, that we comprehend by one single intuition. This intuition seems divine before we have exercised it; after we have exercised it, it is no more divine, but the pledge of immortality.

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All persons who reason and are uttering articulate sounds speak empirically; scientific knowledge is the result of historical and original investigation (historical investigation is the acquiring of discoveries made before our day; these discoveries form the body of historical philology); philosophic knowledge involves the very largest empirical knowledge, the knowledge of philological science and scientific philology, and that element which constitutes the artist—genius. And by genius I mean an element which we produce by evolution from our own humanity.

Objectively every language is philosophic and perfection. But this perfection is not always beheld by man. Of Chinese, for instance, we have barely empiric knowledge. But portions of Portuguese, Russian, and Arabic are known to us (I mean to European philology) scientifically. Large portions of English are yet waiting for scientific treatment. Many phases and portions of Greek, Latin, English, French, German, may be known adequately, to perfection, philosophically. Every soul that thirsts for philosophic knowledge must go through the same enchanting process which we admire in those who "gehen auf der Menschheit Höhen."

It will be attempted to treat the structure of the German sentence philosophically.

Speech *seems* to be unlimited, for it is an attempt of reasoning man to reproduce physical and metaphysical realities through the means of articulated sounds. Its source is the universe—the world without man and the world within man, the heavenly constellations, so awful and yet so calm and calming, and the moral law within us, the sleeping emotions that rise marvellously without a bidding in our own small self. The end of speech ends only with the never-ending end of human aspiration. It is imperative to limit the subject, since only limitation promises victory. Fasten the discourse of human speech at a mathematical point, and behold! There are certainly two elements—the physical sound, and the metaphysical thought, or the mental reflex of the object visible or invisible. Certainly, one reflex with its congenial articulation is not human speech; at best it is a word, an interjection it may be, perhaps only an animal exclamation. Speech only begins with the organic and unifying combination, with the living union of thought and thought, articulation and articulation. A may be a word, B likewise; the addition A + B is not speech, but a combination of words; the formula (A + B) is better; the full divinity of speech we have only when we have the truth (A + B) = C. The English expresses this rather felicitously by its use of the words 'infant' and 'person,' 'language' and 'speech.' The unit thus found, logicians call a proposition, grammarians a sentence. *A sentence is the unit of speech;* its smallest appreciable unit and its largest possible effort. The sentence is the circle within which all the possibilities of speech are exhausting themselves.

This understood, we have one element of certainty gained and may prepare for the fruition of all the certainties implied; nay more, for new certainties. The father of modern philosophy, Descartes, compares the conquest of a single certainty to a victorious battle; to have been a victor eight or nine times he considers enough for his entire philosophy. The combining of sentences constitutes the art of rhetoric, poetry, and all literature. The analysis of sentences constitutes the science of etymology and grammar. Nothing great can be done in the study of language unless the field be limited. As soon as we limit ourselves and have discovered certainties, nothing truly great seems to be beyond reach.



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From the nature of the sentence it follows that there can be but *one* sentence, in the same way in which there can be but one square or circle. Every sentence consists of and in a union: the two elements of the union I will call terms. Hence there is but one sentence, a prototype to which all others may be reduced; the sentence consists of terms. In the same manner in which there are imperfect circles, there may be imperfect or unfinished sentences. An unfinished sentence may be made complete by adding to it the one term which it supplements. Terms may consist of one word; but even a whole sentence may be treated as a term.

The two essential parts of a sentence are the subject and the predicate; the subject stands first. The predicate often consists of a verb and its complements; the verb stands first, the complement second. The complement may be a particle, a past participle, or an infinitive; these are arranged in the order indicated. It may be stated incidentally that the particle is always spelled in one word with the past participle or infinitive. Whenever there are any terms besides these, they stand between the verb and its complement, and this constitutes the peculiarity of German sentences. Hence, since the burden of the predicate lies in the complement, the compactness and architectural finish which make German a more excellent instrument for the highest style of art in writing than either French or English.

The terms standing between the verb (and the verb always is in the present or past tense) and its complement are usually objects and adverbs. They are always arranged according to their importance, the most important being the last. When sound and thought go hand in hand, the most important term has also the greatest number of syllables. Often the arrangement is the following: (*a*) a short adverb of time; (*b*) a dative; (*c*) an accusative; (*d*) a prolonged adverbial qualification. As soon as we learn the harmonious coincidences of syllables not heard and syllables heard, we enter the domain either of personal shortcomings or of personal perfection and rhetoric. Any one of these intermediate terms may be made prominent by being placed nearer or entirely at the end of these terms. Another way of making it somewhat emphatic is that of placing it at the beginning of the entire sentence. This is often done to bring variety into the succeeding sentences and to break the monotony of having the subject always first. But always the verb retains its typical place; it is always the second term in the sentence. If the subject cannot be the first term, it is the third. This is also the case in interrogative sentences having an interrogative term. Whenever the interrogative term is wanting, also in conditional sentences that have no conditional term, the verb stands first. The complement stands first only in poetical and highly animated language. The verb stands first also in imperative sentences.

More possibilities of arranging the terms of a sentence there cannot be; and when we know the number of terms and the nature of the sentence we can compute mathematically the number of possible arrangements.

Clauses, or sentences lacking one term, always are linked to this form by a special term, mostly a relative or subordinating conjunction; the verb in all clauses stands last, or after all other terms, including its own complement. It is necessary to indicate with unfailing certainty that a sentence is incomplete, secondary, a mere clause; and this is done by the term that opened the clause and by the placing of the verb at the end of the clause. A clause, just like a complete sentence, may be used as a term.

These laws may be observed to great advantage in the philosophic writers, or rather in the philosophic passages of the great German writers: e. g., Humboldt,

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Lessing, Kant, Fichte, Schelling, Hegel. The difficulty commonly attributed to the letter does not lie in the dimness of their speech but in the weakness of minds that cannot rise to the energy of German philosophy. Such minds are also beneath the philosophic intuition of German sentences. Yet all those being true may rise to its living life, and the truthful shall attain to the rare privilege, that of possessing their own souls.

A recess was then taken until evening.

### Evening Session.

On assembling, the President appointed as the committee on the place and time of the next meeting: Professor E. P. Crowell, Professor C. H. Brigham, Mr. C. J. Buckingham, Professor T. D. Seymour, and Professor W. G. Richardson.

Also, he appointed as the committee to nominate officers for next year: Professor W. W. Goodwin, Professor M. L. D'Ooge, Professor F. P. Brewer, Mr. A. Williams, and Mr. C. D. Morris.

Col. T. W. Higginson then read a paper by Mr. Augustus C. Merriam, of Columbia College, New York City, on "Troy and Cyprus."

The purpose of the writer was, by a comparison of the Cesnola collection of Cypriote antiquities with those dicovered by Dr. Schliemann on the hill of Hissarlik, to show to what extent the "Aryan emblems" of the Schliemann objects are to be found upon the Cypriote, and to exhibit the numerous lines in which the art of Cyprus ran parallel with that of Hissarlik, interlaced with it, or stood as its fountain-head and model.

The pottery of the Cesnola collection may be divided into four classes, of which the first, second, and fourth represent different epochs of time, while the third may or may not be separated chronologically from the second. The first class is from tombs at Alambra, and was found associated with the small terra-cotta warriors and bronze weapons which Lenormant has identified with the Pelasgians who took part with the Teucri and Danai in the invasion of Syria during the reign of Rameses III., thus placing them as early as the 14th century B. C. Confirmatory testimony of the Aryan occupation of Cyprus was cited from Genesis, Homer, and the Egyptian and Assyrian records, demonstrating that the Japhetic element was predominant in the earliest days, while, later, the Phœnician became prominent, and in the 8th century the Greeks ruled the chief towns.

The bronze weapons from Alambra are striking counterparts of those from Hissarlik, and in the spearheads the characteristic noticed by Schliemann, that they fit into the shaft, instead of around it like the later Greek and Roman, prevails here as well. The pottery, like the Hissarlik, is not only destitute of painting, but all ornamentation that is not in raised work has been incised while the paste was soft, and the incisions filled with a white clay to develop the pattern more strikingly upon the red or black ground. The surface has been glazed by a stone-polisher, worked by hand. In shape, parallels are found in the vases with the long, upright, beak-shaped mouth, like No. 105 (Schliemann, English

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Ed.), and those with animal or bird-shaped body (Nos. 114, 151, 152); but no "owl-faced" vessels, or *depa amphikupella*, appear. Their ornamentation is much more profuse than that of their parallels, and is lavished upon the vessels without stint from top to bottom. But the figures are of the same general character (No. 62), parallel zones, bands filled with strokes invariably oblique to their bases, and quadrilaterals similarly filled. Of "Aryan emblems," there are "full suns," "rising suns," both round and angular, zigzags, etc., but not a single *suastika*, or "flaming altar." Besides the vessels, there are 80 terra-cotta whorls exactly similar to those which Schliemann finds so numerously. Nine-tenths of these are incised, and with like figures upon the flatter upper surface, such as Nos. 320, 338, 433, 440, 478, of the English Edition, and Nos. 24, 25, 115, 183, 225, of the Photographic Atlas. As with the vases, the ornamentation is more profuse, not being limited to the upper surface. The longer face, also, is invariably blazoned all around, with figures like those of the vases. They are quite destitute of the *suastika*, and there are, perhaps, no evidences of wear, in reference to which point it is to be remembered that, unlike the Hissarlik whorls, they occur in graves. The Schliemann vases are generally so fashioned that an upright position must be obtained by the addition of three or four legs, or, most commonly, by suspension, for which pierced projections are especially provided. So, all the Alambra incised vessels are without a base, and only five are tripods. But there are some fifteen Egyptian cruses belonging to a variety of which only a single specimen is said by Birch to have been found outside of Egypt, and that at Tyre. These have a pedestal, are turned upon the wheel, and are not incised; but a part are ornamented with a flat, raised, ram's horn curve, like No. 183. Other foreign objects are an Egyptian *lagena*, and a Babylonian *aryballos*. The only signs of paint are upon a small flask and vase, and upon the terra-cotta warriors, whose accoutrements are roughly delineated in red and black.

The second and third classes are from the so-named "Phœnician" tombs at Idalium, situated six and a-half feet below the Greek graves (fourth class). With the second class were found some Egyptian *scarabaei* and Assyrian cylinders. The former may possibly take the place of the whorls of class one. The color of the vases is mostly a lightish yellow, but in shape there are many counterparts of those with incised patterns. Especially numerous are the upright beaks, above fifty, all told. Tripods are infrequent, and the pedestal is coming more into use. Paint has completely usurped the place of incision, retaining, however, many of the same patterns, particularly on the beaked vessels. Beyond these, the stroke is freer, and the lines begin to cross each other, forming small squares and diamonds. None have the *suastika*, except two of a unique variety, and of these a third specimen shows a character which greatly resembles that on the stamp of the Schliemann seal No. 4; also, a Cypriote *pi*, and arrow-head figures like those on the side of the same seal. When compared with others on the same class of objects, they appear to be decorations simply.

The third class differs altogether from the preceding; color of a brilliant or deep red, clay fine, shapes most elegant and perfect in contour, decorations sparing and consisting almost exclusively of concentric circles, the pedestal used in all varieties but one, the upright beak and Aryan emblems wholly wanting. In fact, they appear to be thoroughly Phœnician, as the guide book declares. Hitherto, the devices resorted to for obtaining a clean discharge with a narrow stream from the vessel have been either the upright beak, some modification of that, or the spout projecting from the body. Such is the case likewise with all the Hissarlik

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vases up to the fourth stratum, where the expedient of simply pinching the edge of the *oinochoe* into a convenient mouth, first appears. This is brought systematically into use in Cyprus among these Phœnician objects first. In general, throughout the collection, upon each of the indentations formed by this process of pinching, a round eye is painted.

The fourth class falls precisely under the definition given for the very earliest ceramic ware from Greek sites, such as Mycenae, Athens, etc., and may represent the revival of Hellenic influence after Phœnician domination. However, with a change of ground-color to light drab, many of the types of class three are exactly reproduced both in shape and ornamentation, though their elegance of contour has degenerated. Other varieties differ materially. In a jug with pinched mouth, short neck, and plump body, with a round eye painted on each side of the lip behind the sharp beak, the handle decorated with braids that end in a flourish on the vase like the loose end of a lock of hair, and a throat-band round the neck, from which depends an apron, or breast ornament, little imagination is needed to see as much of a combination of bird and human being as Dr. Schliemann finds and names "Thea Glaukopis Athene." Add to this that upon these jugs the true *suastika* now appears in numbers varying from two to six, and in conjunction with these the simple cross, with the " nail-marks." The collection shows elsewhere three small vases representing owls clearly developed, while on the neck of a large *oinochoe* a real female face is cleverly moulded. The natural tendency of the potter's art to extend the province of its productions beyond the original aim of mere utility, even to the fashioning of the vase into some resemblance to the human form, or animal, or bird, may be seen not only in the vessels from the Mediterranean, but in those from Peru and from the Indian mounds of Missouri, where these forms are frequent. One represents a female figure in a kneeling posture, with hands upon the knees, almost an exact counterpart of which is found in a Mexican idol of stone in the Peabody Museum.

The *suastika* occurs upon nearly a hundred objects of class four, chiefly in the form with bent ends, with which the "nail-marks" are not found, while they regularly accompany the simple cross. None of these objects belong to the type of class three. Other emblems are the "sacred tree," the antlered animals (one being outlined in nearly the same stage as No. 75), birds, and the zigzag of four sections (Atlas, No. 3,001), sometimes enclosed in a rectangle of red. The Maltese cross, which is an Assyrian emblem of the sun-god Shamas, and occurs on numerous *paterae*, is never found but once on the same vessel with the *suastika*. A bronze shield differs from the one discovered by Schliemann, in its circular shape, in the absence of any furrow, and in the flatness of its rim. Its size, too, is less, being about thirteen inches in diameter. Of the circular, tube-like vases (Nos. 130, 287), there are several specimens, one of which has the *suastika*.

The question whether the figures occurring are to be considered "Aryan emblems" with Schliemann and Bournourf, or, with Von Sybel, the A B C of that elementary school of design through which man struggled from the simplest straight, crossed, and crooked lines, to the fuller achievement of completed figures and life-like representations, is foreign to the present purpose. But the facts of this investigation appear to favor the former.

Professor F. A. March, of Lafayette College, Easton, Pa., presented a paper on "Dissimilated Gemination."

It is a general rule that every letter in a word has meaning. The exceptions

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are for the most part due to changes produced by conformation with similar words in which the letter has meaning, or they are connected with the lengthening of letters. Sound gravitates to accented syllables. A long vowel is well represented by writing it twice. When a consonant is prolonged, or held, we hear the closing of the organs on the preceding vowel, and the opening of them on the following vowel, as separate sounds; and represent them by writing the consonant twice: the first *p* in *happily* represents the closing of the lips in *hap-*, the second *p* represents the opening of the lips in *-pily*. The second letter is said to be caused by gemination—a sort of fissiparous generation.

But it often occurs that the following letter, or some other cause, modifies the sound produced by the parting of the organs, so as to render it unlike that produced by their closing, and a strange letter then appears in place of a simple doubling. This may be called dissimilated gemination.

If the term is applied with a certain freedom to all cases where an emerging letter is made by a slight modification of the final movement of the old preceding letter, it affords a convenient classification for a considerable part of the examples of epithesis and epenthesis heretofore unexplained.

The continuous consonants give the most striking examples, and among these the nasals.

The labial nasal *m* is frequently doubled; but the same movement of the organs which makes *m* with the nose open, will make *b* if it be closed; hence we find *b* appearing in place of a second *m*: Anglo-Saxon *slumerian* (Icelandic *slyma*) changes to German *schlummern*, but English *slumber*; so Gothic *timrjan* to German *zimmern*, English *timber*; Latin *numerus*, English *number*. In English the anticipation of the coming *r* closes the nasal veil as the lips are parting, and what would have been the latter *m* turns out a *b*. A similar effect is produced by *l*, as in *fumble* (L. Ger. *fummeln*), *mumble*, *crumble*. It also occurs at the end of words, as *limb*, *numb*, where the *b* used to be sounded. When a surd, as *t* or *s*, follows this dissimilation, it assimilates the sonant *b*, and in place of *m* we have *p*; *empty* (Anglo-Saxon *emtig*), *tempt*, and *glimpse*, *sempster*, *Thompson*.

Quite similar are the changes of the lingual nasal *n*: *nnr* to *-ndr*, as in *thunder*, Anglo-Saxon *punor*; *nnl* to *ndl*, as in *spindle*; *nn* to *nd* final, as in *sound*, *round*, and sometimes by a surd dissimilation *nn* to *nt*, as in *ancient*, *parchment*, etc.

With these are classified the emergence of *t* after *s*, *ss* being dissimilated to *st*, as in *glisten*, from *glisnian*; and final *st* in *midst*, *against*, the second person singular of verbs (*lovest*), and the like.

So also *rr* to *rd*, if found; *ll* to *ld*, as in *alder*; *tt* to *tr*, as in *partridge*, *cartridge*; *dd* to *dr*, if found: and by a further extension of the thought, *uu* to *uw*, *ii* to *ij*, *ig*, as in the Anglo-Saxon and other old inflections.

So also, by dissimilation of the first or closing movement of the doubled consonant, the emergence of *n* before *d* or *s*, as in *messenger* from old *messager* (*g* = *dzh*), *porringer* from *porridge*, *ensample* from old French *essample*; and *r* before *s* or *th*, as in *hoarse* from Anglo-Saxon *hâs*, *swarth* from *swath*.

Lists of words were given exemplifying these changes.

On motion, it was

*Resolved*, That, in order to attend the excursion for which arrangements have been made by the Local Committee, the Association will hold no session to-morrow afternoon.

Adjourned till to-morrow morning.

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THIRD DAY—THURSDAY, JULY 15.

The Association met at 9 o'clock.

The Secretary reported the election of new members:

Dr. J. B. Bittinger, Sewickly, Penn.; Mr. William A. Goodwin, Portland, Me.; Mr. J. A. Shores, Connecticut Literary Institute, Suffield, Conn.; Rev. J. Colver Wightman, Taunton, Mass.

The Committee on that part of the President's Address which referred to a reform of English Spelling, presented a report.

It does not seem desirable to attempt such sweeping changes as to leave the general speech without a standard, or to render it unintelligible to common readers; but the changes adopted in our standards of the written speech have lagged far behind those made in the spoken language, and the present seems to be a favorable time for a rapid reform of many of the worst discrepancies. The Committee think that a considerable list of words may be made, in which the spelling may be changed, by dropping silent letters and otherwise, so as to make them better conform to the analogies of the language and draw them nearer to our sister languages and to a general alphabet, and yet leave them recognizable by common readers; and that the publication of such a list under the authority of this Association would do much to accelerate the progress of our standards and the general reform of our spelling.

They recommend that a committee be raised, to consist of the first president of the Association (Professor W. D. Whitney) and other recognized representatives of our great universities and of linguistic science, to whom the whole subject be referred, and who may prepare and print such a list of words if they think best, and who be requested to report at the next meeting of the Association.

On motion, it was

*Resolved*, That a committee be appointed to take the whole matter into consideration, with power to sit in the recess, and to report at the next meeting of the Association; and that the committee consist of Professor W. D. Whitney, Dr. J. Hammond Trumbull, Professor F. J. Child, Professor F. A. March, and Professor S S. Haldeman.

Professor Franklin Carter, of Yale College, New Haven, Conn., read a paper on "Begemann's Views on the Weak Preterit of Germanic Verbs."

The question underlying this paper was, whether the *d* in 'loved' is itself a preterit and stands for an original 'did.' The question is to be answered by an examination of the earliest forms in the Germanic verbs. Begemann, instructor in the New Academy for Modern Languages at Berlin, has denied the generally-accepted theory of composition (which makes the *d* in English stand as the representative of an old 'did'), and adopted the early supposition of Bopp, that in Gothic and old German, and therefore in the other Germanic languages, this

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preterit was derived from the past participle. Grimm noted a resemblance between these preterits and another form so striking as to overbalance in his judgment this agreement in form between the weak preterit and the participle, namely, the agreement between the inflectional endings of the dual and plural in both weak and strong verbs. Under his influence, Bopp abandoned his idea of the derivation of the weak preterit from an ancient participle, and held that the weak preterit was compounded of the original stem of the infinitive and a preterit, meaning 'I did,' so that Gothic *nasida* would mean 'I safe did,' or 'I safe made.'

The resemblance between the weak and the strong preterit is striking when as in the preterit of *bidjan* we have a stem ending in *d*.

| Weak Preterit of Nasjan. | Strong Preterit from Bidjan. |
|---|---|
| Nasida, | Baþ, |
| Nasidês, | Bast, |
| Nasida, | Baþ, |
| Nasidêdu, | Bêdu, |
| Nasidêduts, | Bêduts, |
| Nasidêdum, | Bêdum, |
| Nasidêduþ, | Bêduþ, |
| Nasidêdun. | Bêdun. |

A resemblance so complete in dual and plural asks to be applied to the singular, and for fifty years the termination of the weak perfect has been identified with the preterit of a strong verb, *dad* or *daþ*, *dast*, *daþ*, *dêdu*, *dêduts*, *dêdum*, *dêduþ*, *dêdun*. This preterit has been referred to the stem Sanskrit *dha*, Greek *θε* in *τίθημι*, Latin *da* in *condĕre*.

In regard to the details of the development of this compound, scholars have not agreed, and Begemann makes much of this disagreement.

Begemann himself helps to establish by forms from Old High German and Old Saxon, that there was once such a strong preterit as would correspond to *daþ* in Gothic, though the verb-root does not exist independently in Gothic or Old Norse, except in a substantive form.

There was, then, earlier than *tëta* in Old High German, a form *tat;* earlier than *dêda* in Old Saxon, a form *dad*. This may indeed prove that *nerita* is not contracted from *neri-tëta*, or even that *nerita* and *tëta* are precisely similar formations, but it does not prove that *nasi-da* is not compounded, or that the last syllable may not be this very strong preterit in Gothic, whose existence in Old High German and Old Saxon is demonstrated. The only possible difficulty about this supposition is, that we do not know any Gothic laws by which the final *þ* could be dropped and the form *nasi-daþ* become *nasi-da*. But it may be wise for all that to believe that the change *did* take place. More than one of Begemann's arguments reduces itself to the "incomprehensible" of this change.

The agreement in form between the preterit and the participle is the strong reason with Begemann for deriving the preterit from the participle. This agreement may be either incidental, accidental, or organic. The agreement is too uniform to be accidental in all cases. If organic, the participle must be derived from the preterit or the preterit from the participle. The participle is the representative of the Sanskrit participle *ta*, *tas*, Greek *το*, *τος*, Latin *tu*, *tus*, and cannot be derived from the preterit. Is the preterit derived from the participle? Begemann says "yes," and that on this explanation all difficulties vanish. He admits that the *ed* of the dual and plural are inexplicable on his theory, but calls this difficulty "an innocent orphan boy" in comparison with the objections to the

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composition theory. The defective preteritive *iddja* is also made to support this theory, apparently because it does not end in *da*. But Grein's explanation of this preterit as for *idida*, he mentions in a note, but does not seriously consider. Apropos of *iddja*, Begemann says: "Linguistic facts do not allow themselves to be adapted to measuring rules." But an anomalous *iddja* without any generating participle is here assumed as the "most brilliant confirmation" of the theory that the weak preterit is the offspring of the participle. Furthermore, in support of this theory an anomalous second person singular *saisost* dictates a second personal singular ending *st* to the preterit of all strong verbs, and becomes in Gothic "eine durchgreifende Regel." But to crown all, the *ddj* in *iddja* suggests and imposes a new form of comparison (*dj*) on all Indo-Germanic adjectives!

Begemann's second treatise deals with the difficulty presented by many that a transitive tense is not to be derived from a passive participle. Building on Von der Gabelentz's treatise in the reports of the Royal Saxon Society, he endeavors to prove these propositions: (*a*) passivity developes itself from activity through the medium of reflexiveness; (*b*) reflexiveness is expressed formally, or results from the conception and remains unmarked; (*c*) in the verb the usage is various, while in the noun reflexiveness lies only in the conception. The different Indo-European languages are examined and found to contain many participles, passive in form with active significations. The Gothic presents fewer than the Middle German. Begemann claims that it is because of the scantiness of the records. But it seems most improbable that enough past participles retained an active meaning in the primeval Germanic period to give an active meaning to all words or verbal forms derived from them.

The analogy between the participial perfect in the Iranian languages and this preterit is exhibited. But the analogy is *first* assumed, and the Lithuanian, which, according to its investigators, is the connecting link, both in grammar and word-fund, between the Germanic and the Aryan, presents us a compound past tense, possibly a compound of the very stem which has been found in the final syllable of *nasida*. An analogy from the Hungarian, belonging to the Finnish class of languages, is worth little here.

The organic development of the weak preterit from the ancient participle is then not proven. But is it incidental, that is in many cases the result of assimilation? Such is the meaning of Bopp's "Schutzbündniss," and such must be the truth, as for instance, in English the *l* of *would* has forced its way into the preterit of *can*, and the *o* of the preterit of *will* is found in *won't*. This incidental agreement cannot exclude the accidental in some cases, as in *nasi-da*, *nasips*.

Dropping derivation of the preterit from the participle, "the incomprehensible" of the loss of the final *d* in *nasi-dad* (the first form according to the composition theory) and the contraction of the appended verb in the Old High German plural is rendered somewhat natural by the loss of the dual in the other old Germanic tongues. This loss shows a tendency to disregard the fulness of the old inflectional forms. Moreover, the persistence of the vowel personal-endings in Old High German and Old Norse involves fuller vowels and stronger elements than those of the personal-endings of the presents or participial stem-endings in *a* will account for.

The old theory must be regarded in view of these facts and considerations as not overthrown by Begemann, though great credit is due him for the establishment of certain points bearing on the question.



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Professor J. B. Sewall read a paper by Mr. William A. Goodwin, of Portland, Me., on "The Word 'Juise'—Its Pronunciation in Worcester's and Webster's Dictionaries."

Both Worcester and Webster give the word 'juise,' pronounced *jus* with a long *u*, and defined 'justice,' 'judgment,' referring to Gower and noting the word as obsolete. In Pauli's edition of Gower's "Confessio Amantis," at least two instances of the use of *juise* are found. The first is as follows:

All sodeinly the stone shall fall
As Daniel it hath beknowe,
Which all this world shal overthrowe;
And every man shal then arise
To joie or elles to *juise*. (Vol. i., page 38.)

This must be scanned so as to make *juise* a dissyllable, rhyming with *arise* and pronounced *jew-ize*.

Again:

And saide unto her in this wise:
O beste of helle, in what *juise*
Hast thou deserved for to deie (Ib., page 202.)

This is clearly a dissyllable, the accent falling on *ize*. Chaucer spells the same word 'jewise,' and Halliwell's Archaic Dictionary has it defined 'punishment':

I am thy mortal fo, and it am I
That loveth so hot, Emelie the bright,
That I wold dien present in hire sight.
Therefore I axe deth and my *jewise*,
But sle my felaw in the same wise. (Cant. Tales, 1741.)

The word is still a dissyllable, rhyming with 'wise.'

Another instance from Chaucer:

The king commanded his constable anon,
Up peine of hanging and of high *jewise*,
That he ne shulde soffren, in no wise,
Custance within his regne for to abide. (Ib., 5215.)

Another form of the word appears in Wright's edition of the "Deposition of Richard II." (page 26), as follows:

Ther nas rial of the rewme that hem durste rebuke,
Ne juge ne justice that *jewise* durste hem deme.

This has no strictly limited measure, being simply alliterative verse; but making the cesural pause after 'rewme' in the first line, and 'justice' in the second, 'jewise' steps off promptly on its two feet where 'juice' might limp on one.

Still another form occurs in Wright's edition of Piers Ploughman (page 392):

And if the kyng of that kyngdom
Come in that tyme
There feloun thole sholde
Deeth or oother *juwise*,
Lawe, wolde he yere hym lyf
If he lokyd on hym?

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This is also alliterative verse, with scarcely as much of metre as can be found running throughout "The Deposition"; and, contrary to the before named examples, the accent naturally falls on the first syllable of the word; but it will still not be 'juice' but 'jewise,' following the trochees 'kyngdom' 'tyme,' 'feloun,' 'sholde,' and many others in the immediate context. From these instances, which are all I can now adduce, I do not think there can be any *juice* in the word, and I fear that the discussion of the question will prove to be dry reading; howbeit, "fair play is a jewel." Would it not be a luxury to roam about among our letters and combine them at will, as the above-quoted eminent spellers did, without fear of being snapped up by any school-boy fresh from a spelling-match?

Mr. Charles D. Morris, of Lake Mohegan, Peekskill, N. Y., read the next paper, on "Some Forms of Greek Conditional Sentences.".

This paper was designed to criticize certain statements of Professor Goodwin as to the import of some forms of the Greek conditional sentence, as laid down in his books and enforced in a paper read before the Association at its Easton meeting. The point specially controverted was, that between conditions expressed by *ἐάν* with the subjunctive and *εἰ* with the optative there is no distinction except that the former is a "more vivid" mode of statement than the latter. It was maintained on the contrary that, if sentences truly typical be selected, it can be seen that one of these forms cannot be substituted for the other without introducing a change so great that, while the one is felt to be perfectly appropriate to the circumstances, the other could not have been used by the speaker without his being conscious that he was talking nonsense. The passages quoted to establish this position were Aesch. Agam. 36:

*οἶκος δ' αὐτός, εἰ φθογγὴν λάβοι,*
*σαφέστατ' ἂν λέξειεν,*

and Arist. Nub. 754:

*εἰ μηκέτ' ἀνατέλλοι σελήνη μηδαμοῦ,*
*οὐκ ἂν ἀποδοίην τοὺς τόκους.*

These were written on the blackboard, and parallel to each the following suggested modifications:

*οἶκος δ' αὐτός, ἢν φθογγὴν λάβῃ,*
*λέξει σαφέστατ',*

and

*ἢν μηκέτ' ἀνατέλλῃ σελήνη μηδαμοῦ,*
*οὔπως ἀποδώσω τοὺς τόκους:*

and the question was submitted to the judgment of the Association, whether the watchman or Strepsiades could possibly have expressed himself in the latter way; and the opinion was strongly maintained that no instance can be found in which a future supposition as to a thing *known to be impossible*, such as a *change in the order of nature*, is expressed otherwise than by *εἰ* with the optative, unless indeed it is expressed otherwise for rhetorical purposes, or in the manner of a prophet.

The speaker controverted also the statement of Professor Goodwin, that the proverbial expression, "if the sky falls we shall catch larks," must be translated by *ἢν* with the subjunctive; as

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*ἢν γὰρ πέσωσιν οὐρανοί, μάλ' εὐχερῶς*
*τώρνίθια ληψόμεσθα:*

and it was maintained that, unless expressed rhetorically or prophetically, it must be rendered:

*εἰ γὰρ πέσοιεν οὐρανοί, μάλ' εὐχερῶς*
*τώρνίθι' ἂν λάβοιμεν*

The conclusion was, therefore, that *εἰ* with the optative expresses a supposition lying consciously within the range of the ideal, while *ἢν* with the subjunctive expresses one to which attaches a greater or less expectation that it will or may conceivably come within the range of the actual; and that, while in a large number of instances the thought may be expressed in either one way or the other according as the mind of the speaker happens to regard the matter, still, if the character of the supposition be such as to necessitate a consciousness of the nature of the case, one form will be necessary to the exclusion of the other.

Professor Fisk P. Brewer, of the University of South Carolina, Columbia, S. C., next read a paper on "The English Suffix *ist*."

It is a common observation that many nouns have been formed lately with the ending *ist*. This suffix in such words as 'artist,' 'jurist,' and 'evangelist,' has been introduced into English from the Greek, where it is a compound. It differs from the old agent-suffix *er* in being more limited. It denotes only the personal agent, while a noun in *er*, as 'baker,' may denote either a person or a thing. Where there is a cognate verb in *ize*, as 'eulogize' or 'plagiarize,' the noun in *ist* may signify the person who does an individual act; but all other derivatives in *ist* are formed from nouns, substantive or adjective, and they denote only the habitual agent. Thus, a 'copyist' is one who makes a business of copying; but one who copies only as occasion requires, is a 'copier,' not a 'copyist.' Among habitual agents are included those whose business concerns itself with some particular article, as 'tobacconists,' or department of knowledge, as 'philologists,' and those who advocate some theory, as 'Darwinists,' or some policy, as 'inflationists.'

With the progress of civilization there is going on a great subdivision of employments and of departments of learning, and it is fortunate that a suffix has been found in English which can be used almost exclusively for forming names of men with reference to their business and pursuits, their theories and principles. Its increasing use is justified by its utility.

Professor F. A. March, of Lafayette College, Easton, Penn., read the last paper of the morning, on "The Immaturity of Shakespeare as shown in Hamlet."

An examination of the works of Shakespeare in the order of their composition shows that he rose very slowly to the hights of his power. He worked for years dramatizing popular tales with a comic vein, and then years more on patriotic parts of English history, before he tried the grand tragic style. After the love story of Romeo and Juliet, Hamlet was his first tragedy, and it has some of the defects as well as the merits of such a work. It was probably long in hand. The following topics were discussed to exhibit traits of age or immaturity:

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1. The meter. The formal metrical peculiarities of the early plays were pointed out and the later changes. In Hamlet, it was said, the early rhymes and formal restraints have gone, but there is still care and finish, perfect art without the negligences of the latest period.

2. There are many things which are not natural utterances of the characters to carry out the thought of the play; but good things brought in to make hits:

Allusions to matters of the day, such as the talk about the children players; Act ii. 2, the actor who played Hamlet, "fat and scant of breath"; and perhaps allusions to Mary Queen of Scots.

Taking off the fashionable style of speech, as in Polonius's imitation of Euphues, and the ranting passage of the player in the style of Marlowe.

Good things from his own common-place book, such as the advice to players, and large parts of the soliloquies, on the badness of the world in general, the effect of prayer, and the like.

3. The want of lively characterization of the subordinate characters. Many of them talk a good deal, but they leave no impression.

4. The youthful point of view from which the characters are seen. Ophelia is ripe in age; her sagacious father is a superannuated bore. Doubt is depth. Made up minds seem superficial. Not so with Miranda and Prospero, or Perdita and Polyxenes.

5. Immature view of the problems of life and death. The writer is wrestling with them. By and by Shakespeare quietly gave them up, and was a cheerful believer that "we are such stuff as dreams are made of, and our little life is rounded with a sleep."

6. Immature treatment of the ghost. In the later plays the ghosts are apparitions of unhinged minds; the Hamlet ghost is the simple ghost of the story-books, visible to vulgar eyes, and what with his poses and long-winded declamation on the stage, and his moveable subterranean noises, is a common-place creation, a "poor ghost." Hamlet does not quite believe in him.

7. Immature treatment of insanity. Shakespeare had not so fully mastered this subject as to give the reins to his imagination, but made Hamlet and Ophelia speak by a theory, according to which the intolerable grossness of Hamlet was the necessary utterance of madness in his circumstances. The writer of Lear would have felt that such grossness was no subject for art.

8. The general atmosphere of lechery.

9. The character of Hamlet is not brought to unity. Some passages seem to have been taken up from the old play, in which Hamlet has a different character from Shakespeare's prevailing thought of him. This, combined with the defective handling of his insanity, is the solution of the enigma of his character.

A recess was then taken until evening.

## Evening Session.

The Secretary reported the election of new members:

Professor W. H. Whitsitt, Southern Baptist Theological Seminary, Greenville, S. C.; Professor W. B. Owen, Lafayette College, Easton, Penn.

On recommendation of the Executive Committee, it was adopted

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as a standing rule, that no paper read before the Association shall exceed forty-five minutes in length, except by special vote of the Association.

On motion, it was

*Resolved*, That the Executive Committee be instructed to forward to the members of the Association, with the notice of the annual meeting, such information as may have previously reached them as to the subject matter of papers intended to be presented to the Association; and that to this end each member of the Association intending to present any paper be requested to inform the Executive Committee of its title at least two months before the meeting.

The committee on the place and the time of the next meeting recommended that the next meeting be held in New York City, on Tuesday, July 18th, 1876.

On motion, the report of the committee was accepted, and the recommendation therein contained was adopted.

Dr. L. A. Sherman, of New Haven, Conn., read a paper on "Some Facts from a Grammatical Analysis of 'The Owl and the Nightingale.'"

Attention was called to the small research which has as yet been made into the grammatical forms and usages of the English language, in the middle period between Anglo-Saxon and Chaucer. Manifestly nothing can be affirmed with exactness concerning English at this stage, until order has been brought out of the chaos of individualities, and all differences of vocabulary and inflection have been brought to light and classified by careful analysis. From such an examination into the grammatical character of the Southern English poem of "The Owl and the Nightingale," a few facts were quoted. The poem is in many respects remarkable. It appears to have been written by a priest, and not earlier than 1250; but there is no certainty as to its author or its date. In spite of the comparatively late date of its composition, it shows, first, a singularly close adherence in inflections to the Anglo-Saxon norm; secondly, a like careful adherence, in the main, in the gender of its nouns to their Anglo-Saxon primitives; thirdly, an unusual paucity of French words.

On the preservation of inflections the first feature noticed is the continued presence of the strong and weak declension in the noun and adjective. The strong has nearly unified the dative and accusative cases in both numbers, and has begun to employ *-s* as a plural ending to feminine and neuter nouns. The weak nouns have changed *-an* to *-e*. The adjective employs strong and weak forms in the same way as Anglo-Saxon with but very few exceptions, and has shortened *-an* to *-e*. The pronoun has begun to lose the distinction between the dative and accusative relation. The pronoun *hwo* is only interrogative, *þe* and *þah* being employed as relatives.

The verb is almost entirely unaltered. The plural of *am, art, is*, is always *beoþ*, which form occurs five times also for *is*.

In negative sentences as many as three negatives are frequently met with, but two must be *compound*.

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In gender, the masculine is still found in such words as *drem, song, red, wrenche, dep, wei, lust, dom, cwed;* the feminine in *stefne, murpe, heorte, luve, speche.* Not infrequently the nominative singular of nouns shows an inorganic *-e.*

A paper by Professor W. W. Goodwin, of Harvard College, Cambridge,, Mass., entitled "Remarks on Some Points of the Solonic Legislation," was read by title in the absence of the author.

The committee to nominate officers for the next year, presented nominations as follows:

For *President*—Professor Albert Harkness, Brown University, Providence, R. I.

For *Vice-Presidents*—Professor S. S. Haldeman (of the University of Pennsylvania), Chickies, Penn., and Professor Frederick D. Allen, Cincinnati, O.

For *Secretary and Curator*—Professor Samuel Hart, Trinity College, Hartford, Conn.

For *Treasurer*—Mr. Charles J. Buckingham, Poughkeepsie, N. Y.

For additional members of the *Executive Committee*—

Chancellor Howard Crosby, University of New York, New York City.

Professor James P. Boyce, Southern Baptist Theological Seminary, Greenville, S. C.

Professor W. G. Richardson, Central University, Richmond, Ky.

Dr. J. Hammond Trumbull, Hartford, Conn.

Professor Wm. D. Whitney, Yale College, New Haven, Conn.

The report was accepted, and the persons therein named were declared elected to the offices to which they were respectively nominated.

On motion, it was

*Resolved,* That the thanks of this Association are due to the citizens of Newport, who have so cordially tendered their hospitalities and so generously provided for the comfort of the members present at this meeting, and particularly for the very pleasant excursion to Rocky Point; to the gentlemen of the Local Committee, for their kind attentions; to the Newport School Committee and the authorities of the Unitarian Society, for the use of the Rogers High School building and of the church edifice; to the officers of the People's and of the Redwood Libraries, for the kind invitations received from them; and to Mr. A. J. Ward for the copies of *The Daily News* furnished to the members.

The minutes of the meeting having been read and approved,

On motion, the Association adjourned.

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## 1875–6.

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EXECUTIVE COMMITTEE.

The officers above named, and—
HOWARD CROSBY,
JAMES P. BOYCE,
W. G. RICHARDSON,
J. HAMMOND TRUMBULL,
WILLIAM D. WHITNEY.

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# TRANSACTIONS

OF THE

# AMERICAN

# PHILOLOGICAL ASSOCIATION.

1876.

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# CONTENTS.

I. On εἰ with the Future Indicative and ἐάν with the Subjunctive in the Tragic Poets.
By Professor B. L. GILDERSLEEVE. . . . . 5

II. On Grote's Theory of the Structure of the Iliad.
By Professor L. R. PACKARD. . . . . 24

III. On Negative Commands in Greek.
By Professor M. W. HUMPHREYS. . . . . 46

IV. On Hebrew Verb-Etymology.
By Professor C. H. TOY. . . . . 50

V. A Botanico-Philological Problem.
By Professor W. D. WHITNEY. . . . . 73

VI. On 'Shall' and 'Should' in Protasis, and their Greek Equivalents.
By Professor W. W. GOODWIN. . . . . 87

VII. On Certain Influences of Accent in Latin Iambic Trimeters.
By Professor M. W. HUMPHREYS. . . . 107

VIII. On the Algonkin Verb.
By Dr. J. HAMMOND TRUMBULL. . . . . 147

IX. On a supposed Mutation between L and U.
By Professor S. S. HALDEMAN. . . . . 172

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TRANSACTIONS

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# I.—*On εἰ with the Future Indicative and ἐάν with the Subjunctive in the Tragic Poets.*

BY BASIL L. GILDERSLEEVE,

PROFESSOR OF GREEK IN THE JOHNS HOPKINS UNIVERSITY.

ON looking over the proceedings of this Association for the last few years, I have noticed that the conditional sentence in Greek has had at least its full share of attention; and I should hesitate to introduce the subject at this meeting, especially in the absence of the distinguished scholar whose treatment of this important class of sentences has found so wide an acceptance in this country, if the paper which I had prepared in the hope of meeting Professor Goodwin here were not of a statistical rather than of a theoretical character.

At the same time, a general theoretical preface is necessary in order to make my results intelligible; and I will endeavor to state, as simply as possible, the view of the conditional sentence which I have found to be serviceable in the work of practical instruction.

In common with most grammarians, I divide the conditional sentence into four classes, for which I have been in the habit of using the designations "Logical," "Anticipatory," "Ideal," "Unreal." If nothing more can be said in behalf of this nomenclature than that it saves time, something at least has



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been said; and I am glad to learn that a part of this nomenclature, as applied to the Latin language, has found favor among teachers. Logical, Ideal, and Unreal conditions occur in Latin also. The Anticipatory is peculiar to the Greek.

I. 1. The *Logical* condition states the elements in question. It is used of that which can be brought to the standard of fact; but that standard may be for or against the truth of the postulate. All that the Logical condition asserts is the inexorable connection of the two members of the sentence. Aeschines gives us a good type (3, 188): *εἰ τοῦτ' ἔχει καλῶς, ἐκεῖνο αἰσχρῶς, εἰ ἐκεῖνοι κατ' ἀξίαν ἐτιμήθησαν, οὗτος ἀνάξιος ὢν στεφανοῦται.*

It is the favorite form of condition in argument. So the chorus in Thesmoph. 789 makes a home thrust when it says: *εἰ κακόν ἐσμεν, τί γαμεῖθ' ἡμᾶς, εἴπερ ἀληθῶς κακόν ἐσμεν;*

It is the fairest form, and so Demosthenes as he introduces it (18, 10) says: *θεάσασθε ὡς ἁπλᾶ καὶ δίκαια λέγω.*

I call it the Logical condition on account of this argumentative use, and compare it with the indicative question and corresponding indicative answer.

2. The Logical condition, like every other form of the conditional sentence, is particular or generic according to the character of the apodosis. Hence when it has its apodosis in the present, it has a double meaning, which adapts it admirably to personal argument. So especially when the form *εἴ τις* is used, which may point either to a definite or to an indefinite person, the Logical condition is a two-edged sword, often wielded in the keen encounter of Attic wit. But as the *ἐάν* conditional with a present indicative apodosis is regularly generic, it is not without reason that this form should be preferred, when distinctly generic action is to be expressed. Just as the conative element is not so distinct in the present as in the imperfect, simply because the present has the double function of a present of continuance and a present of attainment, so the *ἐάν* conditional is more distinctly the generic conditional. But take such an example as AESCHIN. 3, 196: *εἴ τις ἐν δημοκρατίᾳ τετιμημένος τολμᾷ βοηθεῖν τοῖς παράνομα γράφουσι, καταλύει τὴν πολιτείαν ὑφ' ἧς τετίμηται;* and I am unable to see why such a gnomic sentence

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should not be generic.* And when we turn from prose to poetry, we find the Logical condition freely employed in generic as well as in particular relations, doubtless because poetry loves the more concrete form. But here again we must distinguish; for in the epic the subjunctive is the regular construction in generic relations, while we find the Logical condition more frequently in dramatic poetry than in prose. Note also that proverbs delight in logical forms.

II. In the *Anticipatory* conditional sentence the action of the protasis is anticipated. For this form of the condition we want a word that will harmonize present and future. Anticipation is not expectation, though it is loosely used for expectation and may be stretched to cover it. Anticipation treats the future as if it were present; and, as we find a useful parallel for the Logical condition in the simple indicative question, so we can best illustrate the Anticipatory conditional by the imperative, as Curtius and others have done.

The Anticipatory conditional was divided thirty years ago by Bäumlein (notably in his *Griechische Modi*, *s.* 219 and 221) into the particular and the generic, according to the character of the apodosis; and I have found this division of no little practical service in my experience of twenty years. Professor Goodwin, as is well known to you, came to the same result independently and made a more extensive application of the analysis—too extensive, as he himself has admitted with a frankness which does him all honor.†

The Anticipatory conditional, then, as I will permit myself to call it, is the more common form of the generic conditional and the usual form of the toneless future conditional—of the postulated future. How it differs from εἰ with the future indicative or logical future, it is the object of this paper to illustrate rather than to prove.

It may be worthy of especial note that the Anticipatory condition is invariably used in laws, and it may also be called the Legal condition.

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* Examples of the generic logical conditional might be multiplied indefinitely: see Xen. Oec. 11, 24; Plat. Legg. 9, 865, a.b; Dem. 23, 54 (an instructive passage).

† See Transactions of American Philological Association for 1874.

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In the early language and occasionally in Attic poetry, rarely in Attic prose, εἰ with the subjunctive is found in very much the same sense as ἐάν with the subjunctive. I am not disposed to refine on the difference; but I think that I have noticed that the tone approaches εἰ with the future indicative. This form, εἰ with the subjunctive, seems to be the older, and hence, when there is a transfer to *oratio obliqua*, the conditional appears as εἰ with the optative, and so the apparently exceptional disappearance of an *oratio recta* ἄν is satisfactorily accounted for.

In these transfers of the Anticipatory conditional to the past, we have again the division into particular and generic—for the so-called optative of indefinite frequency is nothing but the *oratio obliqua* of the subjunctive; for even when the *oratio obliqua* is not formally expressed, it lies in the notion of will, inclination, habit, which is involved in rule of action.

As the Ideal and Unreal conditionals do not come within the scope of this paper, I may say briefly that

III. The *Ideal* condition (εἰ with the optative—optative with ἄν) is the condition of the fancy. There is often an element of wish for or against, of hope, of fear. It is the great condition of illustration, and is often used in comparisons, where those untrained in Greek modes of conception would expect the Unreal condition.

IV. In the *Unreal* condition (εἰ with indicative—indicative with ἄν), the protasis is against reality. It is the other side of the Logical, and, like the Logical, is often used in argument. The formula of the Unreal condition is, in my judgment, too narrow, and the opposition should be represented as opposition to continuance, attainment, and completion, and not to past and present simply.

But it is time to return to the point under special consideration, the relation of the Logical Future Condition and the Anticipatory.

It is a matter of every-day grammar that ἐάν with the subjunctive is the common form of the future condition in Greek, and the parallel is often drawn between ἐάν with the

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present subjunctive and *si* with the future indicative, ἐάν with the aorist subjunctive and *si* with the future perfect indicative; and it would be well, in my opinion, if teachers were to limit themselves to this statement, and leave out the more or less foreign elements of probability and practicability. It is very true that the anticipatory conditional has to do with practical matters chiefly, but so has the parallel imperative, and yet the imperative is not bound by probability or practicability, and when Menelaus says to Orestes (Eur. Or. 1593):

ἀλλ' οὔτι χαίρων, ἤν γε μὴ φύγῃς πτεροῖς,

"But to thy sorrow, an thou do not 'scape by wings,"

there is a sheer contradiction of the course of nature. So Eur. Phoen. 1216: ἤν μή γε φεύγων ἐκφύγῃς πρὸς αἰθέρα.

The fact then is patent enough to every one who will be at the pains to count, that for model Greek prose ἐάν with the subjunctive is preferred to εἰ with the future indicative. The reason of this seems to be to a considerable extent the greater temporal exactness, the same greater temporal exactness which has wholly displaced the future indicative with the temporal particles, the same greater temporal exactness which has given so wide a sweep to the optative with ἄν as a sharper form of the future.

In future relations, εἰ with the future indicative may be dissected into ἐάν with the present subjunctive and ἐάν with the aorist subjunctive; and hence, whenever it is important to distinguish continued from concentrated action, whenever it is important to distinguish overlapping from priority, ἐάν with the subjunctive is preferred, not only in general sentences but in particular sentences; and it is with particular sentences that we have to deal in this essay.

Now the neglect of this distinction in εἰ with the future indicative shows a certain coldness, a certain indifference; and this added to the general rigor of the logical condition, which faces fact in all its grimness, gives a stern, minatory, prophetic tone to the future indicative, which commentators and grammarians have noticed, but noticed only in passing, and noticed without attempting to account for it. Especially familiar is the combination εἰ μή τις, in which τις is often a

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covert second person. In the practical handling of the language in Greek composition, I have made it a rule to exclude εἰ with the future indicative except in harsh threats, solemn warnings, in passages where δεῖ or μέλλω would be appropriate (so-called modal use of the future), and with verbs and phrases of emotion.

In striking contrast to the comparative infrequency of the combination εἰ with the future indicative in prose (and I may add in epic poetry), is the frequent use of it in the tragic poets; and having had the curiosity to examine the comparative usage of Aeschylus, Sophocles, and Euripides in this regard, I have thought that the results might be of some interest to the Hellenists of this Association. In preparing my statistics, I have excluded all clearly generic sentences; but even if these should be included, the result would not be materially affected; and, though I at one time intended to meet any possible objection based on the mechanical *metri causa*, I found subsequently that the usage of Aristophanes completely disposed of any such superficial cavil. In spite of fashionable sneers at oversubtlety of distinctions and overrefinement of syntax, I am a firm believer in the organic unity of artistic character; and I should expect to find a specific difference under the common type in every fibre of the living organisms of Greek literature. Surely if the mechanical structure of the verse of Vergil and the verse of Ovid reflects the diverse ideals of the two poets, we may expect to see in the varying employment of the sterner forms of expression a manifestation of the character of the three great tragic poets. I grant, in advance, that the induction is not so wide as I could desire in the case of Aeschylus, who seems not to delight in conditional expressions so much as Sophocles, and far less than the lawyer-like Euripides; but even this is a significant fact and may deserve a closer notice.

As I have already intimated, the three tragic poets are faithful to the same general type. They all deal largely in the future conditional, but in a decreasing ratio—always far exceeding the normal usage of prose, but still differing in their individual conception.

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In brief, of the future conditions in Aeschylus over seventy-three per cent. have the future indicative, in Sophocles over fifty-four per cent. (and if we leave out the Oedipus Coloneus and Philoctetes, as I think we may, as much as sixty-two and one-half per cent.), while Euripides, approaching, as he does, more nearly to the language of prose, puts the majority of his future conditions in the *ἐάν* form, so that only about forty-three per cent. have *εἰ* with the future indicative.

Of course, I cannot undertake to discuss the hundreds of conditional sentences which I have examined in this way, nor even to comment on the more striking forms. A condensed tabular exhibit has been prepared to accompany this paper.

I would only add that, so far as I have observed, *εἰ* with the future indicative and *ἐάν* with the subjunctive are seldom used in antithesis. Where such antitheses occur they are in the main confirmatory of the difference of tone already recognized. So ARISTOPH. Nub. 586. 591, in which *εἰ στρατηγήσει Κλέων* presents the threatening, unfavorable alternative, *ἢν Κλέωνα φιμώσητε* the favorable view. So in XEN. Cyr. 4, 1, 15 the favorable alternative is introduced by *ἢν μέν*, the unfavorable by *εἰ δέ* with future indicative. In DEM. 27, 20. 21. 22, *ἐὰν μὲν φῇ—εἰ δ' αὖ φήσει—εἰ δ' αὖ ἐρεῖ*, we may recognize an increasing hardness of tone, although that is not to be insisted on. A clear case is the one to which Professor Price, of the University of Virginia, has called my attention in DEM. 8, 17: *ἂν μὲν τοίνυν ᾖ τὸ συνεστηκὸς τοῦτο στράτευμα* (favorable), *εἰ δ' ἅπαξ διαλυθήσεται* (unfavorable); and yet others occur in ISOCR. 6, 107; 12, 237; 15, 130; LYS. 27, 7. In ISOCR. 17, 9 three courses are open, the worst being put in *εἰ* with future optative (for future indicative), the other two in optative (for subjunctive), the passage being in *oratia obliqua*. In HEROD. 6, 11, *ἢν μὲν βούλησθε—εἰ δὲ διαχρήσεσθε*, the same thing is found; but in 1, 71, *εἰ νικήσεις—ἢν νικηθῇς*, both alternatives are unfavorable, and in 3, 36, 3, *εἰ μὲν μεταμελήσει—ἢν δὲ μὴ μεταμέληται*, the usual practice seems to be reversed (as in HOM. Il. 1, 135. 137).

A striking example, which lies, however, beyond our present sphere of observation, is found in THEOCRIT. 7, 107. 109, in

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which deliverance is besought for Pan in one case (ἢν μὲν ταῦτ' ἔρδῃς), all manner of evil invoked in the other (εἰ δ' ἄλλως νεύσεις).

But while the antithesis of ἐὰν μέν with the subjunctive and εἰ δέ with the future indicative is not common, the elliptical use of εἰ δὲ μή following ἐὰν μέν is one of the stock observations of the grammars. This want of correspondence in the antithesis may be accounted for by the phraseological use of εἰ δὲ μή, 'otherwise,' *sin minus*, as one of the numerous unconscious ellipses of which every language is full; but it is worth noting that εἰ δὲ μή gives not only the negative but, as a rule, the unfavorable alternative also (e. g. Eur. Iph. Aul. 916. 917; Med. 241. 243), and this minatory and monitory character would suggest the distinct ellipsis of the future indicative, unless indeed εἰ with the subjunctive may be considered an equivalent.

A striking contrast to the usage of the tragic poets is presented by Aristophanes. In his future conditionals, little more than twenty per cent. take the form εἰ with the future indicative; and although the count was a rough one, owing to want of time for a careful exploration, the result is sufficiently accurate to establish the peculiar character of this form. Of sixty-seven such conditionals that I have examined, fifty-three are minatory or unfavorable; and it is no stretch of fancy to see in the others a certain mock-seriousness, which is one of the charms of Aristophanes.

I had hoped to find time for the collection of statistics as to the usage of Attic prose-writers; but as the fact of the preponderance of the ἐάν conditional is sufficiently familiar, I have been content to limit myself for the present to two or three specimens.

And first of Thucydides. As every one knows, the usage of this remarkable writer is highly individual and by no means characteristic of normal Greek prose. No wonder, however, that in the greatest tragedy of Greek history, he should have approached the standard of tragic expression and exhibited proportions which are closely those of Euripides.

The speeches in Thucydides show an unusually large proportion of Logical future conditionals. Of his future condi-

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tionals nearly forty-five per cent. take the logical form, and of these logical futures only five out of fifty-two do not distinctly indicate an unfavorable hypothesis, or, in other words, are not distinctly "monitory and minatory." This minatory character makes the natural tonelessness of the other form appear favorable by comparison, and in fact we find that in the majority of ἐάν conditionals the more favorable side is assumed. Comp. 5, 111, 2: εἰ μὴ σωφρονέστερον γνώσεσθε with § 4: ἢν εὖ βουλεύησθε; and the tone of the angry speeches of the Corcyraeans and Corinthians (1, 32–43) and of the warning address of Archidamus (1, 80 foll.) with the *paraeneses* of Hermocrates (6, 33 foll.) and of Nicias (6, 61).

In twelve orations of Lysias—the first thirteen, omitting the *Epitaphios*—I find twenty-one sentences with εἰ and future indicative against twenty-four with ἐάν. To the latter may be added, however, seven optatives in *oratio obliqua*, which are clearly transferred from ἐάν with the subjunctive. Here too, as in Thucydides, we note a much larger proportion of the sterner forms than is usual in prose, not a much larger proportion than might have been expected from the incisive genius of Lysias. All the examples may be referred to the classes mentioned. The conditions are either minatory and monitory, depend on verbs or phrases of emotion, or involve a modal use of the future. (See 3, 37. 43; 6, 13; 7, 41; 8, 1; 10, 10. 22 (*bis*); 12, 11. 29. 35. 70. 83. 85. 88. 90 (*bis*); 13, 15 (*bis*); 13, 93. 94.)

In Isocrates, I have selected three discourses—the *Philippus*, the *Archidamus*, and the *De Permutatione*. In the courtly *Philippus* I have noted thirty-two particular anticipatory conditionals and only two in εἰ with the future indicative—one referring to Isocrates himself (§ 82), the other to the Greeks generally (§ 121)—one unfavorable, the other monitory. In the martial *Archidamus* there are seven logical futures against twelve anticipatory conditions, and all of them fall under one or other of the categories mentioned (§§ 2. 10. 13. 28. 40. 56. 107)—the last one (§ 107) giving the unfavorable antithesis of a condition with ἤν. In the *De Permutatione*, I have counted some twenty-four anticipatory conditionals and only



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four logical futures (§§ 130. 152. 243. 272). In § 130 we have another antithesis between the two forms—the less favorable preceding with εἰ μέν, the more favorable following with ἤν δέ.

In Demosthenes *De Corona* εἰ with future indicative occurs only six times (§§ 7. 63. 114. 160. 205. 207), every time with stern, impressive tone; and in the oration against Meidias about twenty six per cent. of the future conditionals assume this form, all twelve examples noted (§§ 57. 100. 106. 109. 118. 131. 160. 204. 206. 212. 221. 222) being monitory or minatory, emotional or semi-causal.

In the speeches of Xenophon's Anabasis the proportion seems to be about the same as in Aristophanes. Worthy of note here also is the largely minatory character of εἰ with future indicative. Of thirty such conditionals that I have examined, all but four are distinctly minatory and the others are to be referred to the category of ἔεῖ or μέλλω.

In comparing the frequency of the ἐάν conditional with the frequency of the future logical, it may be well to take into consideration the abridged conditional sentence or the participial expressions of conditional relations. All conditional participles which are connected with future tenses must be resolved into ἐάν with subjunctive: λαμβάνων = ἐὰν λαμβάνω -ῃς -ῃ, λαβών = ἐὰν λάβω -ῃς -ῃ. There is no participial expression for the stern future indicative. The tone is lost in the emphasis of the temporal relation.

I pass now to an enumeration of the forms in the Tragic Poets. In the first column are given the occurrences of εἰ with the future indicative, in the second those of ἐάν with the subjunctive.

AESCHYLUS.*

PROMETHEUS VINCTUS.

| | |
|---|---|
| (1). 311 εἰ...ῥίψεις (minatory). | (1). 326 πειράσομαι, ἐὰν δύνωμαι (tentative, i. e. semi-interrogative). |
| | (2). 1014 ἐὰν μὴ πεισθῇς. |

SEPTEM CONTRA THEBAS.

| | |
|---|---|
| (1). 196 εἰ μή τις ἀκούσεται (minatory). | (1). 242 ἐὰν πύθησθε (may be considered generic). |
| (2). 618 εἰ καρπὸς ἔσται, 'is to be,' (certainty). | (2). 1027 ἢν μή τις...θέλῃ. |

* The text followed is that of Dindorf.

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PERSAE.

(1). 357 εἰ . . . ἵξεται κνέφας (certainty).
(2). 369 εἰ . . . φευξοίαθ' Ἕλληνες—fut. opt. in *orat. obl.* for fut. ind. (minatory).

(1). 529 ἐάν περ μόλῃ.

SUPPLICES.

(1). 461 εἰ μή . . . ὑποστήσει (minatory).
(2). 472 εἰ . . . μὴ . . . ἐκπράξω (minatory).
(3). 474 εἰ . . . ἥξω (minatory).
(4). 511 εἰ (supply ἐκδώσεις).
(5). 902 εἰ μή τις . . . εἶσιν (=fut.) (minatory).
(6). 924 εἴ τις . . . μὴ 'ξαιρήσεται (minatory).

AGAMEMNON.

(1). 207 εἰ δαίξω, 'am to,' 'must.'
(2). 1059 εἴ τι δράσεις, 'intend to,' (minatory).
(3). 1249 εἴπερ ἔσται γ', 'is to be.'
(4). 1338 εἰ ἀποτίσει, 'is to.'

(1). 1424 ἐὰν . . . κραίνῃ θεός.
(2). 1667 ἐὰν δαίμων . . . ἀπενθύνῃ.

CHOËPHORI.

(1). 182 εἰ . . . μήποτε ψαύσει, 'is to.'
(2). 273 εἰ μὴ μέτειμι, 'am not to.'
(3). 571 εἰ . . . ἀμείψω (minatory).
(4). 683 εἰ . . . νικήσει (cold indifference).
(5). 775 εἰ Ζεὺς θήσει (confidence).

EUMENIDES.

(1). 491 εἰ κρατήσει, 'is to.'
(2). 597 εἰ σε μάρψει (minatory).

(1). 741 κἂν ἰσόψηφος κριθῇ.

SUMMARY.

| | εἰ with fut. ind. | ἐάν with subj. |
|---|---|---|
| Prometheus Vinctus, | 1 | 2 |
| Septem contra Thebas, | 2 | 2 |
| Persae, | 2 | 1 |
| Supplices, | 6 | 0 |
| Agamemnon, | 4 | 2 |
| Choëphori, | 5 | 0 |
| Eumenides, | 2 | 1 |
| | 22 | 8 |
| | 73.33 per cent. | 26.67 per cent. |

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## SOPHOCLES.

### AJAX.

(1). 313 εἰ μὴ φανοίην (= fut. ind.) (minatory).
(2). 510 εἰ... διοίσεται, 'is to,' w. οἴκτειρε (verb of emotion).
(3). 1155 εἰ ποιήσεις (minatory).
(4). 1241 εἰ φανοίμεθα, 'are to.'
(5). 1248 εἰ ἐξωθήσομεν, 'are to.'
(6). 1256 εἰ μὴ κατακτήσει (minatory).
(7). 1308 εἰ βαλεῖτε, 'undertake to,' (minatory).
(8). 1314 εἰ πημανεῖς (as above).

(1). 528 ἐὰν... τολμᾷ.
(2). 688 ἢν μόλῃ.
(3). 1068 κἂν μὴ θέλῃς.
(4). 1184 κἂν μηδεὶς ἐᾷ.

### ELECTRA.

(1). 245 εἰ... κείσεται, 'is to.'
(2). 376 εἰ... λέξεις, 'have to say.'
(3). 379 εἰ μὴ λήξεις (minatory).
(4). 430 εἰ γάρ μ' ἀπώσει (minatory).
(5). 465 εἰ σωφρονήσεις (stern warning).
(6). 470 εἰ πείσεται (as above).
(7). 503 εἰ μὴ... εὖ κατασχήσει (conviction).
(8). 582 εἰ γὰρ κτενοίμεν, 'are to.'
(9). 832 εἰ... ὑποίσεις (remonstrance).
(10). 1004 εἰ τις... ἀκούσεται (stern warning).
(11). 1044 εἰ ποιήσεις (as above).
(12). 1210 εἰ στερήσομαι, 'am to be' (emotion).
(13). 1369 εἰ δ' ἐοέξετον (stern warning).

(1). 554 ἢν ἐφῇς μοι.
(2). 593 ἐάν περ καὶ λέγῃς.
(3). 821 χάρις μέν, ἢν κτάνῃ.
(4). 822 λίπῃ δ', ἐὰν ζῶ.
(5). 938 ἐὰν δέ μοι πίθῃ.
(6). 967 ἀλλ' ἢν ἐπίσπῃ.
(7). 1053 οὐδ' ἢν σφόδρ' ἱμείρουσα τυγχάνῃς.

### OEDIPUS REX.

(1). 54 εἴπερ ἀρξεις, 'are to.'
(2). 233 εἰ σιωπήσεσθε (minatory).
(3). 521 εἰ... κεκλήσομαι, 'am to' (=fut.).
(4). 586 εἰ ἕξει, 'is to.'
(5). 620 εἰ... προσμενῶ, 'am to.'
(6). 666 εἰ... προσάψει, 'are to.'
(7). 702 εἰ... ἐρεῖς, 'art going to.'
(8). 843 εἰ μὲν... λέξει.
(9). 846 εἰ δ'... αὐδήσει (dread alternative).
(10). 889 εἰ μὴ... κερδανεῖ... ἐρεῖται, (stern prophetic tone).
(11). 901 εἰ μὴ... ἁρμόσει (as above).
(12). 1166 εἰ ἐρήσομαι (minatory).

(1). 216 ἐὰν θέλῃς.
(2). 321 ἢν ἐμοὶ πίθῃ.
(3). 341 κἂν ἐγὼ σιγῇ στέγω.
(4). 461 κἂν λάβῃς.
(5). 605 ἐάν με... λάβῃς.
(6). 748 ἢν ἐν ἐξείπῃς ἔτι.
(7). 839 ἢν... εὑρεθῇ.
(8). 1062 οὐδ' ἐὰν... φανῶ.
(9). 1159 ἢν φράσω, διόλλυμαι.

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ŒDIPUS COLONEUS.

(1). 628 *εἴπερ μὴ ψεύσουσι* (confident expectation).
(2). 648 *εἰ σοί γ'...ἐμμενεῖ* 'is to.'
(3). 827 *εἰ μὴ πορεύσεται* (minatory).
(4). 837 *εἴ τι πημανεῖς* (minatory).
(5). 1181 *κεἰ παραινέσω* (=*μέλλω π.*).
(6). 1340 *εἰ σὺ...ξυμπαραστήσει* (confident expectation).
(7). 1435 *εἰ τελεῖτέ μοι* (as above).

[NOTE.—166 *εἴ τιν' οἴσεις* I omit as merely an old conjecture.]

(1). 457 *ἐὰν θέλητε.*
(2). 505 *ἢν...σπάνιν ἰσχῃς.*
(3). 814 *ἢν σ' ἔλω ποτέ* (minatory).
(4). 862 *ἢν μὴ...ἀπειργάθῃ.*
(5). 1040 *ἢν μὴ θάνω.*
(6). 1197 *κἂν κεῖνα λεύσσῃς.*
(7). 1210 *ἐάνπερ σώζῃ.*
(8). 1407 *ἐὰν αἱ τοῦδ' ἀραὶ τελῶνται.*
(9). 1443 *δυστάλαινα...ἐγώ, εἴ σου στερηθῶ* (see above, page 8).
(10). 1770 *ἐάν πως διακωλύσωμεν.*

ANTIGONE.

(1). 59 *εἰ...παρέξομεν*, 'are to.'
(2). 90 *εἰ καὶ ὀυνήσει.*
(3). 93 *εἰ ταῦτα λέξεις* (minatory).
(4). 229 *κεἰ ταδ' εἴσεται* (minatory).
(5). 234 *κεἰ τὸ μηδὲν ἐξερῶ.*
(6). 307 *εἰ μὴ...ἐκφανεῖτε* (minatory).
(7). 324 *εἰ δὲ ταῦτα μὴ φανεῖτε* (minatory).
(8). 414 *εἴ τις ἀφειδήσοι* (= ind. in *or. rect.*) (minatory).
(9). 462 *εἰ...θανοῦμαι*, 'must die.'
(10). 485 *εἰ...κείσεται*, 'are to.'
(11). 659 *εἰ...θρέψω.*

(1) 45 *ἢν σὺ μὴ θέλῃς.*
(2). 87 *ἐὰν μὴ κηρύξῃς.*
(3). 327 *ἐὰν δέ τοι ληφθῇ τε καὶ μή.*

TRACHINIAE.

(1). 305 *εἴ τι δράσεις* 'dost intend to.'
(2). 666 *εἰ φανήσομαι* (after verb of emotion: semi-causal).
(3). 712 *εἴ τι μὴ ψευσθήσομαι.*
(4). 719 *εἰ σφαλήσεται.*
(5). 732 *εἰ μή τι λέξεις* (Herm. *οὐ μή τι λ.*).
(6). 1113 *εἰ σφαλήσεται* (expression of emotion).
(7). 1246 *εἰ τέρψεις* 'art going to.'

(1). 411 *ἢν εὑρεθῇς.*
(2). 570 *ἐὰν πίθῃ.*
(3). 572 *ἐὰν...ἐνέγκῃ.*
(4). 584 *ἐάν πως ὑπερβαλώμεθα.*
(5). 597 *κἂν αἰσχρὰ πράσσῃς.*
(6). 672 *ἢν φράσω.*
(7). 1107 *κἂν τὸ μηδὲν ὦ.*

PHILOCTETES.

(1). 66 *εἰ δ' ἐργάσει* (stern).
(2). 68 *εἰ...μὴ ληφθήσεται* (as above).
(3). 75 *εἴ μέ...αἰσθήσεται.*
(4). 353 *εἰ...αἱρήσοιμι* (fut. opt. = fut. ind. *εἰ* almost=*ὅτι*: semi-causal).
(5). 376 *εἰ...ἀφαιρήσοιτο* (as above).
(6). 988 *εἰ...ἀπάξεται*, 'is to.'

(1). 52 *ἢν τι...κλύῃς.*
(2). 126 *ἐὰν δοκῆτε*
(3). 479 *ἐὰν μόλω ζῶν.*
(4). 769 *ἢν μόλωσι.*
(5). 817 *ἢν προσθίγῃς.*
(6). 982 *οὐδ' ἢν δράσῃ*
(7.) 985 *ἢν μὴ ἕρπῃς.*



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PHILOCTETES.

(7). 1198 οὐδ' εἰ...εἶσι (an extravagant supposition which seems natural to a man in a frenzied state).
(8). 1393 εἰ...δυνησόμεθα, 'are to.'
(8). 999 οὐδ' ἢν χρῇ.
(9). 1259 κἀν...ῥρουῇς.
(10). 1298 ἐὰν...θέλῃ.
(11). 1299 ἢν τοῦτ' ὀρθωθῇ.
(12). 1342 ἢν...ψευσθῇ.
(13). 1405 ἐὰν πορθῶσι.
To which add opt. for subj. in *or. obl.*
(14). 613 εἰ ἀγοιντο.
(15). 618 εἰ μὴ θέλοι.

SUMMARY.

| | εἰ with fut. ind. | ἐάν with subj. |
|---|---|---|
| Ajax, | 8 | 4 |
| Electra, | 13 | 7 |
| Oedipus Rex, | 12 | 9 |
| Oedipus Coloneus, | 8 | 10 |
| Antigone, | 11 | 3 |
| Trachiniae, | 7 | 7 |
| Philoctetes, | 8 | 15 |
| | 67 | 55 |
| | 54 92 per cent. | 45.08 per cent. |

## EURIPIDES.

ALCESTIS.

(1). 386 εἰ με δὴ λήψεις.
(2). 700 εἰ τὴν γυναῖκα...πείσεις.
(3). 704 εἰ δ' ἡμᾶς...ἐρεῖς (minatory).
(4). 733 εἰ μὴ...τιμωρήσεται.
(1). 56 κἀν γραῦς ὀλῆται.
(2). 846 κἀνπερ μάρψω.
(3). 850 ἢν δ' οὖν ἁμάρτω.

ANDROMACHE.

(1.) 61 εἰ τις...αἰσθήσεται.
(2). 254 εἰ μὴ...θανοῦμαί γε.
(3). 314 κεἰ μὴ...ἐρημώσεις (minatory).
(4). 378 εἰ μὴ θήσομαι.
(5). 409 εἰ σωθήσεται.
(6). 708 εἰ μὴ φθερεῖ (minatory).
(7). 925 εἰ δ' ἥξει πάρος.
(8). 970 εἰ πέρσει πόλιν.
(9). 1282 εἰ...οἰσεται (=μέλλει οἴσεσθαι).
(1). 44 ἢ με κωλύσῃ θανεῖν.
(2). 90 ἢν τι καὶ πάθω.
(3). 163 ἢν...θέλῃ.
(4). 188 ἢν...κρατήσω.
(5). 202 ἢν σὺ μὴ τέκῃς.
(6.) 338 ἢν...ὑπεκδράμω.
(7.) 381 ἢν...θάνῃς.
(8). 414 ἢν ὑπεκδράμῃς μόρον.
(9). 432 ἢν . θέλῃ.
(10). 442 ἢν...θέλῃ.
(11). 663 ἢν...μή τέκῃ.
(12.) 689 ἢν δ' ὀξυθυμῇς.
(13). 740 κἀν...κολάζῃ.
(14). 999 ἢν...μείνωσιν.

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BACCHAE.

(1). 239 εἰ...λήψομαι (minatory).
(2). 786 εἴπερ πεισόμεσθα, 'are to.'
(3) 1261 εἰ...μενεῖτε, 'are to.'

(1). 50 ἢν...ζητῇ.
(2). 311 ἢν δοκῇς.
(3). 355 κἀνπερ λάβητε.
(4). 640 κἀν...ἐλθῃ.
(5). 817 κἀν ἔλθῃς λάθρα.
(6). 823 ἢν ἀνὴρ ὀφθῇς.
(7) 960 ἢν σὺ μὴ ληφθῇ πάρος.

HECUBA.

(1). 347 εἰ δὲ μὴ βουλήσομαι.
(2). 802 εἰ...διαφθαρήσεται.
(3). 863 εἰ διαβληθήσομαι, 'am to.'
(4). 1233 εἰ τῷδ' ἀρκέσεις, κακὸς φανεῖ.

(1). 293 κἀν κακῶς λέγῃ.
(2). 313 ἢν τις αὐ φανῇ.
(3). 399 οὐκ, ἢν γε πείθῃ.
(4). 731 κἀν τύχω κἀν μὴ τύχω.
(5). 870 ἢν τι βουλεύσω.
(6). 872 ἢν...φανῇ τις.
(7). 1006 ἢν σὺ κατθάνῃς.

HELENA.

(1). 919 εἰ διαφθερεῖς, 'art going to.'
(2). 975 εἰ...συλήσετε (minatory).
(3). 1011 εἰ μὴ ἀποδώσω.
(4). 1051 εἰ δὲ κερδανῶ.
(5). 1155 εἰ κρινεῖ.
(6). 1631 εἰ μη μ' ἐάσεις (minatory).

(1). 429 ἢν πως (semi-interrogative).
(2). 479 ἢν...λάβῃ.
(3). 506 ἢν...ὠμόφρων ᾖ.
(4). 508 ἢν...ἐνδιδῶ.
(5). 743 ἢν δυνώμεθα.
(6). 832 ἢν...μὴ ἀποδέξηται.
(7). 929 ἢν...δ' Ἑλλάδ' ἔλθω.
(8). 980 ἢν...μὴ πόδ' ἀντιθῇ.
(9) 1049 ἢν...λέξῃ.
(10). 1071 ἐάνπερ...λάβω.
(11). 1085 ἢν...δρᾷ.
(12). 1091 ἢν ἁλῶ.
(13). 1176 ἢν γε δὴ ληφθῇ μόνον.
(14). 1291 ἢν...ἔλθω
(15). 1292 ἢν γυνὴ γένῃ.
(16). 1388 ἢν δυνώμεθα.
(17). 1392 ἢν...δόξω.
(18). 1394 ἢν...μὴ παρῇς.
(19). 1425 ἢν διδῷς
(20). 1444 κἀν . . . θίγῃς.
(21). 1637 ἢν λέγω

ELECTRA.

(1). 48 στένω...εἰ εἰσόψεται (emotional).
(2). 261–3 ταρβῶν—εἰ δή ποτ' ἥξει.
(3). 336 αἰσχρόν, εἰ...μὲν ἐξεῖλεν...ὁ δ' οὐ δυνήσεται.
(4). 584 εἰ...ἔσται, 'is to be.'
(5). 686 εἰ...πεσεῖ (solemn warning).
(6). 1093 εἰ δ' ἀμείψεται, 'is to.'

(1). 110 ἢν τι δεξώμεσθα (semi-interrogative).
(2). 274 ἢν ιόλῃ.
(3). 582 ἢν δ' ἀσπάσωμαι.
(4). 638 ἢν θεὸς θέλῃ.
(5). 690 ἢν μὲν ἔλθῃ.

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HERACLIDAE.

(1). 165–8 εἰ...ἐμβήσει (stern warning).
(2). 197 εἰ...ἔσται, 'is to be.'
(3). 243 εἰ...παρήσω.
(4). 272 εἰ μὴ...μαθήσεται (minatory).
(5). 373 εἰ...ἡξεις (minatory).
(6). 418 εἰ δὲ δὴ δράσω.
(7). 444 μέλει εἰ...τέρψω.
(8). 495 εἰ μή τι ... ἐξαμηχανήσομεν (minatory).
(9). 593 εἰ γὰρ ἐξομεν.
(10). 652 εἰ...προσθίξει (minatory).
(11). 739 εἰ δή ποθ' ἡξομέν γε· τοῦτο γὰρ φόβος.
(12). 763 εἰ...παραδώσομεν, 'are to.'
(13). 974 εἰ δράσεις τόδε (minatory).

(1). 47 ἢν...ἀπωθώμεσθα.
(2). 67 κἂν σὺ μὴ θέλῃς.
(3). 158 ἢν πεπανθῇς.
(4). 253 ἢν δίκαιον ᾖ τι.
(5). 256 ἐὰν...μεθῶ.
(6). 263 ἢν τι σωφρον ᾖς.
(7). 310 ἢν...φανῇ.
(8). 342 κἂν θυραῖος ὦ.
(9). 424 ἢν δίκαια δρῶ.
(10). 586 κἂν εὑρεθῇ.
(11). 712 ἢν θάνῃς σύ.
(12). 714 ἢν...χρήσωνται.
(13). 1020 ἢν θάνῃ.

HERCULES FURENS.

(1). 210 εἰ...θανούμεθα, 'are to.'
(2). 490 εἴ τις...εἰσακούσεται.
(3). 1074 εἰ με κανεῖ (minatory)
(4). 1412 εἰ σ' ὄψεται (minatory).

(1). 279 ἢν...δοκῶ, 'in case' (semi-int.).
(2). 848 ἢν πίθησθε.
(3). 1282 ἢν δὲ καὶ μένω.

SUPPLICES.

(1). 521 εἰ 'πιταξόμεσθα δή.
(2). 539 εἰ...τις ἕξει, 'is to.'
(3). 543 ταρβεῖτ' εἰ κρυβήσονται χθονί.
(4). 603 εἰ...φανήσονται.
(5). 711 εἰ μὴ σχήσετε (solemn warning).
(6). 784 εἴ περ ὄψομαι, 'am to.'

(1). 388 ἂν θέλωσι.
(2). 389 ἢν δ' ἀπιστῶσι.
(3). 397 ἢν σ' ἀπαλλάξῃ, 'in case.'
(4). 473 ἂν... πίθῃ μοι.
(5). 499 ἢν τε μὴ θέλῃ.
(6). 541 ἢν τεθῇ νόμος.
(7). 1194 ἢν...ἔλθωσιν.
(8). 1208 ἢν...ἔλθωσιν.

HIPPOLYTUS.

(1). 305 εἰ θανεῖ (stern).
(2). 327 εἰ πείσει (minatory).
(3) 461 εἰ μὴ στέρξεις.
(4). 481 εἰ μὴ εὑρήσομεν, 'are not to.'
(5). 501 εἴπερ ἐκσώσει γε, 'is going to.'
(6). 697 εἰ δέξει, 'wilt consent to.'
(7). 938 εἰ...ἐξογκώσεται, 'is going to.'
(8). 976 εἰ...ἡσσηθήσομαι, 'am to.'
(9). 1088 εἰ μὴ πείσει (minatory).

(1). 505 ἢν λέγῃς καλῶς.
(2) 512 ἢν σὺ μὴ γένῃ κακή.
(3). 995 οὐδ' ἢν σὺ μὴ φῄς.

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IPHIGENIA AULIDENSIS.

(1). 103 εἰ μὴ...εἶσιν.
(2). 296 εἰ προσαρμόσει.
(3). 654 εἰ σέ γ' εὐφρανῶ.
(4). 817 εἴ τι δράσεις.
(5). 941 εἰ δι' ἐμ' ὀλεῖται, 'is to.'
(6). 972 εἴ τις...ἐξαιρήσεται, 'undertakes to' (minatory).
(7). 1171 εἰ στρατεύσει (Elmsl.)
(8). 1190 εἰ...εὖ φρονήσομεν.
(9). 1262 εἰ μή σε θύσω.
(10). 1268 θέσφατ' εἰ λύσω θεᾶς.
(11). 1415 ἀχθομαι...εἰ μή σε σώσω.

(1). 32 κἂν μὴ σὺ θέλῃς.
(2). 150 ἢν...ἀντήσῃς.
(3). 515 ἢν...ἀποστείλῃς.
(4). 519 ἢν σανῇ.
(5). 533 κἂν...ἐκφύγω.
(6). 915 ἢν τολμήσῃς.
(7). 1007 ἢν σώσω.
(8). 1016 ἢν δ' ἀντιβαίνῃ.
(9). 1025 ἢν...μὴ πράσσωμεν.
(10). 1134 ἢν γ' ἐρωτᾷς.
(11). 1166 κἂν τίς σ' ἐρηται.
(12, 13). 1271 κἂν θέλω κἂν μὴ θέλω.
(14). 1344 ἢν δυνώμεθα.
(15). 1421 ἢν δυνώμεθα.
(16) 1541 ἢν ταράξῃ.

IPHIGENIA TAURICA.

(1). 690 εἰ σε...κτενῶ.
(2) 975 εἰ μή με σώσει Φοῖβος.
(3). 986 εἰ μὴ ληψόμεσθα.
(4). 999 εἰ... γενήσεται.
(5). 1022 εἰ σὲ σώσει, 'is to.'
(6). 1034 εἰ κερδανεῖς, 'expectest to.'
(7). 1412 εἰ μὴ...γενήσεται.

(1). 100 ἢν...ληφθῶμεν.
(2). 337 ἂν ἀναλίσκῃς.
(3). 754 ἢν καλῶς ἔχῃ.
(4). 755 ἢν τι ναῦς πάθῃ.
(5). 762 ἢν ἐκσώσῃς.
(6). 764 ἢν...ἀφανισθῇ.
(7). 980 ἢν...κατάσχωμεν.
(8). 1010 ἢνπερ μὴ...πέσω.
(9). 1219 ἢν...δοκῶ χρονίζειν.
(10). 1230 ἢν νίψω φόνον.

ION.

(1). 168 εἰ μὴ πείσει (minatory).
(2). 254 εἰ...ὀλούμεθα.
(3). 375 εἰ...ἐκπονήσομεν.
(4). 445 εἰ δ', οὐ γὰρ ἔσται, τῷ λογῳ δὲ χρήσομαι...δώσετε.
(5). 648 εἴπερ...εὐτυχήσουσιν.
(6). 669 εἰ μὴ...εὑρήσω.
(7). 750 εἰ μηνύσετε.
(8). 847 εἰ ὑφήσεις...ἀπαλλάξει βίου.
(9). 1024 κεἰ μὴ κτενεῖς.
(10). 1062 εἰ καιρὸς ἀπεισι.
(11). 1075 αἰσχύνομαι εἰ...δψεται.

(1). 425 ἐὰν θέλῃ.
(2) 527 ἢν κτάνῃς.
(3). 595 ἢν...ζητῶ.
(4). 1037 κἀνπερ διέλθῃ.
(5). 1130 ἢν...μένω.
(6). 1259 ἢν θάνῃς.
(7). 1309 ἢν...θέλῃς.
(8). 1415 κἂν μὴ φράσω γε.

CYCLOPS.

(1). 199 εἰ φευξόμεσθα, 'are to.'
(2). 307 εἰ...ἀναλώσεις (solemn remonstrance).
(3). 474 εἰ...ἐκθύψομεν.

(1). 217 ἢν θέλῃς.
(2). 314 ἢν... δάκῃς.
(3). 427 ἐὰν βούλῃ.
(4). 573 κἂν μὲν σπάσῃς.
(5). 575 ἢν δ' ἐκλίπῃς.



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MEDEA.

(1). 78 εἰ...προσοίσομεν, 'are to.'
(2). 346 εἰ φευξούμεθα.
(3). 352 εἰ...δψεται (minatory).
(4). 381 εἰ ληφθήσομαι.
(5). 512 εἰ φεύξομαι, 'am to.'
(6). 577 κεἰ...ἐρῶ, 'must.'
(7). 931 οἶκτος εἰ γενήσεται τάδε.
(8). 1249 εἰ κτενεῖς σφ' ὅμως.

NOTE.—184 φόβος εἰ πείσω is interrogative. 1109, 1298 are doubtful.

(1). 260 ἢν...ἐξευρεθῇ.
(2). 389 ἢν...φανῇ.
(3). 391 ἢν...ἐξελαύνῃ.
(4). 727 ἐάνπερ...ἐλθῃς.
(5). 787 κἀνπερ...ἀμφιθῇ.
(6). 1362 ἢν σὺ μὴ' γγελᾷς.

ORESTES.

(1). 157 ὀλεῖς, εἰ βλέφαρα κινήσεις (warning).
(2). 239 εἰ μὲν εὖ (λέξεις).
(3). 240 εἰ δ' εἰς βλάβην τιν'.
(4). 272 εἰ μὴ' ξαμείψει (minatory).
(5). 304 εἰ γὰρ προλείψεις.
(6). 509 εἰ...ἀνταποκτενεῖ (?).
(7). 511 —— λύσει (?).
(8). 566 εἰ...ἡξουσιν.
(9). 599 εἰ μὴ...ῥύσεται.
(10). 803 εἰ σε μὴ...ἐπαρκέσω, 'am to.'
(11). 935 εἰ...ἔσται, 'is to.'
(12). 940 εἰ δὲ δὴ κατακτενεῖτέ με.
(13). 1102 εἰ τιμωρήσομαι.
(14). 1106 εἰ γ' ἔσται καλῶς.
(15). 1212 εἴπερ εὐτυχήσομεν.
(16). 1533 εἰ...ἐπάξει.

(1). 69 ἢν...μὴ...σωθῶμεν.
(2). 308 ἢν σὺ κατθανῇς.
(3). 559 ἢν λέγω.
(4). 644 ἢν...σώσῃς.
(5). 778 ἢν μένῃς.
(6). 1148 ἢν μὴ σπασώμεθα (Dind. εἰ μὴ σπάσω μέλαν).
(7). 1149 ἢν δ' οὖν μὴ κατάσχωμεν.
(8). 1191 ἢν τι δρᾷ.
(9). 1195 κἂν μέν σε σώζῃ.
(10). 1198 ἢν δὲ...κτείνῃ σε.
(11). 1200 ἢν πολὺς παρῇ.
(12). 1218 φύλασσε ἢν τις φθῇ.
(13). 1316 ἢν ἁλῷ.
(14). 1332 ἢν σὺ μὴ λέγῃς.
(15). 1593 ἢν γε μὴ φύγῃς πτεροῖς.

RHESUS.

(1). 113 εἰ μὴ κυρήσεις (warning).
(2). 169 εἰ τόδ' αἰτήσει γέρας.
(3). 600 εἰ διοίσει (warning).

(1). 118 ἢν ἄρα (verb implied).
(2). 126 κἂν μὲν αἵρωνται.
(3). 141 κἂν μὲν πυθώμεθα.
(4). 143 ἐὰν δ' ἀπαίρωσι.
(5). 521 ἢν τι καὶ δέῃ.
(6). 572 ἢν δ' οὖν ἐγείρῃς.
(7). 880 ἢν...πίθῃ.

TROADES.

(1). 662 εἰ...ἀναπτύξω.
(2). 735 εἰ γάρ τι λέξεις (solemn warning).
(3). 890 αἰνῶ σε εἰ κτενεῖς (verb of emotion: semi-causal)

NOTE.—1249 εἰ...τείξεται may be considered interrogative.

(1). 355 κἂν μὴ...ᾖ.
(2). 701 κἂν δρᾷς τάδε.
(3). 718 ἐὰν λέγῃς.
(4). 914 ἢν θάνω.
(5). 904 κἂν εὖ...
(6). 914 κἂν κακῶς δόξω.
(7). 1059 κἂν ἔτ' ὦσ' αἰσχίονες.

Add

(8). 874 }
(9). 928 } opt. for subj. in *orat. obl.*
(10). 930 }

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PHOENISSAE.

(1). 19 εἰ γὰρ τεκνώσεις (minatory).
(2). 244 εἰ τι πείσεται.
(3). 725 εἴπερ...σωθήσει.
(4). 885 εἰ μὴ λόγοις τις...πείσεται (minatory).
(5). 1621 εἰ με...βαλεῖς.

(1). 559 ἢν δ' ἔρωμαι.
(2). 561 ἢν...νικήσῃ.
(3). 571 ἢν ἕλῃς.
(4). 578 ἢν δ' αὖ κρατηθῇς.
(5). 629 κἂν τι γένηται.
(6). 757 ἐάν τι...σφαλῶ.
(7). 765 ἢν τύχῃ.
(8). 775 ἤνπερ κρατήσῃ.
(9). 777 κἂν φίλων τις ᾖ.
(10). 938 ἢν λάβῃ.
(11). 975 κἂν μὲν φθάσωμεν.
(12). 976 ἢν δ' ὑστερήσῃς.
(13). 1216 ἢν μή γε...ἐκφύγῃς.
(14). 1231 κἂν μὲν κτάνω.
(15). 1280 ἢν μὲν φθάσω.
(16). 1657 κἂν ἀπεννέπῃ.

SUMMARY.

| | εἰ with fut. ind. | ἐάν with subj. |
|---|---|---|
| Alcestis, | 4 | 3 |
| Andromache, | 9 | 14 |
| Bacchae, | 3 | 7 |
| Hecuba, | 4 | 7 |
| Helena, | 6 | 21 |
| Electra, | 6 | 5 |
| Heraclidae, | 13 | 13 |
| Hercules Furens, | 4 | 3 |
| Supplices, | 6 | 8 |
| Hippolytus, | 9 | 3 |
| Iphigenia Aulidensis, | 11 | 16 |
| Iphigenia Taurica, | 7 | 10 |
| Ion, | 11 | 8 |
| Cyclops, | 3 | 5 |
| Medea, | 8 | 6 |
| Orestes, | 16 | 15 |
| Rhesus, | 3 | 7 |
| Troades, | 3 | 10 |
| Phoenissae, | 5 | 16 |
| | 131 | 177 |
| | 42.53 per cent. | 57.47 per cent. |

NOTE.—At my request, two Fellows of the Johns Hopkins University, Messrs. A. D. Savage and J. H. Wheeler, undertook to revise the count, Mr. Savage reading half of Euripides, and Mr. Wheeler the other half and the whole of Sophocles. While the result has not been materially affected by the new count, I am glad to have the statistics brought nearer to absolute accuracy, and am happy to express my thanks to these scholars for their kind and intelligent assistance.

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## II.—*On Grote's Theory of the Structure of the Iliad.*

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Grote's theory of the structure of the Iliad is stated and defended in the second volume of his History of Greece, which was published in 1846. It was noticed in reviews of the new history in some English and American magazines. Most of these articles, I believe, I have seen, but I do not find in any of them a minute, careful examination of the new theory on a critical basis. The discussion is from a literary point of view and of a general character, containing little more than the expression of opinions. In Germany it seems to have been thought worthy of more thorough consideration. Several scholars whose names are familiar to students of Homer discussed it with different results. Friedländer published in 1853 a book in which he adopted and defended it. Düntzer in the *Jahrbücher für Philologie* (2d Supplementband, 1856) claimed to have already published nearly the same view, though he refused assent to the precise form which Grote proposed. Ribbeck attacked it from the Lachmann standpoint in *Philologus* (1853), aiming to show that Grote was wrong, not in throwing out what he did, but in regarding the rest as a self-consistent poem. Finally, W. Bäumlein in *Philologus* (1856) subjected the theory to critical examination from the conservative point of view, and concluded by rejecting it as merely one of many attempts, equally unsuccessful in his judgment, to break down the unity of the Iliad. Thus being approved by some and rejected on conflicting grounds by others, it seems still fairly open to discussion. It may indeed be thought that the long silence of English scholars on the subject is an argument for its own continuance, as showing that no one cares to have it interrupted, but, as the theory confronts every student of Homer or reader of Grote's history, it may be reasonable to suppose that some would be interested or profited by a discussion of it. I ought perhaps in the outset

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to say that I have no new theory of the Iliad to propose. My purpose is simply critical, or, if you please, destructive; it is to take up the statements and arguments of Grote and see if they bear examination. I do not mean to argue either for or against the unity of the poem. Into those larger questions of the authorship and integrity of the poem I do not enter. Of course I shall not knowingly rest any argument on a line or passage which is reasonably or generally suspected. Nor shall I try to evade an argument by suggesting new suspicion of the text. Here is the poem, which certainly received its present form from somebody, though we know not who it was or when he did his work. Our question may really be only as to the success or failure of a compiler or editor, but that is a fair subject for discussion in connection with a theory which aims to go behind such a man's work and show his materials uncombined; does the poem as it stands admit this analysis? In my examination of the subject I have made great use of Bäumlein's article above mentioned, and wish to acknowledge fully my debt to him. Part of the same material is also to be found in Bergk's *History of Greek Literature*, but in the general plan and many particulars I have followed Bäumlein.

Grote's theory may be stated best in his own words (*History of Greece*, II. p. 175 f. Am. edition):

"The first book, together with the eighth, and those from the eleventh to the twenty-second inclusive, seem to form the primary organization of the poem, then properly an Achilleis; the twenty-third and twenty-fourth books are perhaps additions at the tail of this primitive poem, which still leave it nothing more than an enlarged Achilleis. But the books from the second to the seventh inclusive, together with the tenth" (and here he might have added the ninth also), "are of a wider and more comprehensive character, and convert the poem from an Achilleis into an Iliad. The primitive frontispiece, inscribed with the anger of Achilles and its direct consequences, yet remains, after it has ceased to be co-extensive with the poem. The parts added, however, are not necessarily inferior in merit to the original poem: so far is this from being the case, that amongst them are com-

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prehended some of the noblest efforts of the Grecian Epic. Nor are they more recent in date than the original: strictly speaking, they must be a little more recent, but they belong to the same generation and state of society as the primitive Achilleis."

In support of this theory, the first statement made is that in the books of the original Achilleid "the sequence of events is more rapid, more unbroken, and more intimately knit together in the way of cause and effect" than in the others. If this difference is tested by the impression made upon a reader by the two parts of the poem, there would be room for difference of opinion. It is hardly possible to expect general agreement in such a matter. If it be tested by the number of incidents crowded into a day, the difference is not very great. Following Faesi's analysis, there are twenty-one days covered by the first book, the twenty-second day covers five books and nearly a sixth (ii. 1–vii. 380), the twenty-third and twenty-fourth days some fifty lines each, then the twenty-fifth day three books (viii.–x.), the twenty-sixth day eight books (xi.–xviii.), the twenty-seventh day four books and a few lines more (xix. 1–xxiii. 108), then two comparatively short or empty days, and some twenty in the last book. Of course these long periods in the first and last books are made up by groups of ten or twelve days which are mere intervals in which nothing is narrated. Throwing these out, the other single days cover from fifty to some five thousand five hundred lines each, and all differ in length. In this point of view not much distinction can be made between the parts of the poem which Grote regards so differently. But let us grant that there is a distinction of the kind he suggests; does it prove difference of origin or relation to the whole poem? Are not such differences in the rate of movement to be looked for in any poem on so large a scale? Look at the Odyssey. There the distribution of days is much more even—many of them covering about the length of an average book. Only the fifth book corresponds to the first and last of the Iliad in containing in its limits twenty-one days. But there are two pivots of the action, two important points, one the narrative

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of Odysseus's return told by himself, the other his arrival in his own house and vengeance upon the suitors. At each of these the time is crowded; the first, occupying one night, covers five books (viii.–xii.), and at the second the events of two days extend over seven books (xvi.–xxiii.). This parallel case, where Grote admits no division, illustrates the scheme of days in the Iliad. The introduction and the close cover between them forty-two days in two books, where long intervals, of little action for the purpose of the poem, are condensed into a few lines. Of the intervening eight days, four are briefly dispatched, but the other four, containing the struggles of the Greeks without Achilles, his return, and the killing of Hector, belong to the crises of the story and cover respectively three, five, six, and eight books. There is in this respect nothing true of the Iliad which is not equally true of the Odyssey, and nothing true of either which would not be equally true of any poem expanding a single selected point in a long story. As to the alleged impossibility of stopping anywhere between books eleven and twenty-two, so as to break apart the separate songs, that may trouble one of the Lachmann school but not one who discusses the poem as a whole. Mr. Grote goes on to say that there could never have been a separate poem called Patrokleia ending with the death of Patroklos, because he is so subordinate a character, "standing to Achilles in a relation of dependence, resembling that of Telemachus to Odysseus." The illustration is perhaps unfortunate, because recent investigators have regarded a Telemacheia as one of the constituents of the Odyssey. The remark itself, however, is open to question. What Patroklos did in his brief flash of action might as well be a distinct song, one would think, as the achievements of any hero in the long war. If we had not the Iliad and Odyssey, some critic might well suggest that no such poem as these could be constructed on the events of ten or twenty days in a war of ten years and a subsequent wandering of another ten.

The next general argument that Grote adduces is summed up in a few words on the next page (178) of the history, that "the consequences of the anger of Achilles do not appear

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until the eighth book." In other words, the poet, after concentrating our attention in the first book on that anger and the consequent promise of Zeus to Thetis, seems to lose entirely from his thought for the next six books the fulfilment of that promise. This remark seems to rest upon the idea that a purpose thus formed and expressed must be put into immediate, if only partial, execution. Such an idea holds good, one may say, as what ought to be in the sphere of morals; but does it apply to poetry? Is the method of art conformed to it? Is it not rather the artistic plan, adopted from the experience of life, to defer the fulfilment of a promise or a purpose, to interpose and remove obstacles one by one, to vary the success on both sides in a conflict, to make the finally defeated party prove itself hard to defeat? All these things contribute to the suspense and so to the interest in the hearer's or reader's mind, and seem essential to the building up of a story in due proportion. Nor does this degree of artifice belong to a more developed stage of art than that in which the Achilleid must have been composed. The poet who imagined that dispute between Achilles and Agamemnon in the first book, with its skilful delineation of the growing anger in the words of each from vague to precise threats, from sarcasm and taunt to defiance and denunciation, was certainly equal to such simple and natural development of the subsequent plot. A similar delay, too, occurs at the end of the Achilleid when Achilles declares (xviii. 114 f.) a purpose to go at once to find and slay Hector, but one thing after another comes in to put off the execution of that purpose, so that it does not come until late the next day, in the twenty-second book. This I mention only to show of what the poet's art was capable.

Again it may be remarked that such prompt fulfilment as Grote requires of the promise of Zeus is not in harmony with the character of the God and of his government as shown in any part of the Homeric poems. He does not in either poem work out promptly, firmly, and openly a plan which he has independently and wisely formed. He is not, in the Homeric conception, a being strong enough to have in himself the

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ultimate decision of questions on which the gods are divided. There is a fate dimly shadowed behind him, and when by the materialized device of a balance he learns what that fate is in a given case, he must execute it, though his heart is filled with pity for the losing side. Even then he acts irregularly and inconsistently, somewhat like the conventional oriental despot who gets things done somehow when he insists upon it, but has not the intellect or the energy to inspire obedience and secure efficient government. He alone knows certainly what the fate is, and with this knowledge and a consciousness of power, he lets the other gods fight among themselves, laughs at their wounds, and indolently allows them to outwit him and almost succeed in thwarting his plan, but always in the end has his way and executes the fate. Even in the Achilleid, after his promise to Thetis, a prayer from Agamemnon touches his pity and he sends (as again in xiii. 821 f.) a favorable prodigy which gives the Greeks a temporary success (viii. 246–252). Again, in book xiv. he lets himself be seduced by Hera into a sleep; during which Poseidon stimulates the Greeks so that they drive back the Trojans from the ships and Hector is carried off stunned and senseless. In the thirteenth book, too, still within the Achilleid, Poseidon without the knowledge of Zeus secures a success for the Greeks on the left wing, while Zeus himself inspires the Trojans in the centre of the line; but it is Hector without help from Zeus who finds out and in a measure repairs the injury caused by Poseidon. His favor varies from one side to the other even in book xvii., in the battle over the body of Patroklos. Grote himself notices the inconsistencies in the action of Zeus, but only at one point where they afford him an argument for his theory. He speaks (p. 189) of the attitude of Zeus at the beginning of the fourth book as irreconcilable with that in the first and eighth books, saying that he "discusses nothing but the question of continuance or termination of the war, and manifests anxiety only for the salvation of Troy," forgetting all about his promise to Thetis. He seems in this criticism to overlook entirely the words with which the first speech of Zeus in book iv. is introduced:



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αὐτίκ' ἐπειρᾶτο Κρονίδης ἰρεθίζεμεν Ἥρην
κερτομίοις ἐπέεσσι, παραβλήδην ἀγορεύων·

"Zeus at once attempted to exasperate Hera with taunting words and deceitful speech." This plainly means that the speech which follows is designedly insincere and may be expected to conflict with his words and deeds elsewhere. And when in the next speech he reproaches Hera with her hatred of Troy, but still, though unwillingly, consents to her desire to destroy it, there is nothing in that inconsistent with his promise to Thetis, for the fulfilment of that promise by bringing Achilles again into the field secured the victory of the Greeks. In thus requiring that Zeus should begin at once to make his taking the Trojan side perceptible, Grote seems then to require something not in keeping with the character of the god as conceived by the poet, nor yet with his action in the Achilleid itself.

Again, it is not easy to see what valid objection there is to the common conception of the Iliad, that it was in the intent of the poet, as far back as we can see, to bring out in the first half of the poem the inability of the other Greek heroes to make good the absence of Achilles. It is certainly worth observing how carefully this purpose seems to be carried out, in bringing forward each one of the principal heroes in turn to bear the burden of the fight. It may be said indeed that these separate instances are interpolations, or rather enlargements of the poem by subsequent additions of whole books, in the interest of other chiefs. That may perhaps be true of the tenth book, for instance, which most critics agree in regarding as a later addition. But on examination it does not appear that these ἀριστεῖαι of other chiefs are confined to the part of the poem which Grote would reject. Menelaus is made prominent in iii. and xvii., Diomedes in v., Aias Telamonius in vii. and xv., Agamemnon has his turn in xi., and Patroklos in xvi. All these displays of valor are actually conditioned upon the absence of Achilles from the field, and are at the same time poetically conditions of his return to it. They are actually conditioned upon his absence, for apparently there would be no occasion or room for them in the story, were he

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present. They are poetically conditions of his return, because as the poem stands, it seems that the poet designedly postponed his return until after they had all been worked into his plot. Now how does Grote deal with this view of the relation of these books to the rest? That they contain the "attempts of the other heroes to compensate for the absence of Achilles," says he, "is noway borne out by the poet himself. From the second to the seventh book, Achilles is scarcely alluded to. * * * He is mentioned two or three times as absent, and Agamemnon (ii. 377) regrets the quarrel, but we never hear any such exhortation as 'Let us do our best to make up for the absence of Achilles.'" (Note, p. 192.) As to this last remark, it is true there is no such exhortation to be found in those books, but it is also true that there is none in the much longer passage of the poem included in book viii. and books xi.–xvii. inclusive, that is, in the whole Achilleid before his return—so that the remark should have no force as an argument against books ii.–vii. The only approach to such a remark is made when Hera in v. and Poseidon in xiii. and xiv. exhort the Greeks not to let the absence of Achilles make so much difference in their success. As to the frequency of reference to Achilles, there are twenty places where his name occurs in the three thousand four hundred lines of these books, and in the books of the Achilleid in which he does not take part in the action (viii., xii.–xv.), containing three thousand one hundred lines, there are only fourteen references to him. Grote further remarks that these books (ii.–vii.) show "not the insufficiency of all the other heroes without Achilles, but the perfect sufficiency of the Greeks under Diomedes, Agamemnon, etc., to make head against Troy." The object of the Greeks however was not to make head against Trojan attack, but to capture the city by defeating utterly the Trojan army, and this they cannot do in these books—nor do they make any appreciable progress towards it. There is very little fighting, comparatively, in these books, for there is none at all in ii., and almost none in iv., while iii. and vii. are taken up with the two duels of chosen champions, and vi. with Hector's visit to Troy. So the only book full of fighting is

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the fifth, where the Greeks certainly have the advantage on the whole, but not more than they do afterwards at the beginning of xi., until their chief heroes are wounded. Of course, in speaking of the relative valor and success of the two parties in the Homeric battles, one must not leave out of view the influence of the gods, which is continually exerted, first on one side, then on the other. It may truly be said, I think, that no hero gains striking advantage on either side, without an explanation of it by reference to the help of a deity; and hence to trace the varying successes of the two sides, it is only necessary to note the intervention of the divine power here or there. Grote seems to lose this fact out of sight for the moment when he speaks (in the same note) of the glory of Diomedes as beyond that of Achilles; for the successes of Diomedes are due to the presence of Athene in his chariot, stop when she leaves him, and never approach to the feat of Achilles in killing Hector. The want of prompt fulfilment of the promise of Zeus to Thetis is due simply to the intervention of other deities which Zeus allows or which for a time goes on without his knowledge, and it is not confined to books ii.–vii. The change from Greek success to Trojan success is gradual, but it must begin to be perceptible somewhere. Why should not that first marked success come in the eighth book, without casting suspicion on the six books that precede it? Athene, as has been mentioned, gives the Greeks their success in v. Then in viii. she and Hera start from Olympus to help them again, but are intercepted and sent back by Iris as the messenger of Zeus. After that they do nothing directly to aid the Greeks, who suffer in consequence. But Hera in xiv. seduces Zeus to slumber, and meanwhile Poseidon does his part to put off the fulfilment of the promise to Thetis by helping the Greeks most actively. Previously too in xi. Agamemnon has driven the Trojans back from the camp to the very walls of Troy, and Zeus himself sends word to Hector not to risk a conflict with him, but to keep out of the fight until Agamemnon is wounded and withdraws. Thus Zeus himself, as has been pointed out before, allows a temporary triumph to the Greeks within the limits of the Achilleid,

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a fact which shows that similar successes of the Greeks in books ii.–vii. ought not to be used to cast doubt on the genuineness of those books.

The next point to be considered is the criticism of the way in which this enlargement, as Grote considers it, is connected at each end with the rest of the poem. At each place there is in his opinion an awkwardness or difficulty which marks the union of old garment and new cloth. At the beginning of the second book the awkwardness is that the dream and its false message "produce no effect. For in the first place Agamemnon takes a step very different from that which his dream recommended, and in the next place, when the Grecian army is at length armed and goes forth to battle, it does not experience defeat, but carries on a successful day's fight." To test the justness of this criticism (which, by the way, betrays the same desire to have results brought about at once which has already been noticed), let us ask: Why does Zeus send the dream at all? Evidently to induce Agamemnon to do something which without the dream he would not be so ready to do—that is, of course, to lead out his army to battle. Why should Agamemnon need such encouragement to do this, but for the plague and the retirement of Achilles? This incident then grows naturally out of the first book, and something like this was almost necessary to bring on the fighting in which sooner or later the Greeks were to feel the need of Achilles. It resembles very closely the treacherous suggestion made by Athene to Pandaros, whereby the contest is reopened in breach of the truce, in the fourth book. The dream then plainly produces its effect, though not immediately. Agamemnon trifles with the army, and, when they take in earnest his proposition of an instant return home, the intended effect of the dream is almost defeated; but as soon as that mistake is corrected the designed consequences do follow in the marching out of both hosts to battle. Again the duel between Menelaus and Paris postpones the general combat, but, after that is over, on the same day comes the fighting of books iv.–vii. There is no fighting at all mentioned in the first book, and only a single passage (490 ff.), where Achilles is

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said to abstain for twelve days from going to the agora or into battle, may be thought to imply it. How then should the poem go on from that first book? As it does in the eighth book, Grote would answer. But then at the very beginning of that book we find Zeus (10–17) forbidding the gods to aid either party—which command Athene (33–37) at once interprets as unfavorable to the Greeks, but promises to obey. This implies previous participation of some gods in the fighting, which has occurred with the express consent of Zeus in iv. and v. but has not been at all suggested if books ii.–vii. are omitted. In a note (p. 185), Grote modifies his opinion so as to include the dream, taking in the first forty-seven lines of the second book into his Achilleid, and going on from there to the eighth book. This is including, however, a little too much, for the last six lines of these forty-seven describe Agamemnon's dressing himself, and represent him as putting on his ordinary clothes for peaceful life, throwing his sword over his shoulder, and taking his sceptre in his hand. This is all right for the second book, where he goes out to call the Greeks to an assembly. But in the eighth book the first thing told of him is that in the midst of battle, about noon, he with other chiefs is terrified by the hostile thunder of Zeus, and retreats before the enemy. Now that description of his going forth in peaceful array to the ships of the Greeks is certainly left purposeless, and is inconsistent with what follows, if we go on from there to the eighth book. Suppose these six lines be dropped, and we read on from ii. 41 into the eighth book, then the difficulty remains that the dream should be so narrated and no subsequent reference ever made to it. In the prayer of Agamemnon himself in viii. 228–244, which he introduces by reference to former vows and boasts of the Greeks and bases upon his past offerings at the altars of Zeus, there is no allusion to this dream. The poet may well have forgotten the dream since three thousand five hundred lines have intervened, but could he at the distance of only two hundred lines?

But what shall be said of the false suggestion made by Agamemnon to the assembled army, which becomes the point

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of junction between Achilleid and Iliad, if the Achilleid went on from ii. 41 (or 47) to viii. 1? How can that be explained? Grote says the object was to introduce the splendid picture of the sudden breaking up of the assembly and the interference of Odysseus to bring the men back, together with the episode of Thersites. Thus he intends, if I understand him, to suggest that this part was an addition when the enlargement was made. Surely this is not a necessary and hardly a probable inference. Why should not such a scene be introduced and in such a way by the supposed poet of the Achilleid? In what respect is this picture out of place in a poem devoted chiefly to Achilles, any more than the scene at the close of the first book, where Hephaestos acts as cupbearer among the gods, or the elaborate description of Agamemnon's arming at the beginning of the eleventh book? That it comes in just here does not indicate a juncture here of old and new, for there seems to be but one other place in the Achilleid where it could fittingly come, and that is at i. 54. If one may offer a suggestion, it seems more probable, to work back from effect to design, that the poet's object in introducing the scene at all was to increase the glory of the Greek heroes, by showing with what difficulties they had to contend in their own followers—to bring more clearly before the mind the effect on the army of its nine years of fruitless war and absence from home. This, it is true, widens the view to take in the whole war, but not any more than do the ἀριστεῖαι of Agamemnon in xi. and Menelaus in xvii., and the battle of the gods in xx.

Again, at the close of the seventh book, Grote finds similar difficulty in the construction of the wall around the Greek camp, which seems to him to reveal the hand of an enlarger. "As the poem now stands," says he (p. 186), no plausible reason is assigned why "the wall should be built." "Nestor proposes it without any constraining necessity, for the Greeks are in a career of victory," etc. This is certainly too strong a statement. There seems to be sufficient reason for anxiety on the part of the Greeks in the utter disappointment of the hopes with which they entered the battle. These hopes are expressed by Agamemnon in ii. 37, 413–18, and by Nestor in

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ii. 436, being founded on the recent dream and looking to the capture of Troy on that day. Instead of such a triumph, though they had fought well and Diomedes by the present aid of Athene had achieved wonders, yet they had gained nothing, and repeatedly (v. 701, vi. 107 ff., vii. 17) they are spoken of as hard pressed and giving way. They are not in a "career of victory," but just holding their own against attack and in danger of being driven back to the ships (iv. 247 f., v. 791). Grote adds: "The Trojans are making offers of compromise which imply conscious weakness." Now this offer of compromise is expressly founded (vii. 351) by Antenor, who proposes it in the Trojan assembly, on the recent violation of the sworn truce by one of his own party, which in his view injures their prospects of success. Priam, apparently having no idea that the Greeks will accept the offer, adds to it a proposition for a truce until the dead are disposed of, with the expressed (vii. 377) intention of renewing the fight after it. But *before this message comes*, Nestor has made in the Greek assembly (vii. 337–43) the proposition to build a wall for the protection of the ships and themselves. And there is no hint in the answer of the Greeks to the Trojan message that they detect a confession of weakness in it. Still, granting that the proposal of a compromise and truce does imply conscious weakness in the Trojans, the fact that Nestor's suggestion of the wall precedes the coming of that proposal is a sufficient answer to the asserted inconsistency between them.

Grote goes on to show how on his theory the mention of the wall-building came to be introduced here. He supposes it to have been introduced because after the "brilliant scenes" in the books from the second to the seventh, it would surprise any one to find a wall spoken of at once in the eighth without any mention of its construction or previous existence, while in the Achilleid, passing at once from the first to the eighth book, there was no such previous success of the Greeks to make the existence of a wall seem strange. In his words, "since the Achilleis immediately after the promise of Zeus to Thetis went on to describe the fulfilment of that promise and the ensuing disasters of the Greeks, there was nothing to

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surprise any one in hearing that their camp was fortified." It does not seem quite clear how disasters, following upon and caused by a promise, should make the existence of a previous wall seem natural. The connection between disasters and promise would rather suggest that there had been no disasters and hence no need of a wall before the promise was made. In the first book there is no mention of the wall, though it might well have occurred, especially at 344, where Achilles says Agamemnon cannot plan for the future how the Greeks shall fight safely by their ships, and again at 409, where he asks Zeus to help the Trojans to drive the Greeks to their ships and the water's edge. In the Achilleid the first mention of the walls would be at viii. 177, where Hector speaks with contempt of them. His reference to them agrees better with their recent erection. "I am sure," says he, "that Zeus has appointed victory for me, but woe for the Greeks; fools, who, you see, were building these walls here, weak things, not worth a thought." He would hardly speak so if the walls had stood nine years, although, as a friend suggests to me, this contemptuous language just here may be ascribed to the excitement of success which would make the walls seem now a trifling obstacle in his way. Another reference to the walls as just built occurs in ix. 348–51, but Grote rejects that book. In regard to this wall-building, I do not undertake to defend the seventh book as it stands; I have only tried to show that Grote's criticism of it is open to objection. But there are other difficulties about it, which give probability to Bergk's (*Hist. Gr. Literature*) recognition of a later hand in the form of its account of the building of the wall.

I have now discussed the principal points which Grote urges in support of his general theory, passing over his treatment of the ninth book, because one might accept what he says of that book without accepting his theory of an Achilleid. But I do not wish to stop without noticing that part of his discussion, because it seems open to criticism, and the topic itself is interesting to all students of the Iliad. Grote's reasons for rejecting the ninth book may be briefly stated. There are passages in the eleventh, thirteenth, and sixteenth books, in



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the mouths of Achilles, Patroklos, Nestor, and Poseidon, which not only wholly ignore, but are inconsistent with, such a previous embassy carrying full apology and offers of restitution and compensation from Agamemnon to Achilles. Nothing more than such an offer is asked by Achilles from Thetis and by her from Zeus in the first book, so that this ninth book ought to end the poem if it belonged with the first. The terror of Agamemnon in the ninth book is neither accounted for by any previous disasters of the Greeks, nor consistent with his valor in the eleventh book. And finally the refusal of the offers by Achilles carries his pride and egotism to an excess such as to shock the Greek sentiment of Nemesis. These last two points may be taken up first, as I shall treat them briefly. The disasters of the Greeks in the previous books do not account for Agamemnon's "abject terror" in the ninth book. Elsewhere Grote has admitted that the disasters of the Greeks begin in the eighth book, and the fact appears strongly emphasized by the encampment of the Trojans outside the wall with the purpose, avowed by Hector, of preventing the Greeks from escaping in their ships under cover of the night. This is the very thing suggested by Agamemnon in the beginning of the ninth book, and for a long time no one knows what answer to make. Moreover it is not he who proposes the embassy, but Nestor, and Nestor too has already proposed, apparently for the first time, the stationing of a guard about the camp. That the fear of Agamemnon here is inconsistent with his gallantry in the eleventh book, shortlived as that display is and solitary, ought not to surprise any one who observes how all the heroes, except perhaps Aias and Diomedes, change often from one extreme to the other. That the obstinate refusal of Achilles to be reconciled to Agamemnon goes "beyond even the largest exigencies of insulted honor" is what one cannot so surely determine for another age and a different race of men. Such nursing of revenge might easily be paralleled from fact or fiction, and certainly cannot be ruled out from the possible conceptions of a Homer by the standard of a scholar and philosopher. And if it shocked the Greek sentiment of Nemesis, as well it might,

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do we not find in the poem that sentiment justified by the death of Patroklos, which would not have occurred had Achilles yielded sooner? I know it is denied that this can be regarded as a penalty inflicted on Achilles; I know it is not expressly so explained anywhere in the poem; but I can hardly think any one can read the Iliad or hear its story told without so connecting the two events in his mind, even if the ninth book is left out, and much less if it is included. It may be here observed that the idea of Nemesis is much more clearly formulated and recognized in the Odyssey than in the Iliad, and in the later Greek poetry than in either; and this may account for the absence of direct reference to it in this connection.

It is argued further by Grote that nothing more is asked by Achilles and Thetis of Zeus in the first book than "that Agamemnon and the Greeks may be brought to know the wrong they have done their capital warrior and humbled in the dust in expiation of it." This he regards as accomplished in the ninth book, so that the further progress of the poem after it is inconsistent. Now what Achilles really asks in i. 408–12 is that by the aid of Zeus the Trojans may drive the Greeks to their ships and the water's edge, slaying them, so that the Greeks and Agamemnon may fully learn their folly in insulting him. In xvi. 61 ff. the same idea recurs. "I intended," says he, "not to lay aside my wrath until the din of battle should have come to my ships." It may be added that this phrase "to my ships" does not occur in the first book, but is to be found in the formal answer sent to Agamemnon, at the extreme of Achilles's wrath, in ix. 650 ff. Now the situation in book viii., though bad enough, as has been said, to justify great disappointment and anxiety on the part of Agamemnon, is not yet bad enough to satisfy that fixed terminus of wrath on the part of Achilles. The Trojans with inspiration from Zeus (viii. 335) drive the Greeks back to the trench and wall and are encouraged to encamp on the field, but they do not yet pass the wall or threaten the ships. Hence the remark of Grote (note, p. 182) that the subsequent defeats of the Greeks are causeless cannot be defended. There is no suggestion in the prayer of Achilles in book i. of compensation

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or restitution of Briseis, and so the offers made in the ninth book do not satisfy him—he waits for something else. Now the whole tenor of the ninth book is in entire harmony with these expressions of Achilles in i. and xvi., and constitutes an explanation or expansion of them as well as of the μῆνις οὐλομένη of the first line of the whole poem, without which expansion, one may boldly say, we should only half understand the conception of the poet. It may be not original, but if not, we must admit that some one else saw better than the first poet what was involved in those words and thus developed it. The speech of Achilles in rejecting the offers of the embassy is a most complete and powerful expression of such a state of mind as is only suggested by his words in i. and xvi. His love of glory in war is gone altogether for the time (316–322). He chooses (393–415) a long, peaceful, quiet life, instead of the brief but glorious career which he decides for in xviii. He dwells continually on the old wrong done to him, repeating it over and over in almost the same words (335 f., 344, 367 f., 375, cf. 646 f.). In the view of such a passion gifts are nothing, and he labors in his language to express fully his contempt for them: not ten or twenty times as many as are now offered, not all the wealth of Orchomenos or Egyptian Thebes, not gifts countless as the sand or the dust (379–85) will pacify him until—what? He repeats here, with this intense emphasis, the idea expressed more precisely in i. and xvi. "Until Agamemnon shall discharge for me the whole bitter outrage" (ix. 387). That line, in that connection, cannot be explained unless by Achilles's own words, in the Achilleid itself, until the suffering of the Greeks from his absence had reached the limit his passion had fixed. This is the only adequate conception of the "accursed wrath" of Achilles. If we compare with this his speech in xvi., where he gives up in a measure his original purpose and consents to let Patroklos lead his men to battle, we find the same feeling there. He repeats there in 54 and 58 his complaint in nearly the same words. He repeats, as was mentioned above, the same intended limit to his inaction. But he does not speak so strongly in other respects, both because he is addressing his

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friend, not the messengers of his enemy, and because he is now laying aside in a measure his former wrath. The rest of the speech will be considered farther on in another connection. This then is the answer to Grote's argument that Achilles asks nothing more than the humiliation of the Greeks and the return of Briseis, and that therefore, to be consistent with himself, he ought to accept the offer made in the ninth book. He does ask something more in i., as he himself says he had in xvi., and with these two passages of the Achilleid his conduct in ix. is consistent, and these are the only places in the poem where he suggests a limit to his wrath.

We take up now the subsequent passages which seem to Grote irreconcilable with the previous occurrence of the embassy of the ninth book, and which constitute the strongest argument against that book.

The first of these is in xi. 609 f., where Achilles, seeing a wounded man borne to Nestor's tent, calls Patroklos to go and learn who it was, and says to him: "Now I think the Greeks will come to my knees in supplication, for their need is no longer to be borne." It cannot be denied that it is strange that in this remark there is no reference to the ninth book; he would naturally say "will come *again*," if the ninth book had preceded. It does not seem, however, "a glaring inconsistency," as Grote calls it, for it appears from the ninth book that Achilles expected to be approached again and again in the same way. At the beginning of his address in answer to Odysseus he says (ix. 309 ff.): "It is best to declare my purpose to you bluntly, * * that ye may not come one after another to sit down and coax me." His remark in xi. then seems to mean that he expects what those words in ix. were designed to prevent.

Again in xi. 656–803, in Nestor's appeal to Patroklos to urge Achilles to relent at least so far as to let Patroklos take his armor and go into battle, Grote finds an argument against the ninth book in the absence of any reference to the offers there made. But what place is there for such reference? Remorse for the rejection of that offer would hardly move Achilles if the present disasters of the Greeks did not. And

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Nestor had no authority to renew the offer without consulting Agamemnon. Furthermore, there is in the speech a passage which seems to allude distantly, as would be natural, to the fact that an attempt to persuade Achilles had been made and failed. After reminding Patroklos how his father Menoetios had directed him to influence Achilles with good advice, Nestor adds (xi. 790 ff.): "but even now remind him of this, if perhaps he may yield. Who knows but that with a god's help you may move his mind by counsel? There is power in the persuasion of a friend." These words gain meaning if they are taken as contrasting this appeal from a friend with the failure of the formal embassy.

Next comes, in the thirteenth book (95–124), the exhortation of Poseidon to the Greeks in battle, in which, admitting the injury done by Agamemnon to Achilles, he says: "even if he is to blame, it is not for us to be slack in fighting." Then follows a line (115) which Grote translates: "let us make an effort to heal the sore; the minds of good men admit this healing process": ἀλλ' ἀκεώμεϑα ϑᾶσσον· ἀκεσταί τοι φρένες ἐσϑλῶν. The rest of the speech refers only to courage in battle. Grote regards this line as quite inconsistent with the supposition that an attempt to "heal the sore" had already been made in the ninth book without success. It would be so certainly, but is the line to be understood as he translates it? His idea seems to rest upon a suggestion made by Heyne in his notes on the Iliad without any argument to support it, which apparently has not been adopted by later scholars so far as I can ascertain. At least Faesi and Passow (in his Lexicon) return to the earlier understanding of the line as referring to making good by valor the loss of Achilles, an exhortation which the same god utters without any possible ambiguity in xiv. 364–375. This suits better the use of ϑᾶσσον, and the whole context. The line then means in its connection: "Even if Agamemnon is to blame for the absence of Achilles, it is not for us to be slack in fighting, *but let us at once make up the loss; the courage of the brave is not beyond repair.* But do ye no longer, being all chief heroes in the host, neglect your prerogative of valorous defence." On this view, all lack of reference or place for

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reference to book ix. vanishes. The conversation between Achilles and Patroklos in xvi. 21–100 and the address of Patroklos to his troops in xvi. 269–274 are the next passages which Grote takes up, and the difficulty that he finds with all of them is the same, that they are inconsistent with the previous occurrence of such an offer of restitution and compensation as is made in the ninth book. It is not perhaps worth while to consider the passages singly; let it suffice to say that while with Grote's conception of the wrath of Achilles, that it would yield at once to the offer of the ninth book, they are fatal to the genuineness of that book, on the other hand if one reads them with the idea of Achilles's wrath expressed by him in i. and xvi., there appears no reason for reference to a premature and unsuccessful attempt to propitiate him, which, when it was made, he treated as an insult. Moreover, as is well suggested by Bäumlein, there is apparent in xi. and xvi. a gradual giving way of the anger of Achilles, which is best explained by the unrecognized effect upon him of the embassy with its offer of atonement. It appears first in his taking interest enough in the combat to stand on his ship and watch it (xi. 600 f.), then in his sending Patroklos to inquire who the wounded man was (611 f.), then in his sympathetic suggestion, when he returns, that it may be pity for the Greeks that makes him come weeping back (xvi. 17 f.), and finally in his permission to him to go into the battle and the eagerness with which he hurries him off (xvi. 80 f., 125–8). A similar gradual change of feeling in the three speeches of Achilles in book ix. is pointed out by Gladstone in his Homeric Studies. "I will go home to-morrow," he says to Ulysses; "We will deliberate to-morrow whether to go home or not," to Phoenix; "I will not fight until Hector threatens my ships," to Aias.

The same thing is true of the eighteenth and nineteenth books which has just been said of the sixteenth. The general absence of reference to the ninth book will not seem strange if one has in mind the conception of the wrath of Achilles now suggested, that it made him scorn offers of apology and atonement until it had seen its satisfaction in a full measure

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of calamity to the Greek army. The offer made in the ninth book seemed to him of so little importance when made, and is now so driven out of his mind by the death of his friend, that he does not once think of it. His rejection of that offer was but a part of his whole conduct, consistent with the rest, and what he now repents of is the whole, not any part by itself. And why should Agamemnon, in this critical time of reconciliation, risk provoking Achilles again by referring to his refusal of that offer? He does mention, in his only speech bearing upon the quarrel, that he offers now the same gifts as before (xix. 140 f.); but that passage with three others which expressly refer to the embassy of the ninth book Grote regards as interpolations. These passages may well be defended, but there is no room to do it here.

Two other passages, subsequent to the ninth book and seeming to imply it, ought to be mentioned, to which Grote does not refer. One is xi. 794 f., where Nestor says to Patroklos: "But if Achilles is in dread of some oracle and his mother has given him a warning from Zeus, let him send you in his place." Now there is no previous mention of any warning from Thetis to Achilles, except in his words in ix. 410–16. If it be objected that when Patroklos repeats (xvi. 36 f.) this conjecture of Nestor's, Achilles answers (xvi. 50 f.): "I am not in dread of any oracle, nor has my mother warned me from Zeus," the answer is plain that the denial of Achilles means: "That is not my reason for refusing to go into battle." Such a form of expression in Greek is illustrated often, e. g. Ev. Joan. ix. 3. Again, when in xiv. 74–81 Agamemnon for the third time proposes a return home, none of the chiefs present suggests an effort to appease Achilles, or makes any reference to him. Does not this, in the Achilleid, consist perfectly with the previous failure of such an attempt, and need explanation if the ninth book is rejected? The first occasion of such a proposition was in ii. 139 ff., when all the chiefs knew it was not made in earnest; the second in ix. 26 ff., when the embassy follows.

Let me now say in conclusion again that my only attempt has been to present what could fairly be said (passing over a

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few minor points) in criticism of Grote's theory and of his arguments in support of it. My study of it has impressed me with admiration of his clear view and firm grasp of the substance of the poem, which is the more remarkable when one remembers that this theory forms but a brief episode in the grand movement of his noble investigation of Greek history. His theory may be open to fewer objections than any other, but I do not see how some of the objections to it can be removed. It will perhaps seem to some that my criticisms upon it are concerned with minute points. It is on single lines and phrases, however, that the question must turn in great measure, as there is no ground for argument except in what the poem itself furnishes. On the whole I would rather accept the Iliad as it is, considering it a single poem with a poetic freedom in disregarding inconsistencies and subordinating even natural sequence to occasional impulse, than accept Grote's explanation of its present shape.

It may be worth while to add that, so far as I can ascertain, the different investigations seeking in the language or metre of different parts of the Homeric poems a criterion of difference of age, such as Giseke's on the use of prepositions, the order of words, the use of enclitics, etc., Friedländer's on the unique words (*ἅπαξ εἰρημένα*), do not show any distinction between books ii.–vii. and the rest of the Iliad.



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## III.—*On Negative Commands in Greek.*

By M. W. HUMPHREYS,

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The object of this paper is not to produce any new facts, but to offer, in a sort of tentative way, an explanation of the phenomena which are stated, perhaps, with sufficient clearness and accuracy in some of the grammars.

The fundamental rule is to employ the present imperative or the aorist subjunctive in prohibitions, while in positive commands the imperative of both tenses is employed. Now, commands are more likely to give offense than any other ordinary communications, and the imperative is the full representative of a command. Hence, even in positive commands, we not unfrequently find substitutes for this mood, such as the optative with ἄν, or even without ἄν, in Greek; the optative in Sanskrit; the future indicative second person and the subjunctive third person in Latin, etc. These substitutes have their origin in another sort of sentence, and do not have the direct and full force of a command. But negative commands are much more likely to give offense than positive ones, because, first, they are more likely to do violence to the will of the person commanded, while, when a positive command is given, the person is often expecting or awaiting orders; and, secondly, they indicate, on the part of the one who commands, an assumption that the one commanded is going to do the thing forbidden unless he is prohibited. Hence, "young America," when told not to do a thing, is often heard to reply: "You'd better wait till I go to do it," or even: "You'd better wait till I do it." But prohibitions, like all commands, are of two kinds: first, those which are general, such as laws, moral admonitions, standing injunctions, etc.; and, secondly, those which are special, relating to some individual act that is about to be performed unless prohibited. Prohibitions of the former class are not so likely to give offense. Generally the order is of such a character that it is expected and approved of, and frequently a person included under it does

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not take it to himself at all, but thinks it is more especially adapted to the case of his neighbors, such as: *μὴ κλέπτε*, "Thou shalt not steal." But special commands come directly home to the person addressed, and if he was, otherwise, going to do the thing prohibited, he feels that violence is done to his will, and that the course he was going to pursue is implied to be an improper one; while, if he was not going to do it, he feels a just indignation at the presumption of the person who forbids it.

Now, in Greek the present imperative is used for the most part in general commands, and hence is retained in negative sentences, while the aorist imperative is employed for special commands, and so needs mollifying when negative. Of course it is not claimed that the present is used *only* for general cases; but the very fact that this is its normal use renders it a modified command when it is applied to a special case. The general order is issued, and the special case is merely one of its applications, and the shoe need not be worn if it does not fit.

Commands expressed in the third person frequently have reference to absent individuals, and as we are not as considerate towards them as we are towards those present, we should expect to find the aorist imperative retained in such instances; and in fact, just in the third person, the aorist imperative is by no means uncommon. But why is it not universal? Because it does not always apply to an absent individual, although it is in the third person, as: *μή σοι μελήσῃ*, 'let it not concern you.' An apparent objection is found in the fact that sometimes we meet with the aorist imperative third person when the command does apply to the person addressed, as PLAT. Crit. 45, B: *μήτε ταῦτα φοβούμενος ἀποκάμῃς σαυτὸν σῶσαι, μήτε ὃ ἔλεγες ἐν τῷ δικαστηρίῳ δυσχερές σοι γενέσθω*: where we have the aorist subjunctive second person and the aorist imperative third person both in one sentence, and it is quite evident that the imperative relates, as well as the subjunctive, to the person addressed. But we must not suppose that the Greeks, after their language was fully developed, were conscious of the reason why they adopted certain modes of expression: why, for instance, they employed the subjunctive rather than the

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imperative in negative commands. The cause of this modification did not have universal application in the third person; whence was developed the usage of employing sometimes the one form and sometimes the other, and in the course of time, when the origin of the distinction was lost sight of, they began to use both forms almost or quite indiscriminately. Moreover, the very fact of the commands being expressed, though only grammatically, in the third person, serves as a sort of mollification.

There is, however, one kind of negative commands, not included either in the general or in the special; and that is, when the act forbidden is already going on. In such cases the aorist does not suit, as it looks to an individual act in the immediate future. Strictly and logically, the imperative of the word denoting the act in question should not be employed at all, but the participle with the aorist imperative of παύομαι, 'cease,' as: παῦσαι λαλῶν, 'cease speaking'; but the one positive command, 'cease' or 'stop' doing a thing, is as offensive as a negative command, and virtually is one. Hence, when we choose to employ the ordinary expression of a command without the circumlocution with παῦσαι, the only way is to place it under the form of a general injunction 'not to do' so and so, or the progressive form '*do not continue*' doing so and so, and let the person so commanded *infer* that he is to cease. Consequently we find the present imperative in such cases. It may be added that in this instance the principal element of discourtesy—the assumption that the act is going to be performed—no longer exists, for the act is already going on.

It may be objected to all this that, if it is true, the principle should apply to other languages as well as to Greek. This will depend on two things: first, the sense of politeness and regard for the feelings of others on the part of the people who employ the language, and, secondly, the capability of their inflected forms to furnish a substitute for the imperative. (Of course a substitute would be developed if the people were desirous of being polite or courteous when they were creating their inflections; but the sense of politeness would not be very considerable at so early a period in the history of any people.)

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Accordingly, as far as I am acquainted with the ancient inflected languages, the principle holds good everywhere. In Latin, the retention of the long, that is, the *general*, form of the imperative, and the replacement of the short, or *special*, form by the subjunctive in negative commands, are familiar to every one. Of course there are exceptions here, as everywhere else; but some of these are only apparent, as: *Tu ne cede malis sed contra audentior ito*, where *cede* is not used so much where *cesseris* or *cedas* should be expected, as where we should look for *cedito*, as is shown by *ito* connected with it; that is, the short form is used in the sense of the long, and so does not need modifying.

So in Sanskrit, the aorist tense with suppression of the augment is often employed instead of the imperative mood in negative sentences, as *mā kṛthās* (=μὴ ποιήσῃς). The imperative may also be employed; but to what extent the one is used for special and the other for general prohibitions (if the distinction exists at all), I am not prepared to say; but I have no doubt that such a distinction once existed, just as it once did between the imperfect and the aorist; and it is certain that this substitute for the imperative was introduced to mollify *negative* commands only.

Even in Hebrew, where there is no distinction between the special and the general, the imperative is never used in prohibitions, whether general or special, but the future is employed, as לֹא תִגְנֹב (=μὴ κλέπτε or οὐ κλέψεις).

In modern languages, where inflections have to a considerable extent disappeared, but courtesy has attained a high grade, it has become customary to modify almost all commands addressed to individuals, by some apologetic expression, such as 'please,' 'bitte,' 's'il vous plait'; and in some modern languages the negative imperative is never employed at all.

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## IV.—*On Hebrew Verb-Etymology.*

By CRAWFORD H. TOY,

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MANY years have elapsed since the first attempts were made to analyze existing Hebrew verbal stems in order, if possible, to reach simpler forms, and yet great uncertainty seems to hang over the results obtained. Such an analysis will be a very valuable contribution to Hebrew lexicography, and must certainly precede any attempt at a comparison between Hebrew and non-Shemitic languages. It is therefore the interest both of the Shemitic student and of the comparative grammarian to engage in the investigation of the form of the Shemitic verb-stem—I say Shemitic, because it is impossible to understand the Hebrew verb or that of any other dialect of the same family, without taking into account all the other dialects, and because their substantial identity is so completely demonstrated as to allow us to use one for the explanation of another. And if the search for Hebrew roots should seem to have anything in common with the search for the Philosopher's Stone, it may be hoped that it will at any rate, like that vain pursuit, bring to light many valuable facts and pave the way to a scientific construction of the verbal system of the language.

A word may be proper as to the relation of the stem to the root. Every word, after all inflectional and other merely modifying elements have been removed, is reduced to a simplest form, which contains the fundamental meaning of the word, and to which the name *root* is commonly given. The significance of this term, however, must always be relative; that is, it must be understood as setting forth the limits of our knowledge with respect to any given phonetic complex, and as being always liable to modification from further research. A set of words called roots by one generation may in the next generation have been resolved into simpler elements, which will then be roots till they are in their turn resolved, and so on. The name *stem* is given to the existing form, which is regarded as resulting from the addition of uninflectional

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elements to the root. Thus the Hebrew *katab* 'he wrote' may be conceived of as built up from a form *kat* with some more general meaning, or from a still simpler *ka* having a still more general signification. Or, it may be supposed that *katab* itself is an original form, not developed out of any simpler one. In the latter case the name 'root' will be given to *katab*, in the former *kat* or *ka* will be the 'root' and *katab* the 'stem.' We have here supposed the vowels to be component parts of root or stem; but it may also be inquired whether the original form was not vowelless (*ktb* or *kt*), merely ideal and unpronounceable, the vowels serving to attach some definite formal conception to the word. Further, if *katab* have come from *kat*, it is necessary to inquire into the origin of the added *b*. Here we shall call the existing Hebrew verb-form the stem, and designate by 'root' any simpler form to which the stem may be referred.

The ultimate elements or roots of language are conveniently divided into the two classes, Substantive or Material (Verbs and Nouns) and Relational or Formal (Pronouns and Particles); this division may be adopted without deciding whether it will eventually turn out to be well-founded, that is, without deciding whether the pronominal forms of language have arisen independently of substantive words. The distinction between these two classes is well marked in Hebrew; not so, however, the difference between Verbs and Nouns. A verbal stem, indeed, is in our dictionaries supposed to underlie every nominal form, but strictly speaking every verb is also based on a nominal conception. In the last analysis the grammatical difference between Noun and Verb disappears; the former is the name of a thing considered as an isolated and motionless object of thought, the latter the name of an action or state considered as fused into a unity with its agent or subject. Thus *katab* signifies the act of writing; if the pronoun *na* 'we' or 'us' be added to it, there results *katab-na* 'a writing pertaining to us,' which, viewed as an isolated object, is the noun 'our writing,' but viewed as combined with its agent is the verb 'we write.' And as the Hebrew noun-stem is identical in form with the verb-stem, it is unnecessary to distinguish

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between them in an examination into substantive roots. The object of this paper is to give an account of what has been attempted up to this time in the analysis of the Hebrew verbal (that is, substantive or material) stem, so as, if possible, to determine what may be regarded as certain or probable, and what as doubtful or unknown. It will be proper first to inquire as to the present form of the stem, and afterwards as to the possibility of resolving it.

1. It will appear below that the view taken of the form of the existing Shemitic stem is not without influence on the methods of analysis adopted, and it will therefore be necessary to make a somewhat careful statement of the facts. Triliteralism is generally acknowledged to be the distinguishing characteristic of the Shemitic family of languages, that is, a triconsonantal stem, the substantive signification of which resides wholly in the consonants, the function of the vowels being simply to give coloring to the meaning. A few stems contain four letters (quadriliterals) and still fewer contain five (quinque-literals); the origination of most of these from triliterals or biliterals is tolerably obvious. We may therefore take as the type of the ordinary transitive verb the perfect third person singular masculine *katab* (neuter *katib* (*kateb*) and *katub* (*katob*))*. But we may go further and say that no Shemitic verb-stem contains fewer than three consonants, that those that seem to be monosyllabic (namely, the weak verbs, those with second radical doubled, and those with initial, medial, or final weak letters) are really contractions from true triliterals. The structure of the language would lead us to look with suspicion on this alleged monosyllabism, for the principle of triconsonantism does not exhibit its power wholly or chiefly in fixing the number of stem-letters, but controls the whole word-structure. Namely, it is a consequence of, or rather, a

* Philippi (*Der Grundstamm des starken Verbums im Semitischen, &c.*, in the *Morgenländische Forschungen, Festschrift Professor Fleischer gewidmet*, Leipzig, 1875, p. 75, sq.) has made it probable that the form of the primitive Shemitic simple stem is not *kataba* or *ktab*, but *katab*. He maintains that the forms in which the personal endings begin with consonants, as *katab-ta* suppose the dissyllabic stem, and that this explains the phenomena of the tone or accent and general vocalization, not only in Hebrew and Arabic, but also in Aramaic.

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coördinate fact to, this rigid consonantism, whereby the substantive meaning of the stem is given in its consonants, that the inflectional and other modifications of meaning are largely expressed by the vowels that connect themselves with the stem-consonants, and this in such a way that the significance of the vowel depends on its syllabic position in the word. Hence come certain definite laws of vowel-coloring (by which expression no symbolical significance is claimed for the vowels) and vowel-change, which run through all the word-forms and are obviously based on the triliterality of the stem. Thus, in the large class of segholate nouns the singular is marked by a slender vowel on the first syllable (*malk*, Heb. *melek*), and the plural in Hebrew by a half-vowel in the first syllable and a broad *ā* in the second (*mᵉlakim*); and this sequence has evidently been observed in the case of monosyllables, where it is obscured by contraction, as in *yōm* 'day' (from *yaum*, for *yawm*), plural *yamim* (for *yᵉwamim*); in the verb the transitive character is marked by *a*, the intransitive by *i* (*e*) or *u* (*o*) in the second syllable, the passive is denoted by two peculiar vowels (*u* and *i*), and the active and passive participles are distinguished by the vowel-coloring of the second syllable; the diminutive is denoted in Arabic by a uniform sequence of *u* and *ai* (as *rajail* 'a little man' from *rajul* 'a man'), which rigidly maintains itself in all nouns, whatever the number and character of their consonants (*ubaiy* 'little father' from *ab* 'father'); in monosyllabic forms with the second consonant doubled by *dagesh forte*, the reduplication is resolved when the dissyllabic sequence of vowels is necessary to characterize the form, as from the perfect *sab* the participles in Hebrew are *sobeb*, *sabub*, the Arabic infinitive (Stem III.) is *sibāb*, the Hebrew noun *am* 'people' sometimes simply doubles its second consonant (when a half-vowel occurs in the second syllable), as *ammi* 'my people' (for *amᵉmi*), sometimes resolves the reduplication (when a full vowel stands in the second place) as plural *amamim*.* As with the phenomena of vowel-color-

* Ewald and others hold that the primitive nominal and verbal forms are distinguished by their vowel-coloring, the former having the vowel in the first syllable (*katb*), the latter in the second (*katb*). But noun and verb both go back to th



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ing, so with those of vowel-change—they suppose a triliteral dissyllabic stem. The mutations of vowels depend on the nature of the syllable and the place of the tone or accent. Take, for example, the nouns in Hebrew (which indicates the vowel-mutations more minutely by its vowel-notation than any other Shemitic language), the changes in which may be discovered to a great extent by comparison with the related dialects. The vowels of monosyllabic nouns and the changes connected with inflectional additions point naturally to, and are satisfactorily explained only from dissyllabic stems. *Moth* (for *mauth*) is from *maweth* ($m^a w^a t$), *zēth* (for *zaith*) from *zayeth* ($z^a y^a t$), *shem* 'name' (Aramaic *shum*) is from a form with final Waw or Yod, as is shown by the Arabic and Aramaic plurals *samaw.at* and *sh'mah.at*, *yād* 'hand' from a similar stem (Arab. plu. *yudiy.yun*), many monosyllables double the second consonant before additions (as *ēm*, *shēn*, *kēn*, *ām*, *yāṁ*, *ṭal* and others), *ab* 'father' and *ah* 'brother' have the forms *abi* and *ahi* before suffixes, and the numeral *shēsh* 'six' appears in Arabic as *shadsh*. Here the origin of the two first forms lies on the surface, and in the others by comparison of the plural forms we can trace the vowel of the singular. From an original *zayat* with the accent on the first syllable, the second vowel, being untoned, would naturally be degraded and finally disappear or coalesce with the first, whence *zait*, *zēt*; the broad *ē* of *sem* is an extension of *ă* in *samay* or *i* in *simay*, as *ṣēfer* 'book' from *ṣafar* or *ṣifer*, while in *yād* the *ă* has passed into *ā*, and similar extensions occur in the reduplicated forms; *āb* and *āh* are for *abaw* (*abay*) and *ahaw* (*ahay*), whence *ahot* 'sister' for *ahawat*. Nor do we find an exception to this general principle in those verbal forms that are cited as examples of true Shemitic monosyllabism. In the concave verbs the three ground-forms with middle *ă*, *i*, and *u*—*kān*, *meth*, *bosh*—are readily explained as contractions from *kawan*, *mawith*,* and *bawash*, while *kān* cannot be accounted for from

form *katab*, on which the present nominal and verbal forms are based; see Philippi, *ubi supra*, p. 75.

* Ewald (*Ausführl. Lehrbuch d. Heb. Spr.* § 54) explains *kān* as from *kāwan* and *mēth* as from *māwēth*, holding to the specifically Hebrew form of the ground-stem (*kātab*, *kāteb*) instead of going back to primitive Shemitic form (*katab*, *katib*).

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the stem *kūn*. The persons whose suffixes begin with consonants have in Hebrew *kăn* (in Arab. *kŭn* from the third class, middle *ŭ*), *math* (Arab. *mith* shortened from *meth*, Aeth. *meth*), *bōsh* or *bŏsh* (Arab. *bŭsh*, Aeth. *bōsh*), as the firm verbs in *ē* and *ō* write *ă* and *ō* or *ŏ* in these persons (*kabad.ta*, *yakol.ta*). The Hebrew infinitives *kōn* and *kūn* are for *kᵉwōn*, imperfect *yākun* for *yak.wun*, participles *kān* and *kūn* for *kāwin* and *kawun*. So the derived stems are easily accounted for from the triliteral ground-stem. The Niphal perfect *nākōn* is for *nakwan*, the weak *w* falling out and the *ā* passing into *ō* as in the active Qal participle of the perfect verb (Heb. *koteb* for original *kātib*); the infinitive *hikkon* for *hikkān* from *hikkawan* for *hin.kawan*. From the original *hakwan* and *hukwan* the Hiphil *hekin* (for *hakin* and this for *hakan*) and the Hophal *hukan* come without difficulty. The forms of the verbs with middle radical doubled also are easily gotten from the triliteral stem. The infinitive *sob* is for *sᵉbob* (from *sobeb* or *subeb*, Arab. *sabbun*), the imperfect *yāsob* is for *yasbob* or *yasbab* (Arab. passive *yasbub*, indic. *yasubba*, Aeth. *yesbab*, *yesabeb*, Aram. *yissob* or *yissub* for *yassub*), Niphal *nasab* for *nasbab*, imperfect *yissab* for *yissabeb* (Arab. *yansabbu* for *yansabibu*), Hiphil *heseb* = *hasab* for *hasbab* (Arab. *asabba* for *asbaba*, Aram. *asseb* for *asbeb*, *asbab*, Aeth. *asbaba*), Hophal *husab* for *husbab*. It is equally clear that the forms of Verbs First Yod and Waw are based on the triliteral stem: Qal imperfect *yesheb* for *yawshab* (the *w* falling out) and *yirash* for *yiyrash* (Aeth. *ye.wger*, *yelad*) Hiphil *hoshib* for *haushab*, *hawshab* (Aeth. *awlada*), *hetib* for *haitab* = *haytab* (Aeth. *aytaba*) Hophal *hushab* = *huwshab*, Niphal *noshab* for *naushab* = *nawshab*. Evidently the various forms of the verb in the several dialects become symmetrical and regular if we suppose them based on a triconsonantal stem, and some of them cannot be otherwise understood. In the geminated verb the doubling of the second letter before a vowel and the frequent resolution of the doubled letter point to a stem *sabab*. In the concave verb the Qal perfect *kān* cannot be satisfactorily accounted for from a stem *kun*, for if we suppose a *ku.an* contracted into *kan* (as Arab. Pass. *kuwina* into *kina*), this is really intro-

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ducing a middle consonant *w*; on the other hand, a stem *kan* will not account for the introduction of the radical *w* in the intensive stems in Arabic, Aethiopic, and Aramaic. The causative forms *hoshib* and *hetib* might be considered as coming from stems with initial vowel, *ushab* and *itab*, but such stems do not actually exist; we find only triliterals beginning with full consonantal *w* and *y*. Finally, these two letters appear as consonants in the Verbs Third Waw and Yod (Heb. Third He, Aram. Third Aleph), rarely in Hebrew (in Qal pass. participle, and sporadically elsewhere), and regularly in certain forms in Arabic, Aethiopic and Aramaic; and all other forms receive easy explanation on the supposition of a triliteral stem. These three classes of verbs, therefore, cannot be properly designated as having initial, medial, and final vowels.

The quadriliteral of the form *gargar* does not exhibit an existing biliteral, for the *gar* is not found separate. The *gar* is the basis of *gargar* as *sab* is of *sabab*, but neither *sab* nor *gar* is an actual word, nor is any such substantive word to be found in the Shemitic languages without an intimation in its inflections that it is based on a longer form. We are therefore warranted in saying that there does not exist a Shemitic substantive stem of less than three consonants, and that any stem-analysis must consist essentially in the removal of one or more consonants. In an active triliteral the first vowel must always be *a*, and it may be inferred from the preceding statements that this *a* must always be short (in the primitive stem). The second vowel may be *a*, *i*, or *u*, of which the first marks the transitive, the second and third the intransitive character. The quadriliteral has only *a* in both syllables, broadened into *ā* or diminished to *ĕ* or taking the form *ē*.

2. Having fixed the form of the existing Hebrew stem, we may now go on to examine the attempts that have been made to reduce it to a simpler form. It obviously does not follow that the present triliteralism is original; the question as to the original form can be answered only by an examination of the phenomena of Hebrew and the other Shemitic languages. It has, indeed, been shown that the whole structure of these languages is based on triliteralism, that this, therefore, is an

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essential feature of primitive Shemitism, that a Shemitic primitive language without triliteralism cannot be conceived. But while this is undeniable (taking the term Shemitic to indicate the present stage of development) it by no means follows that there may not have been a process of growth in the language, whereby it attained its present form from simpler beginnings. The possibility of such a growth of stems is, however, denied (as by Renan*) on grounds derived from the linguistic truth first clearly set forth by William von Humboldt,† that the genesis and development of a language is to be regarded not as an accidental, mechanical conglomeration of sounds, but as the realization of an idea; that we are not to suppose, for example, that Sanskrit or Arabic is a mere advance from a condition of Chinese monosyllabism to a position that Chinese itself might have reached under favoring circumstances; this would be as unfounded a view as that a fern might under favorable conditions grow into an oak. Impressed with this truth, Renan contends that a language must leap into existence full-grown, that, being the product of the spirit of a people, it could not be originated before that spirit had come to full self-consciousness, and, having been originated, could undergo no change. However, Humboldt's view does not exclude growth, but rather supposes it. He rightly claims for each language a definite "form," which clothes itself with "matter," and manifests itself by the manner in which it selects and employs its matter. While this view insists on the unity and individuality of each tongue, and opposes the opinion that linguistic growth is a merely accidental, aimless progression, it is perfectly consistent with the utmost individual freedom in the word-makers, and indeed assumes such freedom as the instrument and immediate originating cause of a language. It is only by combining these two apparently diverse elements that we can account for Shemitic triliteralism and the phenomena of language in

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* *Histoire Générale des Langues Sémitiques*, pp. 97 sq., 418, 421.

† *Ueber die Verschiedenheit des menschlichen Sprachbaues* (Vol. VI. of Kuhn and Schleicher's *Beiträge*). Comp. Steinthal, *Die Sprachwissenschaft W. v. Humboldts*, and *Charakteristik der hauptsächlichsten Typen des Sprachbaues.*

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general. Men at first probably get the materials for language from the imitation of natural sounds; yet each group of men living together and learning from one another is controlled by its individual character in the formation of its language, its own nature sets bounds to its freedom of movement, and, on the other hand, within these bounds it is capable of effecting any linguistic changes. Whether any particular phenomenon falls within these limits can only be determined by examination. Humboldt regarded Shemitic original monosyllables as possible and even probable;* properly speaking, if Shemitic stems were resolved into monosyllables, the monosyllabic language thus reached would not be Shemitic, but it would present the materials out of which Shemitism was developed. On the other hand, it is unnecessary to assert that original polysyllabic roots (with or without a simpler ideal basis) are impossible. Though it may seem improbable that the primitive word-makers would give a polysyllabic name to a new object, especially that they should mentally frame a polysyllable on the basis of an ideal monosyllable, yet we know too little of the conditions of primitive language and of the nature of ultimate roots to decide positively such questions as these.† Assuming only the possibility of stem-analysis, let us inquire whether there is reason to hold to the existence of a form simpler than the present triliteral stem. The inquiry will be facilitated by an examination of the various schemes of analysis that have been proposed, and even those that have proved untenable may not be without instruction.

(1). The *reduplication-theory* of Ernst Meier is one of these. Assuming the original biliterality of the roots, and struck by such cases of reduplication as the geminated verb (*sabab*), and by the fact the ground-stem of the verb (*katab*) is a perfect (but in reality proceeding on Indo-European analogies),

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* *Ubi supra*, p. 405.

† Without adducing Indo-European analogies to decide a principle of Shemitic growth, we may illustrate now the possibility of the growth of stems from the Indo European multiliteral verbs, in which the longer forms exist alongside of shorter and obviously original ones, as Sanskrit *yug* and *yu*, *mat* and *ma* (Schleicher, *Compendium der Indogermanischen Sprachen*, § 206), English 'stood' (root 'sta'), 'stack,' 'stall,' etc.

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Meier concludes that all Hebrew stems are perfects in signification and formed by reduplication (and subsequent vowel-changes) from original biliterals. He supposes three* modes of reduplication: the first radical letter may be repeated at the beginning, as *hakam* from *kam* (*hakam* for *kakam*, *h* interchanging with *k*); or the first letter may be added at the end, as *saras* from *sar* and *nagan* from *nag*; or the second letter may be repeated at the end, as *halal* from *hal*. In the biliteral root, he thinks, the fundamental signification belongs essentially to the final consonant (whereby he would seem to point to a reduction of biliterals to uniliterals), and the reduplicated root-consonant, which forms the stem, has a merely inflectional function, namely, to express the perfect. But, as this modifying consonant undergoes various phonetic changes, he regards these changes as indicating *nuances* in the ground-signification of the root. Thus, if the root *ḳaẓ* signifies 'cut,' the stem *ḳaẓaẓ* properly denotes merely the completed action of cutting, but the stems *ḳaẓah*, *ḳaẓar*, *ḳaẓab*, in which the reduplicated consonant (*ẓ*) is transformed, express modifications of the notion of cutting or peculiar sorts of cutting. In fact, then, Meier does not restrict his reduplicated consonant to an inflectional function, but attributes to it a substantive force, while at the same time he does not distinguish between the effects of the reduplication of the first and of the second letters of the root, though he regards the signification as residing solely in the latter. To all roots whose second letter is a labial he attributes the signification 'bring together,' to all others (those ending in dentals, linguals, palatals, or gutturals) the signification 'separate'; to these two conceptions he traces all the verbs in the language.† That the signification of the root is in the second consonant he undertakes to show by actual examination of the roots; and, to set aside the difficulty that would arise if the two root-consonants were of the same

* He at first assumed a fourth mode, by simple strengthening of the root-vowel, as *kān* from *kan*, but afterwards abandoned this view and regarded *kān* as = *kawan*; see his *Hebräisches Wurzelwörterbuch*, Preface, p. x.

† He reckons the original Hebrew roots to be about twenty-four in number, the collateral roots two hundred and ninety, and the perfect-stems one thousand eight hundred.

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organ of speech, he holds that such identity does not occur, any form in which it seems to occur arising invariably from reduplication and subsequent contraction.

Meier has much useful matter in his *Dictionary of Hebrew Verbal Roots*, but the vices of his system of analysis are obvious and striking. The fundamental and decisive objection to it is that the assumed reduplication of the root-letter does not in fact always carry with it the perfect notion of completedness; the infinitive and participle and notably the form called the "imperfect" frequently express the opposite idea, and yet all these forms contain the supposed reduplication. If the notion of completedness inheres in the form *katab* (reduplicated, according to Meier, from *kat*, so that *katat* = *katah* = *katab*), how can it have vanished from the form *yi.ktob*?* Moreover, Meier connects the completedness with past time. "The root," says he, "extends itself, as it were, into the past to indicate the perfect, and by this direction of growth (the reduplicated letter standing regularly at the beginning, that is, *behind*) sets forth very vividly, or, if you will, symbolically the conception of higher antiquity, of the having come into existence, that is, of completion." But this is directly in the teeth of Hebrew usage, according to which the idea of time does not enter into these verb-forms at all, perfect and imperfect being each used to express present, past, and future time; and the symbolic interpretation of the position of letters is, to say the least, very doubtful. Elsewhere in Hebrew the doubling of a letter, where it is clearly significant at all (that is, not merely euphonic) always expresses intensity, and there is no trace in the language of a properly inflectional function assigned to it. It might be anticipated that, in order to establish the fact that no root is composed of letters of the same organ and to account for the diverse forms of the reduplicated letter, it would be necessary to provide great latitude in the interchanges of consonants,

* Meier merely says (*ubi supra*. p. 14) that the true signification of the Imperfect (incompletedness) stands in contrast with the completedness of the Perfect. Comp. Grill in *Z. D. M. G.*, xxvii. 3, p. 440, sq.

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and Meier in fact allows interchange without regard to organ;* but his proof of these laws is based on and interwoven into his hypothesis of reduplication, and stands or falls with it. He has himself, in fact, partially abandoned this hypothesis in assigning modifications of meaning to the various forms of his reduplicated letter, and has thus approached a correcter view of stem-analysis. Stripped of its untenable perfect-feature,† his dictionary is a comparison of stems for the purpose of determining which letters are original and which merely modifying; but his method has often led him by wrong paths to incorrect results.

(2). The theory of the *composition* of roots or stems as an explanation of triliterals has no longer any supporters, but it is still employed to account for some quadriliterals and quinqueliterals (nouns). It is, however, so contradictory to Shemitic usage that it is not likely to be retained even for these. No example of real composition of substantive words, that is, where components are combined into a true unity of form and meaning, exists in Hebrew. In the nouns that are called compound (chiefly proper names) the components in all cases retain their full form (except the divine name Jahweh, which, from the frequency of its use, suffers abbreviation) and stand in simple syntactical relation to one another: either in a possessive relation (construct state), or in simple apposition, or in copulative relation (like Sanskrit *Dvandva*), or as subject and predicate of a complete sentence, or the first is a negative; in all cases the components are felt to be distinct, there is no such fusion as in the Indo-European languages. The older authors (as Schultens and Schröder)

* As the interchange of gutturals and labials, p. 230. This he supports by citing the Indo-European transition of *k* into *p* through a *kv* (*kp*), without, however, adducing any such intermediary *kp* in Shemitic.

† Meier rightly says (Preface, p. vi. sq.) that no one has yet pointed out what is the mark of completedness in the Hebrew ground-form (*katab*), and that Ewald's distinction of the verbal stem (*ktab*) from the nominal (*katb*) by the place of the vowel, is merely a description, not an explanation. In fact, Ewald's remark, even if correct, would account only for the verbal conception, not for the notion of completedness in the ground-form. The difference of meaning between *katab* and *yikbob* is probably largely due to *usus loquendi*.



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allowed themselves much latitude in combining triliterals diverse in meaning into quadriliterals with compound significations. From *paras* 'spread out' and *paraz* 'cut' Schröder gets *pirsez* 'spread out by cutting" (A B C + A B D = A B C D), from *kabal* 'fold' and *rabal* 'to be loose,' *kirbel* 'to loosen by folding' (A B C + D B C = A D B C). Joh. Simonis prefers to combine only synonymous stems, and explains *kirbel* as made up of *karab* and *kabal* 'to bind,' and *ritpes* from *ratab* and *tapas* 'to be sappy.' All such forms are now explained (in accordance with the principle of determination to be referred to below) as arising from the addition or insertion of formative letters. The same explanation may be offered in the case of certain quinqueliterals held by Gesenius and Ewald* to be true compounds or fusions, as for example, *salanan* (שלאנן), supposed to come from the fusion of *salah* and *saanan*, but more simply explained as made from the latter stem by the insertion of *l* (as Gesenius himself regards it in the *Lehrg.* II. 863). The other quinqueliterals no doubt originated in similar ways. It is not at all probable that *ẓefardē'* (צפרדע) 'a frog' means 'marsh-hopper' or 'marsh-croaker' (literally, 'hop-marsh,' 'croak-marsh'); it is rather a further formation from *ẓafar* 'to croak.' Practically, indeed, as Gesenius points out (*Lehrg.* II. 867) this process of fusion amounts merely to the insertion of an additional letter or letters into the stem; it is in form nothing but an awkward application of the theory of root-determinatives.

(3). A subdivision of the theory of composition is the *prepositional* hypothesis of Fürst and Franz Delitzsch,† which supposes that triliterals are made from biliterals by prefixed particles (in the manner of the Indo-European languages) so that we have only to strip away the prepositional *ba*, *da*, or *pa* to obtain the simple biliteral. The decisive objections to this hypothesis are fully stated by Pott, Renan, and Friedrich Delitzsch.‡ It is opposed by the analogy of the Shemitic

* Gesenius, *Lehrgebäude*, II. 866; Ewald, *Heb. Lehrbuch* (7th ed.), p. 278, sq.

† Delitzsch, *Jesurun;* Fürst's *Heb. Concordance*, Preface, p. 8.

‡ Pott, *Etymologische Forschungen*, II. 1. p. 92; Renan, *Hist. des Lang. Sémitiques*, p. 451; Friedrich Delitzsch, *Indogermanisch-Semitische Wurzelverwandschaft*, pp. 6, 7, 69.

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languages, which know nothing of true composition, and by the fact that the supposed prepositions have left no discernible trace in the signification of the verbs, are not now to be found in separate form in the language, and (as against the particular view of the authors) could not have existed in composition in a presumed primitive Shemitic-Indo-European stage of language. This hypothesis owed its birth to a determination to effect a comparison between the Shemitic and Indo-European families of languages, and is guided in its details by Indo-European analogies. It is silently dropped by Fürst in his Hebrew Lexicon, and is probably no longer held by Delitzsch.*

(4). The unsatisfactoriness of the hypotheses above described has gradually led Shemitic scholars to the conviction that the resolution of triliteral stems must be effected, if at all, by a theory of *affixes* (prefixes, interfixes, suffixes), formative letters with substantive, uninflectional powers. Among the phenomena of the Shemitic languages the derived stems seemed, indeed, to suggest a mode by which triliterals might have come from biliterals as quadriliterals (Hiphil, Niphal, etc.) and quinqueliterals (Pealal, etc.) have sprung from triliterals (so De Lagarde). These are indeed examples of stems formed by consonantal additions (initial, medial, and final) to other stems, but they are merely relational modifications of the

* The same theory of composition is held by Mr. F. J. Crawford in his *Horae Hebraicae*, London, 1868, and by M. E. de Campos-Leyza in his *Clef de l' Interprétation Hébraïque*, Bordeaux, 1872. These books show commendable industry and zeal, but unhappily exhibit a total lack of sympathy with the sound method of modern scientific etymology. One of them (that of Mr. Crawford) relies largely on Keltic, the other (that of M. Campos-Leyza) largely on Spanish to explain Hebrew words. Mr. Crawford even offers emendations of the text of the Psalms and other books of the Bible from the Irish and the Persian. Their etymological analyses and combinations, especially their free use of particles in composition are perfectly arbitrary and fanciful, and, notwithstanding the good intention of the authors, can serve only to confuse our knowledge of the language. Mr. Wale's *Book of Hebrew Roots*, London, 1876, deserves the severest condemnation as an utter misrepresentation of Hebrew under the form of a contribution to biblical studies. Its wild biblical exegesis is mixed with such etymologies as the following: *Ab* 'father' he holds to be composed of *aleph* 'the first' and *beth* 'in,' the father being 'the first in' the house in dignity, while *bar* 'son' is *beth* + *resh* 'in the head,' that is, 'in the father'; *Abba* he calls a "junction of Syriac and Greek"! Such a book is a disgrace to England and to the Nineteenth Century.

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ground-stem (expressing intensity, causation, reflectivity, reciprocity, and the like), and are so far wholly unlike the triliterals, in which the added element must be supposed to have a substantive or material force. Besides, while in the derived stems the formative letters employed are comparatively few (*n*, *t*, *s*, and perhaps *y*, *w*, *h*), all the letters of the alphabet seem to be used in the formation of triliterals from biliterals. And then the derived stems could not have originated till after the triliteral scheme of stems was fully elaborated by the language, since which time we have examples of quadriliterals formed by substantive additions to triliterals with a wholly different sense from that of the quadriliteral derived stems; from which it seems clear that the linguistic conceptions in the two classes are entirely different. Somewhat similar to this is the *nominal* theory of Ascoli, which supposes that the existing triliterals and quadriliterals are denominatives made from biliterals and triliterals by the addition of nominal terminations. But, though denominatives are not rare in the Shemitic languages, it is fatal to Ascoli's view that his supposed nominal terminations cannot be shown to exist now in the language, and that the verbs held to be thus formed do not show a corresponding modification of signification.

We are thus led to the opinion that the formative additions to which the triliterals owe their origin are not inflectional, and to seek some other account of them. That such formative additions do exist in the triliterals has long been believed. Jewish lexicographers of the twelfth century reduced all Hebrew verbs to biliterals, and early Arabic grammarians recognized groups of connected stems in which a common signification was attached to two letters. Forster in the sixteenth century endeavored to fix the meanings of Hebrew words by comparing those that had like consonants, and Neumann in the succeeding century derived the significations of words from the onomatopoetic signification of the separate letters.* J. D. Michaelis, Adelung, and W. von Humboldt speak of the great improbability of dissyllabic roots. The

* Gesen. *Lehrg.* I. 184. In his *Heb. Dictionary* Gesenius also confines himself almost entirely to onomatopoeia in his stem-analysis.

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numerous groups of stems related in form and meaning point to the obvious and now generally recognized fact of a common biliteral element in them, and it may be regarded as established that all Shemitic substantive stems (except the few multiliterals) are triliterals resting on biliterals.

Here it may be remarked that for the purposes of the lexicographer it is immaterial whether this primitive basal biliteral be regarded as having had a pronounceable form and a separate historical existence, or as having been a merely ideal, unpronounceable root on which the stems were mentally formed. The root has no separate existence for us; we have to do merely with the stem and can determine its meaning equally well from an ideal as from an objectively actual root. When we compare the group: *ḳaṭal* 'kill,' *ḳaṭaf* 'pluck,' *ḳaẓab* 'shear,' *ḳaẓah* 'destroy,' 'decide,' *ḳaẓaf* 'cut in pieces' (Arab.), *ḳaẓaẓ* 'trim,' all that we need is to determine the root that underlies these stems, its signification, and the modifications of meaning effected by the various formative additions. If we are warranted in holding that the root is *ḳaẓ* or *ḳaṭ* and that it means 'cut,' then we may regard *ḳaṭal* as $= ḳaṭ + l$, and may proceed to inquire into the force and origin of the *l* wholly without respect to the question whether there was ever any such actually existent word as *ḳaṭ*. But while the lexicographer need not concern himself with this question, it has some interest for the student of language, who desires to reproduce as far as possible the beginnings of speech. Why should the primitive Shemites not have actually spoken these monosyllabic roots? It is not sufficient to say, as Renan does, that the transition from the biliteral to the triliteral is inconceivable. It is difficult, perhaps, but not more difficult than the supposition of a people habitually framing dissyllables on ideally conceived monosyllables. Or, if it is said that a Shemitic root is a vowelless consonant-complex (as *kt*) expressing an abstract idea without relations to other things, and that it ceases to be a root as soon as it is provided with a vowel, and thus assumes a particular relation to other things*—it may be replied that it is quite possible to go

* See Dillmann, *Aeth. Gram.* p. 99 sq., who, after having said that there are "no true substantive roots in the existing language," somewhat inconsistently

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back to the ante-inflectional stage of the language and suppose a word *kat* expressing the idea of 'cutting' without relation to any person or thing, and without variation of vowel. Such words might easily form the materials of a spoken language (as is substantially the case in the Chinese now), and might then by some process be enlarged into triliterals. There is no satisfactory reason why we should not suppose that the roots were actually pronounced, and indeed, as Philippi* remarks, we seem to have in the quadriliteral of the form *gargar* an actual primitive monosyllable, consonants and vowel. It is possible that there was no difference of vowels in the primitive roots, in which case the vowel would be *ă*.† In any case the form *kat* is the basis of all existing triliterals of the forms *katab*, *katib*, *katub*.

The first attempts made in the present century to account for the development of *katab* out of *kat* proceeded in a purely mechanical way; it was supposed that the third consonant was the hardening of a root-vowel, this process being first applied in the case of the weak stems and then extended by analogy to the others. Gesenius in 1817 suggested (*Lehrg.*, I. 185) that the introduction of this law of triliteralism was synchronous with the introduction of Shemitic writing, the vowel-letters (consonants) having been first employed to indicate the vowel-sounds, and then endowed with independent consonantal power. Thus the triliterals with weak initial, medial, and final letters might have originated, and by grammatical reflection the literary leaders in early times, the priests, might have made the law universal. Though the alphabetic side of this hypothesis, which was based on a misconception of the historical origin of Shemitic writing,‡

adds that "the *only easily* pronounceable roots are those that have a vowel for their second sound, as *mut*."

* *Ubi supra*, p. 96.

† Comp. Grill in *Z. D. M. G.*, XXVII. 3, p. 449.

‡ Gesenius supposed the Shemitic writing to be Aramaic and Babylonian in origin. In fact no Shemitic people, as far as is now known, has originated an alphabet, and triliteralism was already developed at a time when there is no reason to suppose that writing was practiced by the Shemitic race. Or, if they had writing, we know nothing of its nature. Fürst seems to proceed on this alphabetical view in his *Lehrg. d. Aram. Spr.*, § 98.

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disappeared before a better acquaintance with the facts, the mechanical-phonetic side has been adopted and maintained so far as regards the weak stems by many later scholars, as Fürst and Dillmann. According to this view the weak stem-letters are nothing but the consonantization of original root-vowels initial, medial, and final.* Original roots are supposed to be of the forms *kat*, *kāt*, *kīt*, *kūt*, *akat*, *ikat*, *ukat*, whence come *katat*, *ka.at*, *kahat*, *kayat*, *kawat*, *hakat*, *yakat*, *wakat*, *katah*, *katay*, *kataw*, and, since the vowels may also pass into liquids, dentals, and sibilants, *karat* (by inserting *r*), *nakat*, *sakat*, *zakat*, *takat*.

This view supposes a scheme of radical vowels that fails to account for the phenomena of the existing language, and is in contradiction to its analogies. It has already been shown that the present forms of the concave or Middle Waw Verb cannot be satisfactorily explained as coming from a root of the form *kun*. But, if *kān* is from *kū.an* (as Arab. Pass. *kīn* from *kŭwin*?) and *sār* from *sī.ar*, we expect *gāl* from *gā.al*, instead of which, however, is found always *găal*.† The Jewish-Aramaic *āl* (עָאל) adduced by Fürst (*Lehrg.*, p. 87) is simply the full writing of the ordinary form (עָל) as if from a Middle Waw Verb, and not a mere collateral orthographic form of *ăl* (from עלל). The assumed initial vowel in other roots is opposed to the Shemitic usage according to which every syllable begins with a consonant. If we may suppose that this principle existed in the primitive language before the development of the triliteral stems, it will be impossible to accept a root of the form *akat*, *ikat* or *ukat* (*akt*, *ikt*, *ukt*). A *tră* might be supposed to come from *tar* by metathesis of the vowel, if we could suppose the Aramaic *tra* (תְּרַע) to be an original form; but, as we have seen above, *tra* must be considered a diminished form of an original *tara*, as other cases of double initial consonant are to be similarly explained. The existing stems with weak letter in the third place are: Arab. *kataw*, *katay*, *katiy*, *kata'*, Aram. *ktay*, *ktiy*, *kta'*. These forms cannot be explained as from roots with

* Fürst, *ubi supra*, p. 80, sqq.; Dillmann, *Aeth. Gram.* pp. 102–107.

† Philippi, *ubi supra*, p. 90.

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broad vowels, *kāt* (*ktā*), *kīt* (*ktī*), *kūt* (*ktū*), since we should then expect, for example, Arabic *katā'* from *ktā*, whereas the actual form is *kata'*; and if the root be assumed to be *kăt* with short vowel, the supposition that *kataw*, for instance, (or שמע) owes its origin to the addition of a vowel is an unsupported assumption,* since the phenomena may all be explained by supposing the final letter to be a consonant. The phonetic theory, therefore, does not account for the phenomena of the weak stems, and does not offer any explanation of this supposed phonetic expansion except an unexplained tendency to triliterality in the Shemitic family of languages. We should have to imagine a phonetic euphonic impulse towards triconsonantism that embodied itself first in the resolution and hardening of vowels, and then, having thus created a type for the stems, extended itself so as to employ all consonants as affixes. Such a view is too vague to be accepted as an explanation of the present form of Shemitic verbal stems. A disposition towards consonantal extension is no doubt to be assumed, since that is what has actually taken place; but the impulse to such extension is rather to be found in some felt need of further material for the expression of ideas.

If, then, it may be regarded as altogether probable that all existing Shemitic verbal stems have sprung from monosyllabic biliterals, but not by resolution of root-vowels or by the addition of inflectional elements, it follows that in every triliteral there is one letter that may be regarded as substantively formative, as modifying the signification of the root by its substantive force. The fixing of the value of this modifying letter or *root-determinative*† is the first task of the Hebrew etymologist, and it can be accomplished only by the widest comparison of groups of related stems. In such comparisons several questions arise, on the answers to which will depend the method and results of the investigation.

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* Comp. Grill, *ubi supra*, p. 431.

† This term is proposed by Curtius in his *Grundzüge d. Griech. Etymologie*, p. 70, and adopted by Delitzsch and Philippi. It appropriately implies that the letter it designates defines the root, fixes its boundaries, and so converts it into a stem and a word.

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In the first place, is there ground for making any distinction between weak and firm letters in their capacity to act as root-determinatives and in their modifying power, or are they to be regarded as equally endowed with modifying force? It has been shown above that the weak letters are not mere phonetic expansions, but true consonantal additions to the root, and therefore presumably significant. Examination of groups of stems differing from one another by a weak letter shows that the modifying effect is as great in such cases as where the determinative is firm. In the groups formed on *ḳaṭ* and *ḳaẓ*, as *ḳaṭaṭ* 'fail,' *ḳaṭal* 'kill,' *ḳaṭon* 'be small,' *ḳaṭaf* 'pluck', *ḳaẓab* 'shear,' *ḳaẓah* 'decide,' Piel 'diminish,' *ḳaẓa*, Hiphil 'scrape,' *ḳaẓaẓ* 'trim,' there is as much distinctness and differentiation in the weak stems as in the others. The groups formed on *sar* (Arabic), as *saraha* 'to send or go away free,' *sarua* 'to go rapidly,' *sartaa* 'to run rapidly,' *sara* (*sarawa*) 'to remove, lay away,' *sara* (*saraya*) 'to make a journey by night,' *sāra* 'to go up or against,' *sara* (Mid. Ya) 'to depart,' *ṣarafa* 'to turn, repel,' *ṣāra* 'to turn, separate, collect,' show equal individuality in the weak letters. This is so generally the case that we may conclude that the weak consonants are as really determinative as the firm. Some groups of stems, it is true, differing only by weak letters, show little or no difference of meaning in a given dialect, but this is true of firm stems also; commonly the other dialects in such cases show marked differences between the stems. It is also to be borne in mind that a weak letter may be the diminished form of an original firm letter, and in general that weak letters may be radical as well as determinative, as is obviously the case in doubly and triply weak stems, as *hayah*, *awah*.

In respect to the *position* of the determinative there can be no doubt that the weak determinatives (Aleph, He, Ayin, Waw, Yod) may stand in any place, as in the stems *ṣar* (*ṣawar*), Arabic *sara* (*sarawa*), *aṣaf*, *yaṣaf*, *ṣafah*, *baar*, *bar*, *bara*, *barah*, and that *n* may be initial or final, as in *nabal*, *abal*, *balah* (root *bal*), *ṭaman*, *haṭam* (root *ṭam*). Whether the firm letters may be initial and medial is to be determined



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from examination of the stems, and is yet an open question. The liquids *l* and *r* are inserted after the first radical of a triliteral to form a quadriliteral, as *sarbit* from *sebet*, and Gesenius* compares such forms as the synonymous *daras* and *dus* (*dawas*) as showing a softening of the liquid into a vowel; but it is possible that we have here two distinct roots *dar* and *das*, as in *darak* (from *dar*) = *dakak* (from *dak*). Whether *s* and *t* prefixed to triliterals to form quadriliterals are root-determinatives or inflectional prefixes is not always clear, and it must be left to further investigation to determine whether these and other firm letters are ever prefixed to biliterals in order to form triliterals.

The laws of interchange of consonants have not yet been determined with exactness, and yet it is obvious that this must be done before the widest comparison of stems and roots can be undertaken. Interchange between letters of the same organ of speech may be safely assumed, and, on the other hand, the entirely unrestricted interchange that is employed by some writers is clearly unwarranted,† but between these points there is still room for much arbitrariness. The investigation of the laws of letter-interchange must go hand in hand with the investigation of the fundamental significations of stems, in order in this way to discover the form and meaning of the roots. It is too obvious to need mentioning, that the meanings of stems must be sought by the widest possible comparison in all the Shemitic languages; yet we find that often resort is had to some one language (most commonly the Arabic) that is supposed to preserve the original meaning. When we consider the imperfections of our lexicons, the numerous gradations in the forms and meanings of the stems (so that different groups sometimes seem to run into one another, one giving one root and another

* Heb. Gram., § 30.

† Meier's license in this respect has already been referred to. Another example of similar license is found in Dr. Davies's Heb. Lexicon, in other respects a very judicious and useful manual. He overlooks the fact that the occurrence of two letters in the third place in two synonymous triliterals (as *katab, katal*) does not prove interchange between them, since they may be independent root-determinatives.

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a different one for the same stem) and the possible changes in the past that the stems and roots have undergone, we can hardly feel ourselves justified in saying that "all Hebrew roots may be clearly determined and their significations fixed with precision,"* though such a result would undoubtedly throw light on the meaning of the Hebrew writings. Perhaps the most pressing need of Hebrew etymology at this time is an investigation of the significations of the stems.

A few classes of stems indicate their roots by their form: (1) quadriliterals of the form *gal.gal*, in which there is almost certainly a reduplication of the biliteral; (2) triliterals of the form *gal.al*, made by reduplication of the second root-letter, in which we can in many cases perceive a notion of intensity, as in Arabic *darra* 'to emit copiously, go rapidly or abundantly,' Hebrew *gazaz*, Niph. 'to be cut off entirely, extirpated,' *hasar*, Niph. 'to be dried up,' *daḳaḳ* 'to pound to pieces,' *hatat* 'to be thoroughly dismayed,' *ḳalal* 'to be slight', Piel 'to curse, blaspheme,' *talal*, Hiph. 'to deceive,' Aeth. *nababa* 'to speak much, be loquacious,' Syr. *h^e^nan* 'to pity, pardon,' *h^e^mam* 'to burn with anger or lust.' The idea of intensity seems to be attained here by the addition of a substantive element of the root, while in the intensive derived stems (as Piel) the same result is produced by the phonetic process of reduplication of a letter. Of these two classes (*palal* and *palpal*) there are in Hebrew about one hundred and fifty stems, including nouns; (3) triliterals of the form *palap* with first and third letters the same, which probably come from quadriliterals of the form *palpal* by dropping the fourth letter (as *saras* from *sar.sar*), and triliterals of the form *papal* out of *palpal* (as *babal* from *bal.bal*);† (4) verbs in which two firm letters are combined with medial Waw, Yod, Aleph, or He or initial Aleph, the root in such cases being found in the firm letters.

Other stems (with firm letters, or with initial or final Nun, He, and probably Waw, Yod, or with medial and final Ayin) do not indicate their roots by their form; they must in each case be subjected to special examination.

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* Fürst, *Handwörterbuch*, Preface, p. viii.

† In Aethiopic these forms all have the second letter doubled; see Dillmann's *Aeth. Gram.*, p. 108.

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So little has been done towards fixing the meaning and origin of the root-determinatives that it is unnecessary to say more than that recent writers are disposed to find in them pronominal forms. Their origin must for a long time yet remain a mystery. The contribution of the etymologist to the lexicographer will not, indeed, be complete till he shall have fixed the meanings not only of the primitive biliterals but also of the modifying additions, and we are yet far from this point. But Shemitic etymology, while its results are still comparatively meagre, is now, we may hope, on the right path, and needs only time to rival its sister science of Indo-European etymology in the extent and accuracy of its achievements. In order that this may be accomplished the Shemitic dialects must be thoroughly studied and the laws of their stem-formation determined, and then the nearest lying group of languages (the Egyptian), after a like office has been performed for it, may be brought into comparison with the Shemitic, in order thus gradually to approach nearer to the original forms of these tongues, and so to bring them into relation, if possible, with more remote families of languages.

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## V.—*A Botanico-Philological Problem.*

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A noted writer on language, F. Max Müller, in the course of his researches among the facts summarized in Grimm's Law, brought to light, some years ago, a curious parallelism between certain botanical and certain philological phenomena.

He first observed that the Germanic words *bōka*, *buche*, *beech*, and their like, agree in meaning as well as in form with the Latin *fāgus;* while, on the other hand, the same word in Greek, φηγός, signifies 'oak.' He then further found that our *fir*—and the German *föhre* (O. H. G. *foraha*), which has the same sense—is to be regarded as historically identical with the Latin *quercus*, 'oak.' Here, accordingly, if the Greek value of the former word, and the Latin of the latter, be assumed to be the more original, there are evident signs of a transfer of meaning in certain European vocables, from 'oak' to 'beech' and from 'fir' to 'oak,' respectively. This is the philological side.

With these facts in his mind, the same scholar, turning over Lyell's "Antiquity of Man," chanced upon a passage which showed that in the lowest strata of certain peat-bogs in the Danish islands the remains of tree-growth are prevailingly fir, while in the central strata they are oak, and in the upper strata beech; the whole region showing now, and having shown ever since the Roman conquest, such a marked proclivity to beeches that oaks are uncommon, and firs can hardly be made to grow there. Thus there has been, clearly and incontestably, a change in the arborescent vegetation of the region, from fir to oak, and again from oak to beech. This is the botanical side.

The analogy between the two sets of facts thus stated is a palpable one. After setting them forth, then (Lectures on Language, American edition, ii. 238 *et seq.*), Mr. Müller proceeds to intimate an actual historical connection between them: the "changes of meaning," he thinks, may have been

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"as the shadows cast on language by passing events." This poetical and somewhat ambiguous phrase he later puts into more definite shape: "The Aryan tribes, all speaking dialects of one and the same language, who came to settle in Europe during the fir period, or the stone age, would naturally have known the fir-tree only;" and the old name of the fir could not well have been changed to mean 'oak' "unless the dialect to which it belonged had been living at a time when the fir vegetation was gradually replaced by an oak vegetation" (pp. 250–1). And again: "I venture to suggest that Teutonic and Italic Aryans witnessed the transition of the oak-period into the beech period, of the bronze age into the iron age, and that while the Greeks retained *phēgos* in its original sense, the Teutonic and Italian colonists transferred the name, as an appellative, to the new forests that were springing up in their wild homes" (p. 252).

This is Müller's theory, in its simple outlines. As to the cautions and reservations which he hangs about it, and the (supposed) analogous facts which he brings up from other quarters in its support, we have for the moment nothing to do with them; we will rather turn our attention first to the acceptableness of the hypothesis considered in itself, taken on its own merits.

In the first place, there seem to be *à priori* difficulties in the way of establishing a cause-and-effect relation between the botanical changes and the linguistic. It is sufficiently obvious, and distinctly pointed out by Müller's quoted authorities, that a complete change of the prevailing tree-growth, from one species to another, would necessarily require many centuries for its accomplishment. There would be, for example, a number of successive generations in whose sight the oaks would be slowly gaining on the firs; other generations before whom the two would be about equally numerous; and yet other generations which would witness the victory of the oak and almost extinction of the fir. How, in this process, should it ever come about that the name of the tree originally prevalent should come to be applied to the tree finally prevalent? If the oak had no name of its own at the outset, during

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its period of rarity and inconspicuousness, it would gain one, alongside the fir, as it rose to rivalry with the latter; and if, as the fir was reduced to unimportance, any name died out, it would naturally be the old name of the fir, and not that of the conquering oak. The fact that, in times long out of memory, the fir had been predominant and had borne a certain title, would not furnish the dimmest shadow of a ground for giving the same title to the oak, in the day when it predominated. The probability is so overwhelmingly against any such transfer, that we have a right to refuse except to the most direct and cogent evidence our acceptance of a theory implying a causative connection between the supplanting of one tree by another and the conversion of the name of the former into an appellation for the latter: there are many ways in which words arrive at new meanings, but this certainly is not one of them.

If, indeed, the people who witnessed the double process of supplanting never had any specific names for different kinds of tree, but only one word for 'tree' in general, this word would of course have been applied to the fir alone in the period of exclusive fir-growth; in that of mingled firs and oaks, it would have belonged to both; when the oaks reigned alone, it would have designated only the oak—and so on: becoming finally the title of the beech, when that tree had come to the throne and exterminated all its rivals. But the linguistic facts are far enough from being what this theory would demand. To satisfy its requirements, we ought to find the Latin and Germanic peoples in possession of only a single tree-name. And this name should not only have changed its meaning from 'oak' to 'beech,' but it should show signs of having, at a yet earlier period, signified 'fir.' Unfortunately, that is not the case. In the great mass of the Germanic dialects, for example, it is only the 'oak'-name that has been changed into a 'beech'-name; they still possess the word which Müller assumes to have meant 'fir' from the beginning, and they still use it to mean 'fir.' How is this to be explained? Shall we say that the Germanic tribes in general did not witness the supplanting of the firs by the oaks, but only that

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of the oaks by the beeches, reaching Europe after the former part of the process had been accomplished? But if it had been accomplished, and so effectually that the people who watched it as it went on had been compelled to turn their superfluous 'fir'-name into an 'oak'-name, these Germans had no business to bring in the old 'fir'-name, and cling to it so obstinately, since they could have nothing to which they should apply it.

Or take, again, the case of the Italians. They (with a small and more questionable body of Germans) constitute the main-stay of Mr. Müller's theory; for they have both changed their 'fir'-name into an 'oak'-name (*quercus*), and their 'oak'-name into a 'beech'-name (*fāgus*); if anybody in the world sat by and saw the whole drama of transmutation, in both its acts, they are certainly the men. But, judging from their linguistic effect, the two acts must have been going on at the same time, independently, and each with a permanent result; for the change of meaning has taken place in two different words, and both are left in the language. If there were not firs and oaks together in primitive Europe, how should these "Aryan tribes, all speaking dialects of the same language," have had both the original 'fir'-name *quercus*, which they should proceed to convert into a name for 'oak,' and the original 'oak'-name *fāgus*, which they should proceed to convert into a name for 'beech'? If the meaning of 'oak' came into *quercus* because the oaks had come in and crowded out the firs, why did not the meaning of 'beech' afterward come into it, when the oaks in their turn gave way to the beeches? If the meaning of 'beech' came into *fāgus* because the beeches had come in and crowded out the oaks, why should not the meaning of 'oak' have come into it in a similar manner, as a result of the displacement of the firs by the oaks? And why should these two corresponding processes have gone on after such a different fashion, the one of them stopping in the middle, and the other, so far as our knowledge goes, beginning at the middle?

There are other equally puzzling questions suggested by the attempt to reconcile and combine, according to Müller's

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theory, the two bodies of facts with which we are dealing; but it is needless to ask them; for we are already in a snarl of difficulties which there seems to be but one way of unraveling. It is this. There was not, as Müller supposes, a double process of displacement and substitution; there were, rather, two independent processes of actual conversion, or transmutation, or transubstantiation. The two did, indeed, take place successively in the Danish isles, and this has led to Müller's slight misapprehension. But in most of Germany only one of them occurred; the firs remained firs, while the oaks were changed into beeches—with an accompanying conversion of the name φηγός, 'oak,' into *fāgus*, 'beech.' In Italy, again, both processes went on to their complete result, and simultaneously: all the firs turned into oaks, and all the original oaks turned into beeches—each species, notwithstanding its changed identity, retaining its old name. If Müller can bring himself to accept this slight modification of his ingenious theory, then the second principal class of obstacles in the way of its success will have been surmounted.

If, however, quitting this line of examination, we try another, we shall encounter another line of obstacles, not less formidable: probably it has already risen before the minds of many or of all who have attended to this exposition. Is it indeed true that the Danish peat-bogs are to be taken as furnishing in their successive layers decisive indications respecting the history of arborescent vegetation through the whole Germanic and Italic territory—not to speak of the rest of Europe? Have the oaks, as Mr. Müller appears to assume, everywhere driven out the firs? and have they been in their turn replaced by the beeches? Mr. Lyell says: "In the time of the Romans, the Danish isles were covered, as now, with magificent beech-forests," almost to the exclusion of any other tree-growth: is this the case with all the European territory, except Greece, occupied by our family of languages?

Doubtless there are many who will answer this question promptly and confidently in the negative, and who will even proceed to moralize on the theory which has called it forth. Here, they will admiringly say, we have one of those ideal



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philologists sometimes read of in story, but rarely met with in actual life: men of the closet, who are so absorbedly devoted to books and words that these have become to them the sole realities; who never lift their eyes to the nature which surrounds them; who care only for the distinctions of the vocabulary kingdom, and are blind to those of the animal, the vegetable, and the mineral. Müller is said to have grown up to adolescence in Germany, and he has probably at least travelled in Italy; yet, when he finds that *bōka* and *fāgus* have in these countries changed their meaning from 'oak' to 'beech,' he is at once convinced that the German and Italian oaks are, and of right ought to be, beeches. And these persons will probably be confirmed in their view of his personality by the way in which he expresses his willingness to put out the question to be decided by a competent scientific tribunal. "I must leave it," he says (p. 252), "to the geologist and botanist to determine . . . . whether the changes of vegetation, as described above, took place in the same rotation over the whole of Europe, or in the North only." It will be seen that, not trusting his own eyes, he is also very particular as to whose eyes shall be allowed to settle the question. Indeed, I have myself personally experienced that, to my cost. Being, as I fondly imagined, a little bit of a "geologist and botanist" in my own right, I ventured, in criticising Müller's theory eleven years ago, to offer my scientific testimony in opposition to it. I said that, instead of firs and oaks and beeches having supplanted and succeeded each other through the whole region occupied by the Germanic and Italic races, "we find all of them, or two of them, still growing peaceably together in many countries."* But Müller, in a reply recently made to my criticism,† being apparently unable to comprehend how one who concerns himself especially with language should presume to know anything on other subjects, fails to see that what I said was meant as testimony, and understands me as simply echoing his suggestion that a scientific oracle, if formally installed and duly inquired at,

* Oriental and Linguistic Studies, i. 257.

† Chips from a German Workshop (American edition), iv. 502.

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might settle the disputed matter. So he quells me with the remark: "Here Professor Whitney is, as usual, ploughing with my heifer;" and then, quoting his former words upon the subject, he goes on to say: "I had consulted several of my own geological friends, and they all told me that there was, as yet, no evidence in Central Europe and Italy of a succession of vegetation different from that in the North, and that, in the present state of geological science, they could say no more. In the absence of evidence to the contrary, I said, Let us wait and see; Professor Whitney says, Don't wait."

Yes, certainly I say, "Don't wait"; a single moment's unnecessary delay is to be deprecated. How can we sit down and fold our hands over a question that affects the weal and woe of so large and respectable a part of mankind? If Central Europe and Italy are really covered, exclusively or prevailingly, with "magnificent beech-forests," then there are a great many millions of people, there and elsewhere, whose mental delusions render it unsafe to trust them any longer outside of an insane asylum. In order to do what I can toward determining their condition and fate, I will follow Müller's own example. He has gathered a set of twenty "principal bones of contention" between himself and me, and challenged me to summon a commission of learned professors to deal with them. I will add to the heap one more bone (bigger and more solid than most of those raked together by him), as follows:

21. Whether Central Europe and Italy are covered with beech forests, to the exclusion, almost or quite, of other trees, especially of oaks and firs.

And I appeal to him, in my turn, "to choose from among his best friends three who are *professores ordinarii* [of natural science] in any university of England, France, Germany, or Italy; and by their verdict I promise to abide" (Chips, iv. 528). I do not feel willing to accept the outcome of his private and confidential consultations with his "own geological friends," as reported by himself, because it has appeared more than once, in connection with other subjects, that those consultations do not yield unexceptionable results. He declares himself (Chips, iv. 498–9) to have been guided in all his

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phonetic investigations, and controlled in their conclusions, by the advice and approval of the highest authorities in physiology and acoustics; yet the influence of Helmholtz and Ellis could not prevent his declaring his independent opinion (Lectures, 6th [English] edition, ii. 133) that the "neutral vowel," the *u* of *but* or *up*, is a non-sonant or whispered element; even as the aid of Main and Hinds could not keep him, in his astronomical reasonings, from assuming (preface to Rig-Veda, vol. iv., p. lii.) that, to any given observer, the ecliptic is identical with his own horizon.

It ought to be added, perhaps, that there seems to be another method in which this particular "bone of contention" can be ground up and gotten completely out of the way, without summoning an International High-Joint Commission to chew upon it; and we have reason to wonder that that method did not suggest itself to Müller: for it is a linguistic one. We cannot question that he is familiar with the principle, generally accepted among philologists, that the presence in a language of a certain name implies the presence in the minds of the people speaking that language of some knowledge respecting the thing named; indeed, he himself, in the course of his illustrations, frequently applies or implies it. If we could, for instance, catch an untaught and untravelled savage off one of those Danish islands, with their exclusive growth of magnificent beeches, we should find that he had no names for 'oak' and 'fir,' any more than for 'mahogany' and 'palm.' With this test in our minds, let us examine the various Germanic and Italican dialects; if they contain words for 'fir' and for 'oak,' as well as for 'beech,' then we, as linguists, shall have the right to hold, and to maintain before all the world, that in Central Europe and Italy the beeches have not crowded out the oaks, which had before crowded out the firs. And this, even though the decision of the scientific triumvirate should be adverse to us. For if, in the very passage under discussion, Müller teaches us (Lectures, ii. 252), in case of an apparent discordance between linguistic and craniological evidences, to "protest that the Science of Language has nothing to do with skulls," we should be

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justified *à fortiori* in maintaining that she is above having anything to do with trees. As for the presence in the same European tongue of words for both 'fir' and 'oak,' besides 'beech,' we have already above seen some of the facts bearing upon the matter; it may be commended to Müller for a more careful and exhaustive examination, with the hope that we shall find the results in a later edition either of the "Lectures" or of the "Chips."

Pending their appearance, we may regard the discussion of Müller's theory as brought to an end; and it does not appear doubtful with what conclusion: no theory can stand for a moment which has so many and so various and so powerful objections arrayable and arrayed against it. We ought not indeed, to leave out of sight the modestly hypothetical tone in which its author originally put it forward, adducing against it more than one consideration which, if he had taken the trouble carefully to weigh them, he would have seen to be fatal to it; and ending his whole exposition thus: "I shall be as much pleased to see my hypothesis refuted as to see it confirmed; all that I request for it is an impartial examination." But then he has, after all, written it down and put it forth, for the examination and criticism of scholars; and he must accordingly be held responsible for its character, and has no right to complain if it is treated just as its own intrinsic merits deserve. If he were also to suggest, as a hypothesis, for the discussion of comparative philologists, that a real analogy existed between Grimm's Law of rotation of the three classes of mutes and the nomenclature of these three trees; that the original names for 'fir,' 'oak,' and 'beech' had each been pushed one step around in the series; that, while *quercus* had changed from 'fir' to 'oak,' and *fāgus* from 'oak' to 'beech,' *abies* had also (though, owing to the loss of needed evidence, we could not so clearly prove it) certainly changed from 'beech' to 'fir'—then, with however many *ifs* and *provideds* he might season his exposition, whatever gratitude he might promise to the man who should convince him that his notion was a foolish one, its folly would remain incontestable, and he would deserve to be well laughed at for ever having

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confessed to entertaining it seriously. Nor would the case be different if he were to put forward, with ever so many allowances that he was perhaps mistaken, the theory that the gradual contraction of the earth's crust had something to do with the universal abbreviation of the vocabular elements of speech: that the latter was, as it were, "the shadow cast on language" by that great event as it passed. Now Müller's modestly indicated hypothesis is just as unreasonable, just as inherently absurd, as either of those others would be; it differs from them only in that its absurdity does not lie so openly and palpably upon its very face. It is one of those queer imitations of a correspondence which now and then call a smile to a man's countenance when he chances upon them over his table; or with the recital of which he, at the utmost, amuses a friend in the course of an evening's walk. To devote to its exposition and support fifteen pages of a treatise on a new "Science," as Müller has done, is to make out of a joke a far too serious matter.

But Müller has done more than this. In a criticism of his volume, published eleven years ago,* I devoted two pages to a complete statement and refutation of the theory; bringing against it, in effect, nearly the same objections which have been here made, though otherwise cast, and in a much briefer form. I had given it, as I knew, a wholly "impartial examination;" and I conceived myself to be fairly earning an expression of the "pleasure" with which its author had promised to greet its refutation. Doubtless this was too sanguine; it might have been suspected that one who could frame and publish such a hypothesis would not be easily accessible to any brief and summary demolition of it; that he might probably enough even show toward it that instinctive special affection which mothers are said sometimes to feel for their weakest and least creditable offspring. At any rate, last year, in an article† which proves him not to have been in a state of mind to profit by any correction of mine, however honestly meant and faithfully administered, he makes what

* North American Review, vol. c., 1865; also Or. and Ling. Studies, i. 239–62.

† Chips, iv. 456 *et seq.*

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he intends as a stout defense of his bantling. His answer to one of my three main points of objection I have already quoted in full: I had been ploughing with his heifer, and urging the poor beast forward with unseemly haste. *En revanche*, he endeavors, in replying to another point, to impale me upon the horns of a heifer of my own. I have, he says, "unintentionally" offered him the "best illustration" of just such a change of meaning as he assumes in his tree-names, by showing* how the English settlers in America applied the old familiar names of *robin* and *blackbird* to new kinds of bird, somewhat resembling those which had borne them in England. But what is the analogy between this case and the one we have had in hand? Perhaps we have all along mistaken his meaning, and he has only intended to maintain that the Italican tribes migrated from a country where firs and oaks prevailed to one where, instead, only oaks and beeches were found, and therefore, having the 'fir' and 'oak' names idle on their hands, proceeded to apply the former to the oak and the latter to the beech, each being the species nearest like what the same word had before designated. This, and this only, would be a real parallelism. If we are to find one for the theory as originally put forward by Müller, we shall be obliged, it seems to me, to turn the American act of nomenclature the other way, and see the robin and blackbird "transferring the name of 'Indian,' as an appellative, to the new white people that were springing up in their wild homes."

It is needless to spend more time upon Müller's attempt to rehabilitate his hypothesis. About each item which he touches he raises a thin cloud of word-dust, just sufficient to obscure its outlines; showing an unclearness so curious and puzzling that one almost gives up in despair the endeavor to trace the mental state which it represents. Anything in the nature of a counter-argument no candid and competent person will pretend to discover. But he sings his little song of triumph at the end, and regards his fir-oak-beech theory as established more firmly than ever before the eyes of the students of language.

* Namely, in Or. and Ling. Studies, i. 303.

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The matter to which we have thus given a few minutes' attention is, in one respect, an almost purely personal one. The scientific question involved is of quite insignificant importance; and no man of real knowledge and penetration is likely to be so far carried off his feet by Müller's persuasive eloquence as to take his hypothesis for an acceptable explanation. The fact that the whole thing is so curiously characteristic of Müller, and that Müller's personality is an element of high importance in the prevailing currents of thought and opinion on a host of subjects, is what gives the subject a wider and impersonal bearing. He has a real genius for exposition and illustration; this very note, "On words for fir, oak, and beech," is full of interesting facts, interestingly grouped, and may be read with lively pleasure by any one who can leave out of sight for what they are marshalled and to what end directed. What its author lacks is inductive logic, the power of combining his facts aright and seeing what result they yield; his collected material dominates and confuses him; often he hits the truth, with a kind of power of genial insight; often he hits wrong, sometimes perversely and absurdly wrong. No man needs to be studied with a more constant and skeptical criticism; no man is less worthy of the blind admiration and confidence, resembling that of a sect in its prophet, with which he has now long been regarded by an immense public, and even by scholars of a certain grade. While he maintains this false position, his influence is harmful, obstructive to the cause of truth; to do anything toward reducing his authority to its true value is a service to truth and to sound science.

There is yet another personal aspect which the controversy bears, personal to both Müller and myself; and I cannot forbear spending a few words upon it in closing. Since, for the reasons just laid down, I have repeatedly controverted views and arguments of his which appeared to me to be false —and sometimes sharply, as I thought they deserved—it was obviously for his interest, in lack of any other more effective method of reply and defense, to represent me as a mere fault-finder and personal vilifier. This he accordingly did, at considerable length, in the Contemporary Review for January,

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1875. In the course of his article, he threw out against me this taunt: "He bitterly complains that those whom he reviles do not revile him again" (p. 314). In my answer, in the same Review (May, 1875; p. 728), I said: "If I stated that any one 'bitterly complained' that he was not answered by those he criticized, I should feel called upon to give chapter and verse for it; and neither Mr. Müller nor any one else can point out any such complaints on my part." It is in answer to the challenge here implied, to authenticate his charge, that Müller returns to the "words for fir, oak, and beech." The way of it is this.

In the preface to the sixth edition of his lectures (1871), Mr. Müller permitted himself a sneering reference to my criticism of an earlier edition, and a sophistical and untenable reply to one or two of the points which I had made against him. I therefore sent to the Review which had printed the original criticism (the North American) a rejoinder,* in which, after setting right the points referred to, and showing that the sneer was a gratuitous one, I went on thus: "We earnestly desire, and heartily invite, a continuation of his exposures. We should be glad, for example, to see him defend his explanation of the phenomena stated in Grimm's Law. . . . We should like, again, to have him try to prove that any one of the three impossible assumptions which we pointed out as involved in his argument respecting the 'names for fir, oak, and beech' does not vitiate that argument"—and so on. This challenge, now, this invitation to go on and set up again, if he could, the hypothesis which I claimed to have overthrown, is what he brings up, and the only thing that he brings up, to justify his former allegation that I had "bitterly complained" of not being reviled by those whom I had reviled! He does not, indeed, venture to repeat the allegation here in precisely the same terms (though he has reproduced it unaltered in his reprint of the Contemporary article in the "Chips," iv. 433); he speaks, rather, of "a passage where Professor Whitney expressed his dissatisfaction at not being answered, or, as I

* North American Review, vol. cxiii., 1871; reproduced in Or. and Ling. Studies, i. 262 *et seq.*



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had ventured to express it, considering the general style of his criticism, when he is angry that those whom he abuses, do not abuse him in turn."* Then, in his summing up at the end, he asks, with all the conscious dignity of re-established blamelessness:† "Was I, then, so far wrong when I said that Professor Whitney cannot understand how anybody could leave what he is pleased to call his arguments, unheeded?" We may ask in return, was there ever seen a more beautiful instance of the *diminuendo* in accusation? The documentary and tangible "bitter complaint at not being reviled" first becomes a purely inferential "anger at not being abused"—incapable of absolute refutation, because the accuser, even when driven into the last corner, can still say: "Oh, I am quite sure that he was angry, though he did not show it;" then this is confessed to be a mere adventurous dysphemism for what, when strictly defined, is only a "dissatisfaction at not being answered;" and the last finally dwindles to an intellectual "failure to understand it that one's arguments are unheeded." Surely, the mutual interchanges of oaks and firs and beeches are nothing to this; it could only be paralleled by the transmutation of oaks into alders and of alders into bramble-bushes. In its last form, moreover, the statement, though less widely remote from truth, is not less strictly erroneous, than in its first; for I have not, in fact, ever been at a loss to understand why Müller left my arguments against his views so long unanswered; any more than why, when he finally attempted to answer them, he found nothing to use against them save evasion, misrepresentation, and detraction.‡

* Chips, iv. 500.

† Chips, iv. 504.

‡ The character of his charges in reference to points of Sanskrit grammar I have briefly exposed in the Proceedings of the American Oriental Society for May, 1876.

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## VI.—*'Shall' and 'Should' in Protasis, and their Greek Equivalents.*

By W. W. GOODWIN,

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Professor Sewall, in his paper "On the Distinction between the Subjunctive and Optative Modes in Greek Conditional Sentences" in the 'Transactions' for 1874, and Professor Morris, in his paper "On Some Forms of Greek Conditional Sentences" in the 'Transactions' for 1875, have criticised especially that part of my classification of conditional sentences in which I maintain that the optative in ordinary protasis (i. e. in all conditions in which it does not express a past general supposition) is "merely a vaguer or less vivid form than the subjunctive for stating a future supposition." In opposition to this view, they agree in maintaining *some* form (though not quite agreed upon the precise form) of the distinction commonly made between the two moods in protasis, based upon the greater or less *possibility* or *probability* that is implied, or upon the presence or absence of an *expectation* or *anticipation* of the fulfilment of the condition. Professor Morris further suggests an important limitation to the use of the subjunctive in protasis, by expressing "a strong opinion that no case can be adduced from the best writers in which a future supposition demanding for its fulfilment *a violation of physical laws* is expressed by ἐάν with the subjunctive."

These attacks are both directed against what I have always felt to be the weakest point in my classification, a point on which I am myself a convert from the doctrines of my opponents. And although I find myself now unable to see the distinction which I once thought I could see, and which most scholars still think they can see, between ἐὰν γένηται and εἰ γένοιτο, I am by no means disposed to be intolerant toward those who are of a different opinion. One gain has thus far come from the discussion—the clearer statement of one important point in the controversy; for I understand it to be generally admitted that the difference between ἐάν with the

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subjunctive and εἰ with the optative is essentially the same as that between 'if he shall' and 'if he should' in English, and that if we can determine the principle that underlies the latter construction, we have the key to the former. The use which all scholars constantly make of these English forms to translate and explain the Greek constructions in question, whatever may be their theories of the latter, shows the general feeling on this point. If this is once admitted, it will aid us greatly in understanding the Greek form of protasis to ask ourselves what distinction we are in the habit of making between 'if he shall go' and 'if he should go' in English. But here unfortunately we meet an obstacle. The modern English, in which we think and express our thoughts, has substituted for the future form 'shall' in protasis the colorless present, so that we now say 'if he goes,' 'if he reads,' etc., for the more exact 'if he shall go' (or 'if he go'), 'if he shall read' (or 'if he read'), etc., which the translators of the Bible in the seventeenth century would have used. Further, this same present form, though we seldom use it to express a purely present condition (for which we should generally say 'if he is reading,' not 'if he reads'), is yet constantly used in general present conditions like 'if any one (ever) reads,' which are entirely distinct from the future conditions we are considering. We must therefore confine ourselves to cases in which we use either 'if he goes,' etc., in the sense of 'if he shall go,' etc., or the latter form itself. Fortunately the translation of the Bible makes every English scholar familiar with the older and more exact form, even if he never uses it in speaking or writing.

It will be understood that, when I compare the optative with the subjunctive in conditions in this paper, I shall confine myself to the optative in its fixed usage in Attic prose, excluding, for example, all notice of the present optative in Homer used to express a present unfulfilled condition, like the imperfect indicative in Attic Greek.

Professor Morris very properly asks for a more exact definition of the term 'vividness,' which I use in stating the distinction between the subjunctive and optative in protasis.

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I have generally called the statement of a future condition which is made by the subjunctive, corresponding to 'if he shall go' (or 'if he go') in English, "more distinct and vivid" than that which is made by the optative, corresponding to 'if he should go.' By this I mean that the picture (so to speak) of the event or the circumstances supposed which is presented to the mind when the former expression is used is a "more distinct and vivid" one, a picture with outlines more sharply defined and more distinct and definite in its whole conception, than that which the latter form presents. On the other hand, as it seems to me, when the optative form is used, i. e. when we state a supposition in the form 'if he should,' the whole conception is vaguer and presents to the hearer a "less distinct and vivid" picture of the event supposed. For example, it seems to me that the supposition 'if some barbarian *shall* ever drag thee away weeping into slavery,' differs from 'if some barbarian *should* ever drag thee away weeping into slavery,' simply in this, that the former presents a more distinct and vivid conception of the event than the latter; and I do not believe that any one who had no theory of Greek syntax in view would ever think of distinguishing them by saying that one implies "an anticipation of the possible realization" of the supposition, while the other implies "an imagination of the possible realization" of the supposition; or that the one is "a supposition relating to contingent fact," while the other is "a supposition of conceived fact." I say merely that *it seems to me so;* and the more I think of the matter, the less I am able to see either of the last mentioned distinctions in the two expressions. If, however, others, when they use such expressions, feel that either of these distinctions is in their mind, it is impossible to appeal from this decision to any tribunal which will have higher authority with them. Let it be understood that I doubt whether any one who thought merely of the English expressions in question would ever make either of these distinctions between them *by his own suggestion;* I do not doubt that witnesses without number, if they were asked directly whether they *did not make* these distinctions, would

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testify that they did so; for where it is so difficult to state or conceive clearly a distinction in language, it is extremely easy to imagine it to be almost any one which is plausibly given on high authority. We need not go far from the subject now under discussion for striking examples of this tendency. How many have quietly and in perfect confidence assented to the doctrine that the subjunctive in final clauses after past tenses "brings the action of its verb down to the present time," although there is hardly a page of Thucydides which would not demonstrate its utter absurdity! It seems to me, further, that the distinction of the optative as a "less distinct and vivid" form of expression than the subjunctive and equivalent forms (e. g. the imperative) appears in most of the constructions which admit the optative. In independent sentences, compare *μὴ πάθητε*, DEM. Lept. § 50 (p. 472), with the common *μὴ πάθοιτε*, the former being *do not suffer*, the latter *may you not suffer*. The same may be seen in the Homeric use of the independent optative compared with the imperative; e. g. in *Ἑλένην Μενέλαος ἄγοιτο*, Il. iv. 19, and *γυναῖκά τε οἴκαδ' ἀγέσθω*, Il. iii. 72, the former being *may he carry*, the latter *let him carry*. In the double construction of oratio obliqua after past tenses, where an option is allowed between a subjunctive of the direct form and the same tense of the optative, the latter is evidently the weaker and less vivid form of expression, differing in no other respect from the former. The fact that two cases of this distinction have been inadvertently cited to illustrate the assumed distinction between the subjunctive and optative in protasis, although the direct forms in both cases would have been identical in construction, has been already used by me* to confirm my argument against admitting any other distinction in direct discourse than is generally allowed to exist in these indirect

* See the remarks in *Transactions* for 1873, p. 72, on *εἰ συμπείθοι* and *εἰ εἰσηγοῖτο* as compared with *ἐὰν αἱρεθῇ* and *ἐὰν ᾖ* in DEM. Cor. p. 276, §§ 147, 148, where the two optatives are due entirely to the oratio obliqua and represent *ἐὰν συμπείθῃ* and *ἐὰν εἰσηγῆται* of the direct discourse. And yet these are standing examples of the "essential and inherent distinction" between the subjunctive and the optative! See, for instance, Dissen's and Holmes's notes on the passage, and Kühner, § 576, Anm. 7

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quotations. The principles of oratio obliqua, as regards the choice of moods, apply, as I have already shown,* to the distinction between the subjunctive and optative in final and object clauses (with ἵνα, ὅπως, μή, etc.) after past tenses, to which I have referred above. These analogies drawn from the other uses of the optative have given a strong and (I may add) an unexpected confirmation to the opinion to which I was led originally by a consideration of the subjunctive and optative in protasis and relative clauses alone.

If now the distinction which I have tried to establish is the true one, the question recurs, when will a speaker naturally use the subjunctive and when the optative in stating a future condition? In most cases he will use the more vivid form to express a supposition which *for any reason* is more vividly conceived and so more prominent in his own mind, or one which he wishes to bring more distinctly before the mind of the hearer; and the less vivid form for one which for any reason is less prominent or which he wishes to present less distinctly. His choice, therefore, may be influenced by various considerations. He will naturally form a more vivid conception of a supposition which he thinks highly probable in its nature or likely to be realized in a particular case, or of one which he especially desires or especially dreads to have realized. He will naturally express with greater distinctness a supposition which he wishes to mark as especially absurd; or one which for any reason he wishes to make especially emphatic in comparison with others in the same sentence, whatever may be the nature of the supposition itself; while he will naturally express with less distinctness one which he wishes to make less emphatic. Cases in which the subjunctive and optative in protasis are brought into contrast in successive sentences are very rare, so that we can generally supply the alternative form only in imagination. It must be remembered too that neither the subjunctive nor the optative expresses any *absolute* amount of vividness or distinctness, still less any absolute amount of probability or desire; these qualities are merely relative, and are made

* See *Transactions* for 1873, p. 73.

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obvious chiefly by contrast. We must not be surprised, therefore, to find precisely the same supposition expressed in different forms by different persons who need not differ in their opinion of the nature of the supposition, or by the same person at different times without any necessity of a change of mind on his part: of this examples will be given below. Now if the distinction between the two moods were essential and fundamental, as Professor Sewall and Professor Morris believe it to be, it seems to me hardly possible that this variety of expression could be allowed: in that case, most conditions would fall by a fixed principle into one class or the other, and any change in the form would involve a grammatical error of the same nature (though of course not of the same degree) as that which an Athenian would have committed if he had said εἰ ἔλθοιμι in the sense of 'if I had gone.' There are some conditions, involving an extreme amount of absurdity or improbability, which would more naturally be stated by the optative alone unless special emphasis were intended; to this class belong most of the excellent and pertinent examples collected by Professor Morris,—*if the house should find a voice,—if the moon should never rise again,—if I should go on with my story for ten days,—if they should get a power like that of Gyges,—if a man should have three talents of gold in his stomach, one in his head, and a stater of gold in each eye.* In English as well as in Greek such conditions would in most cases be stated in the vaguest possible form, to correspond to the vagueness of such conceptions in the mind. But I hope to show below that all these conditions might under certain circumstances be stated in the more vivid form, without involving any grammatical or logical absurdity.

Before proceeding to state another consideration which often influenced the choice of mood in conditions, I will give examples of suppositions in which the choice of mood appears to be affected by one or more of the considerations already mentioned.

1. In Plato's Republic (vi. 494 B—E) we have a famous description of the career of a bright and handsome young

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man, of high birth and great wealth, exposed to the flattery and adulation of a populous city, and of the fate of any philosophic friend who may attempt to divert him into the path of wisdom. We should say that this might be selected as a strong case of "conceived fact" as opposed to "contingent fact," or of the "imagination" rather than the "anticipation" or "expectation" of the condition being realized. If this case had been supposed in the optative form, all would have called it a striking instance of a purely ideal supposition. But here it is plain that Plato had in mind the career of Alcibiades and the relation of the fast young Athenian to Socrates; and he adds a most striking dramatic effect to his sketch by making Socrates imagine the course of the young man in the more graphic and impressive form of supposition. I feel sure that Professor Morris will agree with me in saying that it was optional with Plato to give or withhold this artistic touch; and we shall agree in thinking that the use of the subjunctive makes the sketch more life-like and implies that it is less of a fancy sketch than the optative would have done. He will, however, maintain (I fear) that the "expectant" form, the subjunctive, implies *by its own nature* necessarily a looking forward to realization; while I hold that the "vivid and distinct" form can be used to express emphasis in many other ways, even when there is no thought of realization; as in I. Cor. xii. 15, 16: *ἐὰν εἴπῃ ὁ πούς, if the foot shall say; ἐὰν εἴπῃ τὸ οὖς, if the ear shall say;* or in Matth. xv. 14: *τυφλὸς δὲ τυφλὸν ἐὰν ὁδηγῇ, ἀμφότεροι εἰς βόθυνον πεσοῦνται, and if the blind lead the blind, both shall fall into the ditch.* Let us see what effect would be produced by a change of mood: e. g. by substituting *εἴ τις ἠρέμα προσελθὼν ἀληθῆ λέγοι, . . . ἆρ' εὐπετὲς ἂν οἴει εἶναι εἰσακοῦσαι*; for *ἐάν τις λέγῃ, κ. τ. λ.*, i. e. *if some one should go to him and tell him the truth*, for *if some one shall go to him and tell him the truth.* I can see in the former only the natural form of expression for such a supposition, which any of us would use in a similar case, and which any Greek would have used who had never known a career like the one supposed or who had no desire to make his sketch particularly impressive; in the present case, however, Plato wishes to



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paint as impressive a picture as he can of a most striking historical event, and he therefore uses a more vivid form of statement, precisely as an artist might have used brighter colors in a real picture for an analogous purpose. That Plato could not have felt that the optative form would have been absurd here appears plainly from a parallel passage (Rep. vii. 517 A), where Socrates is made to refer to himself quite as pointedly as before, under the character of the man who attempts to release the prisoners in the cave and to lead them up to the light, and whom they would kill *if they could in any way get him into their hands*, εἴ πως ἐν ταῖς χερσὶ δύναιντο λαβεῖν καὶ ἀποκτείνειν, ἀποκτείνειν ἄν. This could have been expressed by ἐάν πως δύνωνται ἀποκτενεῖν, i. e. *will they not kill him if they can?* If it had this form, it would be simply a more lively picture of the fate of Socrates than we now have, and this would explain (as in the other case) what might otherwise seem too distinct and vivid a statement of a condition which in itself seems eminently fitted for the other form of expression.

2. In Plat. Gorg. 521, 522, Socrates is represented as predicting his own trial and condemnation; and in contrast with this definite foreboding he supposes, merely for illustration, the case of a physician tried by a jury of boys with a pastry-cook as accuser. The outline of the construction is as follows (521 C—522 A): τόδε μέντοι εὖ οἶδ' ὅτι, ἐάνπερ εἰσίω εἰς δικαστήριον, πονηρός τίς με ἔσται ὁ εἰσάγων· καὶ οὐδέν γε ἄτοπον (sc. ἂν εἴη) εἰ ἀποθάνοιμι. . . . οὐχ ἕξω ὅ τι λέγω ἐν τῷ δικαστηρίῳ. κρινοῦμαι γὰρ ὡς ἐν παιδίοις ἰατρὸς ἂν κρίνοιτο κατηγοροῦντος ὀψοποιοῦ. σκόπει γὰρ, τί ἂν ἀπολογοῖτο ὁ τοιοῦτος ἄνθρωπος ἐν τούτοις ληφθείς, εἰ αὐτοῦ κατηγοροῖ τις λέγων ὅτι, κ. τ. λ. . . . τί ἂν οἴει ἔχειν εἰπεῖν; ἢ εἰ εἴποι τὴν ἀλήθειαν, ὁπόσον οἴει ἂν ἀναβοῆσαι τοὺς τοιούτους δικαστάς; Here we have a marked distinction between the more vivid form in which Socrates imagines himself brought before a court and the less vivid conception of the physician on his trial; and I believe that this distinction was based upon anticipation in one case and imagination in the other, a contrast in feeling which found its most natural expression in this contrast of forms. Further, I have no doubt that the change to εἰ ἀποθάνοιμι in the second supposition, where ἐὰν ἀποθάνω would certainly

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have been permitted, if not expected, indicates a less vivid anticipation of being condemned to death than of being brought to trial. Just below (522 B) he applies the comparison to his own case by an apodosis in the optative: *τοσοῦτον μέντοι καὶ ἐγὼ οἶδα ὅτι πάθος πάθοιμι ἂν εἰσελθὼν εἰς δικαστήριον.* But he returns immediately to the other form, with which he began: *οὔτε γὰρ ἡδονὰς... ἔξω λέγειν'... ἐάν τέ τίς με ἢ νεωτέρους φῇ διαφθείρειν... ἢ τοὺς πρεσβυτέρους κακηγορεῖν... οὔτε τὸ ἀληθὲς ἔξω εἰπεῖν... οὔτε ἄλλο οὐδέν· ὥστε ἴσως ὅ τι ἂν τύχω τοῦτο πείσομαι.* In replying to the next question of Callicles, however, Socrates uses the optative form in reference to the same subject, his anticipated trial: *εἰ μὲν οὖν ἐμέ τις ἐξελέγχοι ταύτην τὴν βοήθειαν ἀδύνατον ὄντα ἐμαυτῷ καὶ ἄλλῳ βοηθεῖν, αἰσχυνοίμην ἂν..., καὶ εἰ διὰ ταύτην τὴν ἀδυναμίαν ἀποθνήσκοιμι, ἀγανακτοίην ἄν· εἰ δὲ κολακικῆς ῥητορικῆς ἐνδείᾳ τελευτῴην ἔγωγε, εὖ οἶδα ὅτι ῥᾳδίως ἴδοις ἄν με φέροντα τὸν θάνατον.* All these conditions could have been expressed in the subjunctive form quite as properly as the earlier ones; indeed the last one, *εἰ δὲ... τελευτῴην* might naturally have had a subjunctive to express contrast with the preceding *εἰ μέν*, *κ. τ. λ.*, since one makes a supposition abhorrent to the speaker's feelings, which he regards as impossible, while the other refers to what actually took place and had already taken place when Plato wrote the words. It seems to me that no theory of the two forms of condition which assumes that there is in almost all cases a predetermined form in which alone a given future condition can be properly expressed can be applied consistently to these cases.

3. In PLAT. Phaedr. 259 A, Socrates imagines that the cicadae are watching his conversation with Phaedrus to see whether their chirping will lull the speakers into a noonday nap. He naturally hopes this will not be the case; and his change from the less vivid to the more vivid form of supposition seems to indicate this hope. He says: *εἰ οὖν ἴδοιεν καὶ νῷ καθάπερ τοὺς πολλοὺς ἐν μεσημβρίᾳ μὴ διαλεγομένους, ἀλλὰ νυστάζοντας καὶ κηλουμένους ὑφ' αὐτῶν δι' ἀργίαν τῆς διανοίας, δικαίως ἂν καταγελῷεν'... ἐὰν δὲ ὁρῶσι διαλεγομένους... τάχ' ἂν δοῖεν ἀγασθέντες.* It may perhaps be thought that the antithesis here implies a stronger expectation of the latter condition being realized; and such examples are too rare to decide the question. In

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most cases, however, in which expectation, desire, or hope is more prominent in one of the two successive conditions, the same form is found in both, as in Dem. Cor. § 178 (pp. 287, 288): *ἐὰν μέν δέξωνται ταῦτα καὶ πεισθῶσιν ἡμῖν*, and *ἂν δ' ἄρα μὴ συμβῇ κατατυχεῖν.*

4. Cases of the more vivid form in suppositions the realization of which the speaker strongly dreads, and is trying by his argument to prevent, are found in Dem. Aphob. I. § 67 (p. 834): *ἐὰν γὰρ ἀποφύγῃ με οὗτος, ὃ μὴ γένοιτο, τὴν ἐπωβελίαν ὀφλήσω μνᾶς ἑκατον.* But the use of this form was optional here also; for we find in Aph. II. § 18 (p. 841) *ποῖ δ' ἂν τραποίμεθα, εἴ τι ἄλλο ψηφίσαισθε;* (referring to the same danger of an adverse vote), and within three lines of this we have *τούτου γίγνεται, τὴν ἐπωβελίαν ἐὰν ὄφλωμεν*, and still again in § 21 (p. 842), referring to the orator's sister in the same contingency, *εἰ δ' ὑμεῖς ἄλλο τι γνώσεσθε, ὃ μὴ γένοιτο, τίνα οἴεσθε αὐτὴν ψυχὴν ἕξειν, ὅταν ἐμὲ μὲν ἴδῃ, κ. τ. λ.;* I shall speak of these passages again below.

5. Cases in which the more vivid form is chosen to heighten the absurdity of an already absurd supposition sometimes occur, although they are naturally rare. Such seems to me to be Plat. Repub. X. 610 A: *ἐὰν μὴ σώματος πονηρία ψυχῇ ψυχῆς πονηρίαν ἐμποιῇ*, *unless a bodily vice shall engender in a soul a mental vice*,—a supposition which is at once stigmatized as absurd in 610 C: *τοῦτό γε οὐδείς ποτε δείξει.* Even after this the supposition follows: *ἐὰν δέ τις ὀμόσε τῷ λόγῳ τολμᾷ ἰέναι καὶ λέγειν, κ. τ. λ.* Again, in 612 B, it is said of the soul *καὶ ποιητέον εἶναι αὐτῇ τὰ δίκαια, ἐάν τ' ἔχῃ τὸν Γύγου δακτύλιον ἐάν τε μή, καὶ πρὸς τοιούτῳ δακτυλίῳ τὴν Ἅιδος κυνῆν*, i. e., *that the soul must do what is just, whether she have or have not the ring of Gyges, and besides such a ring the cap of Hades.* In a previous passage (359 C) the former miracle had been mentioned in the other form: *εἰ αὐτοῖς γένοιτο οἵαν ποτέ φασι δύναμιν τῷ τοῦ Λυδοῦ προγόνῳ γενέσθαι*, and again (360 B): *εἰ οὖν δύο τοιούτω δακτυλίω γενοίσθην, κ. τ. λ.*, but with less emphasis. See also Eurip. Phoeniss. 1215, 1216: ΑΓΓ. *οὐκ ἄν γε λέξαιμ' ἐπ' ἀγαθοῖσι σοῖς κακά.* ΙΟΚ. *ἢν μή γε φεύγων ἐκφύγῃς πρὸς αἰθέρα.* And Orest. 1592, 1593: ΟΡ. *φησὶν σιωπῶν· ἀρκέσω δ' ἐγὼ λέγων.* ΜΕΝ. *ἀλλ' οὔτι χαίρων, ἤν γε μὴ φύγῃς πτεροῖς.* Under this head, as it seems to me,

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might very properly come a Greek version of the proverb, 'If the sky fall, we shall catch larks,' in which the absurdity of the condition is heightened by expressing it in the more vivid form in English, and I feel confident (after carefully considering Mr. Morris's argument) that the effect would be the same in Greek. Similar to the examples just quoted is the sarcastic reply of Socrates (PLAT. Gorg. 470 C) to the taunt of Polus that even a child could show him to be in the wrong; to which Socrates replies: *πολλὴν ἄρα ἐγὼ τῷ παιδὶ χάριν ἕξω, ἴσην δὲ καὶ σοὶ, ἐάν με ἐλέγξῃς καὶ ἀπαλλάξῃς φλυαρίας*, i. e. *I shall be much obliged to the child, and equally so to you too, if you shall refute me*, etc. I will refer also to two conditions from PLAT. Euthyd. 299 B, C, which are quoted below (p. 101), one of which supposes a cartload of hellebore to be given at one dose, and the other supposes the patient who drinks it to be as big as the "statue at Delphi." Both are expressed by the subjunctive with *ἐάν*.

6. Occasionally the subjunctive form seems to mark a supposition as more emphatic than others with which it is contrasted, and the optative form to mark one as less emphatic than others, when there is no apparent distinction on the score of probability, expectation, desire, fear, or sarcasm. Thus in PLAT. Protag. 330 C—331 A we have a series of conditions stated by Socrates in the optative form: *εἴ τις ἔροιτο ἐμὲ, ἀποκριναίμην ἄν,—εἰ οὖν μετὰ τοῦτο ἔροιτο, φαῖμεν ἄν,—εἰ οὖν εἴποι, εἴποιμ' ἄν,—εἰ οὖν εἴποι, τί ἂν ἀποκρίναιο*; But here all at once he changes to the subjunctive form, and says: *τί οὖν ἀποκρινούμεθα αὐτῷ, ταῦτα ὁμολογήσαντες, ἐὰν ἡμᾶς ἐπανέρηται, κ. τ. λ.*; The argument had here reached a point at which Socrates felt he had gained an advantage, and he therefore puts this question with special emphasis. The whole conversation is purely imaginary, and certainly there was no greater probability or expectation of this question being asked than of the others, which indeed were a necessary introduction to this; the simple truth, as it seems to me, is that a more vivid form was chosen to state a supposition which was to be made more prominent in the argument than the others. After this vivid statement of the condition, with a repetition of the apodosis *τί αὐτῷ ἀποκρινούμεθα*; Socrates

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returns to the other form and says: *ὑπέρ γε ἐμαυτοῦ φαίην ἂν... καὶ ὑπὲρ σοῦ δὲ, εἴ με ἐῴης, ταὐτὰ ἂν ταῦτα ἀποκρινοίμην.* In PLAT. Crit. 51 D the laws are supposed to say: *καὶ οὐδεὶς ἡμῶν... ἀπαγορεύει, ἐάν τέ τις βούληται ὑμῶν εἰς ἀποικίαν ἰέναι, εἰ μὴ ἀρέσκοιμεν ἡμεῖς τε καὶ ἡ πόλις, ἐάν τε μετοικεῖν ἄλλοσέ ποι ἐλθὼν, ἰέναι ἐκεῖσε ὅποι ἂν βούληται, ἔχοντα τὰ αὑτοῦ.* There the single optative seems to indicate a condition which is less emphatic than the main one, although there is no other apparent ground for the change of form. We may produce the same effect in English: *and if any one of you shall want to go off to some colony,—supposing we and the state should fail to please him,—or if he shall want to go to some foreign country and live, none of us forbid him to go*, etc. No one would have been offended surely if any of the conditions quoted under this head had been expressed in the other form.

In the passages already quoted, the choice of mood in the protasis appears to have been affected more or less by the considerations mentioned, sometimes perhaps by several of them at once, and in each case some peculiar effect is produced by the mood chosen. Now it seems to me that these various considerations can hardly be reduced to the single one of 'probability,' 'expectation,' or 'anticipation of realization,' although I admit that this is one of the most common grounds of distinction where any can be seen. I have already stated (*Transactions* for 1873, p. 71) that this and the other grounds of distinction "seem to me to stand to the more comprehensive one of greater and less vividness in the relation (if I may be allowed the expression) of species to a genus"; in other words, I think the mistake commonly made here lies in confounding a very common (perhaps the *most* common) use of the distinction between the subjunctive and optative in protasis with the distinction itself. It will hardly be denied, I think, that, with the exception of the one relating to the physician before the jury of children, in PLAT. Gorg. 521 E (of which below), all these conditions could have been stated in the other form without *essential* change of meaning, though often not without the loss of some special emphasis or effect. Now, if there were "a distinction in essence and fundamental"

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between the two forms, I hold that this interchange would be impossible, except on the assumption that the examples quoted are exceptional and too infrequent to cast doubt on an established principle of the language. But if the distinction is such as I have stated it, this interchange is just what would naturally be expected.

It will still be urged, however, that exceptions are as fatal to my principle as to the other, and that, if there is no essential and fundamental distinction between the two forms, every future supposition should admit of a double statement. I have indeed said that most of the conditions quoted by Professor Morris are more naturally stated in the optative form, because this vaguer form is in most cases better adapted to an improbable supposition, which must needs be more vaguely conceived than one which is distinctly anticipated. But I have given cases of the subjunctive in conditions which are quite as improbable and even absurd as any in Mr. Morris's list. Surely 'escaping into the air,' 'taking flight on wings,' and 'having the ring of Gyges with the cap of Hades' could not be expressed by the subjunctive if absurdity or violation of physical laws were a bar. And yet every one must feel, with Professor Morris, that most of his examples could not be changed to the subjunctive form without violence to the thought. Why now is this so? Even if it is said that the subjunctives just mentioned are due to "rhetorical effect" (which has long been a *deus ex machina* in Greek syntax), the question remains, why will not this potent agency transform Mr. Morris's examples for me as well as my own? To begin with the strongest case, AESCH. Agam. 37: *οἶκος δ' αὐτὸς, εἰ φθογγὴν λάβοι, σαφέστατ' ἂν λέξειεν*, and the parallel passage in PLAT. Protag. 361 A, where it is said of the issue of the argument: *εἰ φωνὴν λάβοι, εἰπεῖν ἄν, κ. τ. λ.*,—I would remark that there is nothing intrinsically more impossible in a *house* or the *issue of an argument* speaking than in *laws* speaking; and yet in PLAT. Crit. 50 C we find *τί οὖν, ἂν εἴπωσιν οἱ νόμοι*; The supposition of the laws addressing Socrates had first been made by *εἰ ἔροιντο* (50 A). Now why is there no absurdity in this sudden change to the more vivid

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form? Merely because the apodosis is a simple future τί (sc. ἐροῦμεν); so that the whole sentence means *what shall we reply if the laws (shall) say?* If, on the contrary, the sentence were *they* (the laws) *would astonish us by their eloquence if they should speak*, there would be the same objection to changing this to the subjunctive form which is felt in the other case of the house speaking. To say *the laws will astonish us by their eloquence if they shall speak to us* would be felt at once to be unnatural; but there is, as we have seen, no valid objection to be made to the protasis. So also in the changed form of the passage from Aeschylus the apodosis *the house will speak most plainly* is the only objectionable part; and this offends us because 'will speak' is too absolute and unqualified an assertion to make of a house, the more contingent and weaker form 'would speak' being the only one appropriate under ordinary circumstances. If now we substitute an apodosis here in which a simple future can stand, e. g. *dark deeds will come to light*, then grammatically (though not dramatically) all objection to ἢν φθογγὴν λάβῃ οἶκος is felt to be removed. Compare "Foul deeds *will* rise, though all the earth o'erwhelm them."

In ARISTOPH. Nub. 754: εἰ μηκέτ' ἀνατέλλοι σελήνη μηδαμοῦ, οὐκ ἂν ἀποδοίην τοὺς τόκους, the optative is perhaps necessary, as Mr. Morris intimates, to account for the patience of Socrates with the stupidity of his pupil, which would (he thinks) have been intolerable in the subjunctive form; nevertheless, whatever grammatical or logical objection there may be to the subjunctive will be at once removed if we substitute in the apodosis οὔ με δεήσει ἀποδοῦναι τοὺς τόκους (I will not be tempted by Mr. Morris's success in verses to rush madly *in celeres iambos*). If a similar change is made in all the examples in the next fifty lines, to which Mr. Morris refers, no one can object to stating the conditions in the subjunctive except on the excellent ground that their impudence would be thereby greatly enhanced.

In AESCH. Pers. 431 the same principle holds; and if the apodosis be made future, e. g. *half my tale will not be told*, the subjunctive can stand in the protasis. I may add that, in

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my own judgment, the changed form which Professor Morris gives as erroneous is not only correct, but elegant.

Of the next example (PLAT. Repub. II. 359 C) I have already spoken; and I think it will be evident that this could have been stated *ἔσται δ' ἐξουσία . . . ἣν αὐτοῖς γένηται.*

As to the striking example from PLAT. Euthyd. 299 E, already mentioned: *εἴη ἂν εὐδαιμονέστατος, εἰ ἔχοι χρυσίου μὲν τρία τάλαντα ἐν τῇ γαστρί, τάλαντον ἐν τῷ κρανίῳ, στατῆρα δὲ χρυσοῦ ἐν ἑκατέρῳ τῶφθαλμῷ;*—I am encouraged to think that I am right in saying that it might be written *ἔσται . . . ἐὰν ἔχῃ;* by two conditions which precede (299 B and C): *καὶ καλῶς ἐκεῖ ἕξει, ἐάν τις αὐτῷ τρίψας ἐγκεράσῃ ἐλλεβόρου ἅμαξαν;* to which Ctesippus adds: *πάνυ γε σφόδρα, ἐὰν ᾖ γε ὁ πίνων ὅσος ὁ ἀνδριὰς ὁ ἐν Δελφοῖς.* Surely whoever can swallow this "cartload of hellebore" will not be troubled even by "three talents of gold in his stomach!" I may add here, as a proof that no amount of absurdity or impossibility can make the subjunctive incorrect in protasis, DEM. Phil. III. § 68 (p. 128): *ὥστε, μηδ' ἂν ὁτιοῦν ᾖ, δεινὸν πείσεσθαι,* where *ὁτιοῦν* is a sort of *x* for which we are at liberty to substitute anything imaginable. The more common formula would undoubtedly be *οὐδ' ἂν εἰ ὁτιοῦν γένοιτο,* but here the irregular future infinitive after *ὥστε* makes the subjunctive in the dependent clause more natural.

In the argument cited from PLAT. Phaed. 72 B, C, we cannot, it is true, suppose the conditions to be changed to the subjunctive form without injury to the argument, because the apodoses are not of a kind to be stated absolutely in the future indicative; but I can see nothing in the conditions themselves which would be repugnant to the other form. In Phaedr. 245 D, however, I cannot doubt that either form could be used in the sentence *εἰ γὰρ ἔκ του ἀρχὴ γίγνοιτο, οὐκ ἂν ἐξ ἀρχῆς γίγνοιτο.* For in the corresponding sentence just below, which on every ground should be parallel in construction, we have *ἀρχῆς γὰρ δὴ ἀπολομένης, οὔτε αὐτή ποτε ἔκ του οὔτε ἄλλο ἐξ ἐκείνης γενήσεται,* where the participle is clearly equivalent to *ἐὰν ἀπόληται.*

In the quotation from Pericles in ARIST. Rhet. III. 10, 7 (where three of Bekker's four Mss. read *ἐξέλῃ*), the optative seems clearly the more natural form, not only from the nature



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of the supposition, but from the implied apodosis, which would be in the optative with ἄν. With an appropriate apodosis, I think even this condition could have the other form.

In Xen. Anab. iii. 2. 24, the last of Mr. Morris's examples, I cannot see anything in the protasis, καὶ εἰ σὺν τεθρίπποις βούλοιντο ἀπιέναι, to exclude the subjunctive form; but the apodoses are all better expressed by the optative with ἂν than they would be by the future indicative. Indeed, it may be safely said that the implied protasis which conditions the first two optatives is also understood with the third, so that the expressed protasis states only part of the condition.

I fear that the doctrine of the effect of the apodosis upon the protasis may be considered even more heretical than the main proposition which I am defending. I must therefore give a few examples to illustrate this effect. It is especially evident in conditional relative sentences when the apodosis precedes the protasis and consists of an optative in a wish: in such cases the force of the optative in assimilating the dependent verb will be generally admitted. As examples may serve Odyss. i. 47: ὡς ἀπόλοιτο καὶ ἄλλος ὅ τις τοιαῦτά γε ῥέζοι, *may any other man likewise perish who shall do the like of this;* and Mimnermus Fr. 1: τεθναίην ὅτε μοι μηκέτι ταῦτα μέλοι, *may I die when I shall no longer care for these.* Here few will deny that if the wish had been expressed by any other form than the optative—even by a weaker expression, like βούλομαι with an infinitive—the dependent verbs would have naturally been in the subjunctive, without any essential change in meaning. In Il. v. 212–15 we have one protasis in the subjunctive preceding the apodosis (an optative in a wish), and another in the optative following it: the assimilation is here very marked.

> εἰ δέ κε νοστήσω καὶ ἐσόψομαι ὀφθαλμοῖσιν
> πατρίδ' ἐμὴν ἄλοχόν τε καὶ ὑψερεφὲς μέγα δῶμα,
> αὐτίκ' ἔπειτ' ἀπ' ἐμεῖο κάρη τάμοι ἀλλότριος φὼς,
> εἰ μὴ ἐγὼ τάδε τόξα φαεινῷ ἐν πυρὶ θείην.

But in Il. ii. 258–261, a passage otherwise parallel, the assimilation is not effected. In Aesch. Prom. 979: εἴης φορητὸς οὐκ ἂν, εἰ πράσσοις καλῶς, *you would not be endurable if you*

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*should ever be in prosperity*, who can doubt that a change of εἴης ἄν to ἔσται would have caused a change of εἰ πράσσοις to ἢν πράσσῃς, *if you shall ever be in prosperity?* In DEM. Aph. II. § 18 (p. 841), quoted above (p. 96): ποῖ δ' ἂν τραποίμεθα, εἴ τι ἄλλο ψηφίσαισθε; what possible reason can be given for the optative in a condition which is twice expressed by the subjunctive and once by the future indicative, except the assimilating force of the apodosis? The same effect is quite as striking in English as in Greek. We should say 'Turkey will beg for mercy, if Russia shall take (takes) Constantinople'; but we should also say, with no change in our view of the contingency, 'England would be in danger of war, if Russia should take Constantinople.' The form which the apodosis takes (which may be determined by various considerations not affecting our view of the realization of the condition which is to follow) in such cases naturally determines the form of the dependent protasis. That is, the greater or less absoluteness with which we state the apodosis often (though not always) affects the "distinctness and vividness" with which we state the same condition at different times.

Though this assimilating effect is more apparent and probably more powerful upon a protasis which follows its apodosis, it is by no means confined to such cases. I cannot see any other ground than assimilation for the distinction in the two conditions in Odyss. VIII. 352, 353, and 355, 356:—

> πῶς ἂν ἐγώ σε δέοιμι μετ' ἀθανάτοισι θεοῖσιν,
> εἴ κεν Ἄρης οἴχοιτο χρέος καὶ δεσμὸν ἀλύξας;
>
> Ἥφαιστ', εἴ περ γάρ κεν Ἄρης χρείως ὑπαλύξας
> οἴχηται φεύγων, αὐτός τοι ἐγὼ τάδε τίσω.

Here Poseidon, the last speaker, who proposes to be surety for Ares, would naturally be expected to state his supposition in the weakest form; but the apodosis τίσω, *I will pay*, is an absolute statement compared with δέοιμι ἄν, and this decides the form of the protasis by an influence as strong and as unfelt (by the speaker) as that which would cause every schoolboy to translate one protasis by *if Ares shall depart* (or *departs*), and the other by *if Ares should depart*, without

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dreaming of one expressing more probability or implying more expectation than the other. I am sorry that I cannot see the ground for the distinction between the two suppositions of Philip's death, ἂν οὗτός τι πάθῃ and εἴ τι πάθοι, in DEM. Phil. I. §§ 12, 13 (p. 43), for which Professor Morris argues so persuasively; but I cannot be convinced that an orator could use *within a single minute* and with reference to the same future contingency two forms of expression which differed essentially and fundamentally in the manner in which the supposed event was conceived, with regard to its probability or to the expectation or anticipation of its realization. I believe, on the contrary, that here too the protasis was assimilated in each case to the apodosis: in the former case this was ταχέως ἕτερον Φίλιππον ποιήσετε; in the latter it was a complicated sentence consisting of two distinct apodoses in the optative with ἄν, each conditioned specially by a participle. If the former apodosis had been ποιήσαιτ' ἄν (as it might easily have been), and the latter had been simply οὐδὲ Ἀμφίπολιν δέξασθαι δυνήσεσθε, I feel confident we should have had the two forms of protasis reversed.

The strong evidence I have given of the effect of the apodosis on the form of the protasis will, I trust, strengthen the position already taken (p. 90), that the subjunctive differs from the optative in common future conditions very much as it does in oratio obliqua after past tenses in conditions which in direct discourse have the subjunctive, and in the indirect form allow either subjunctive or optative. This is also an effect of the leading verb on the dependent mood, which is as plain in English as in Greek. Thus we say 'he says he will tell her *if she comes;*' but 'he said he would tell her *if she came*' (i. e. '*should come*'); corresponding to the Greek ἐὰν ἔλθῃ and εἰ ἔλθοι. The distinction of the Greek is that its greater freedom allows both εἰ ἔλθοι and ἐὰν ἔλθῃ in the latter case, while the English allows only the weaker form; that is, in Greek the assimilation is optional, in English it is compulsory. Perhaps the most striking cases of this principle in Greek are those in which the apodosis and the leading sentence on which the oratio obliqua depends are united in

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one sentence; as in DEM. Cor. § 145 (p. 275): *οὐκ ἦν τοῦ πρὸς ὑμᾶς πολέμου πέρας οὐδ' ἀπαλλαγὴ Φιλίππῳ, εἰ μὴ Θηβαίους καὶ Θετταλοὺς ἐχθροὺς ποιήσειε τῇ πόλει*, i. e. *Philip saw no way of ending or escaping the war, unless he should make*, etc. Here *ἐὰν μὴ ποιήσῃ* would have been equally proper, as is plain from sentences like THUC. II. 24: *ἢν δέ τις εἴπῃ ἢ ἐπιψηφίσῃ κινεῖν τὰ χρήματα..., θάνατον ζημίαν ἐπέθεντο.* I am very skeptical of any theory which assumes a more fundamental distinction between *ἢν ποιήσῃ* and *εἰ ποιήσειε* in ordinary cases than is universally admitted to exist here.

The same principle is illustrated by the double form of protasis allowed after final clauses which depend on past tenses; but this is really another form of protasis in oratio obliqua. See DEM. Aph. I. § 53 (p. 830); THUC. I. 58, 91.

I must leave many points of detail both in Professor Sewall's and in Professor Morris's paper unnoticed. I will briefly allude in conclusion to what seems to me a fatal objection to the system of classification advocated in both those papers. The fundamental idea assigned to the subjunctive, that of "contingency" or that of "anticipation" or "expectation" of realization, in my opinion, fails utterly to explain the nature of the "present general suppositions" expressed by *ἐάν* and the subjunctive. Professor Morris does not allude to these, and he has perhaps little occasion to do so in his argument; Professor Sewall mentions them as suppositions of "uncertain fact," and quotes two in illustration of this explanation. I cannot see, after carefully considering his interpretations of these passages, how his doctrine would enable us to distinguish between the cases which require the subjunctive and those which require the present or perfect indicative. Why is *if ever they have fought a battle* any more a "supposition of uncertain fact" than *if these men have fought a battle to-day* (the fact supposed being uncertain)? I fear, however, I have failed to understand this part of Mr. Sewall's paper, for he speaks of *ἢν προσμίξωσι* (THUC. II. 39) as "in the *past*, not future." It certainly is not future; but it seems to me impossible to conceive of it as past, or even as strictly present. It rather refers indefinitely to any one of a series or class of

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acts; and the Greek is perhaps the only language which ever undertook systematically to distinguish this indefinite "general" supposition by construction from the simple present supposition. Now I hold it to be impossible to bring these conditions under one head with the future suppositions which take the subjunctive by any such sweeping definitions as the one just mentioned. The subjunctive in the latter case is generally interchangeable with the future indicative, and can be translated by this tense in both English and Latin; the other is regularly expressed in both English and Latin by the present indicative, and sometimes takes this form even in Greek. I have already described the quasi-present general condition as a "variation (so to speak) of the ordinary present condition," while the corresponding past general condition is a variation of the ordinary past condition expressed by the past tenses of the indicative.* This important relation, with its consequences, must be apprehended, as it seems to me, before the true force of the subjunctive in protasis can be understood. It will be borne in mind that I refer here to the distinction between the use of the subjunctive in *future* conditions (where it may be either particular or general) and its use in *present general* conditions, and not to the 'general' character which may be given to any class of conditions without essentially changing their nature. A distinction based upon this latter character has been recognized in the subjunctive in protasis by Bäumlein, as I have already stated;† but it led him to no important result, as it is the basis of no distinction in construction. Bäumlein shows in his first statement of the subject that he has no such distinction in mind as that which I have used in my classification; for his

* See *Transactions* for 1873, pp. 64–66.

† See Bäumlein, *Untersuchungen über die griech. Modi*, p. 211. For remarks on his views of this subject, see *Transactions* for 1873, pp. 66, 67. It is there said: "Bäumlein leaves the subjunctive in general conditions, as well as in other kinds of protasis, to be explained on his single principle as denoting a 'Tendenz zur Wirklichkeit'; and he seems to have no suspicion that the two subjunctives stand in different relations to the present indicative." It is no wonder, therefore, that he found his distinction "unessential" (unwesentlich) and "without effect in changing the meaning of the construction" (die Bedeutung dieser Construction *nicht ändert*). *Modi*, p. 221; cf. p. 224.

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very first example is ὁ δέ κεν κεχολώσεται ὅν κεν ἵκωμαι (Il. I. 139), and of eighteen other Homeric examples cited to illustrate the general use of the subjunctive with ὅς κε, "wonach eine Gattung von Fällen als eintretend gesetzt wird," no less than sixteen contain subjunctives referring to the future. It is absolutely necessary to recognize distinctly the element of *time* in order to appreciate the relations of the subjunctive in its two uses, first, to the future and the present indicative, secondly, to the two corresponding uses of the optative. I cannot feel, therefore, that the two systems of classifications which I have discussed in this paper, as they are based on other considerations and exclude wholly or in great part the element of time, can possibly give just prominence to these important relations.

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## VII.—*On Certain Influences of Accent in Latin Iambic Trimeters.*

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§ 1. With regard to the influence of accent in poetical composition among the Romans two views have been entertained: the one by Corssen, Weil and Benloew, Lucian Müller, etc., that accent had no influence at all either among the Greeks or among the Romans: the other, by Bentley, G. Hermann, Ritschl, Langen, etc., that the Romans did not disregard accent, but as far as it was convenient, avoided conflict of accent and poetical ictus, because the Latin accent was stress of voice as well as elevation, while the Greek accent, being only elevation as in music, was disregarded. Those who hold the former opinion assert that the coincidence of ictus and accent in Latin poetry is a necessary result of the structure of the verse and the Latin system of accentuation. I formerly was of this opinion myself, but by attempting to prove it to be true, I convinced myself that it was false, and

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this paper presents the results of my investigation of the subject. But, although it seems certain that accent had an influence, still this influence is hardly to be attributed to a greater stress than was possessed by the Greek accent. It is, indeed, highly probable, if not certain, that the Latin accent carried with it some little stress of voice, but not appreciably more than the Greek. It is well established that the Greek accent was essentially musical elevation without stress, that is, without conscious effort to produce stress; but the condition of the vocal organs necessary to produce elevation, other things being equal, produces also some intensity of sound, so that an effort would be required to prevent slight stress from accompanying the accent, and there is no reason for believing that such effort was made. In the same way, a consciously made stress of voice would carry with it a slight unconscious elevation. Hence, the Greek accent was elevation with the slight stress which naturally attended it, and ictus among Greeks and Romans was stress with the slight elevation which naturally attended it. This is shown not only from physical considerations, but also by the fact that modern Greek accent, which generally falls on the ancient tone syllable, has the stress-element fully developed; which proves that the germ of stress or the tendency to it already existed in the ancient accent. How early the stress became an appreciable element, it is difficult to ascertain; but it is certain that it must have taken place already in ancient times. Babrius, whose date cannot be determined with certainty, but who, according to Lachmann, must have flourished about the time of Domitian, recognizes the accent to a sufficient extent to show that he felt in it considerable stress or ictus.

§ 2. The following appear to be sufficient reasons for doubting that the Latin accent contained considerably more ictus than the Greek:

1. In the first place, all the Roman grammarians treat accent as a mere elevation of tone, except that Diomedes, who flourished in the latter part of the fourth century, says: "Accentus est elatio orationis vocisve intentio." It is maintained by some that the Roman grammarians blindly followed

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the Greeks in this, and that Diomedes alone perceived the truth. It is true that in many grammatical questions the Romans adopted and tried to adapt what the Greeks had done; but in some matters, and especially in this very question of accentuation, they did not imitate the Greeks, as is shown by the fact that the Roman rules give us 'ámant,' 'amántque,' while the Greek rules give λέγει, λέγει τε; and simlarly 'méă,' 'meăque'—λέγε, λέγε τε; 'mīrĭ,' 'mīrĭque,' εἶπε, εἶπέ τε. Further, the language of Diomedes does not necessarily imply that he considered accent to be stress; for the word "intentio" could well be taken from the instrumental nomenclature, where it means 'tightness' of a string and refers to musical pitch; and even supposing that he imitated the Greeks, we may readily believe that this very expression was borrowed from the ἐπιτεταμένη προσῳδία of Glaucus of Samos. Moreover, Diomedes does not call accent "elatio orationis" *and* "intentio vocis," but "elatio" *or* "intentio," implying that he meant the same thing by both expressions. But even if none of these explanations are satisfactory, it would affect the main question in no degree if accent did contain stress in the times of Diomedes; for, as was shown above, even the Greek accent had at that late day already commenced to partake of the character of ictus. It is scarcely necessary to allude to the statement of the grammarian Pompeius that "illa syllaba quae accentum habet *plus sonat* quasi ipsa habet maiorem potestatem," for a man to whom is universally attributed "verbosa et puerilis tractandi ratio" can have no authority in such matters; and besides it does not make any difference if an accented syllable did "sound more" in his times. When Quintilian finds the ictus on the penultima of "volucres" and for that reason says that that syllable must be accented, he does not mean that the ictus *is* accent; for anybody could see that the ictus fell on it; but he means that the verse ("pecudés pictaéque volúcres") shows that this syllable is considered long by the poet in this instance, and hence must receive the accent like all long penults. If he had meant that ictus was accent, we should have to understand that in the same line "pecudes" is to be accented on the last syllable.



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This remark of Quintilian's, therefore, teaches two important facts: *first*, that accent and ictus were different things; and *secondly*, that accent, as well as ictus, was observed in reading poetry.

2. In the second place, in the old Saturnian Verse, which belonged to an age that was far from being artificial, conflict often takes place between accent and ictus, particularly in the first half of the verse, where it seems rather to have been sought than avoided, as in

consól censór aidílis quí fuít apúd vos,

where "aidilis" could have been placed first, which would not only have removed the conflict, but also would have caused the three offices to appear in their natural order—aedile, consul, censor.

Nor, if accent had been stress, could Plautus and other poets have written such verses as these:

PLAUTUS: Quam id éxpetívisse ópere tám magnó seném.
TERENCE: Dedít, cenávit. gaúdebam. ítem alió dié.
PACUVIUS: Sólisque éxortú capéssit cándorem óccasú nigrét.

Yet such verses are very numerous.

Further, we should surely expect the ictus to fall upon the accented syllable where it could be done with no trouble, and this coincidence could be secured at the close of an Iambic trimeter by employing a monosyllable or trisyllable at the end; but in fact the monosyllable at the end was avoided as much in trimeters where it secured agreement, as in hexameters where it caused conflict of ictus and accent; and trisyllables are evidently not sought after.

Again, when Ennius introduced hexameters, he would have avoided monosyllables at the end of verses, and other causes of discord. It may be claimed that he imitated Homer in this, as he certainly did in many other things; but by a careful examination and computation, I have found that he left such matters to take care of themselves, and not till a later day was any effort made to force the ictus to fall upon the accent. A full investigation of this subject would require a long discussion, and belongs to hexameters, while I propose to confine the present question to trimeters.

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But I shall call attention to one more illustration of the impossibility of assigning to accent any considerable degree of stress. In the Galliambics of Catullus such verses as the following would have been intolerable:

> Phrygium út nemús citáto cupidé pedé tetigít.
> Ut apút nivem ét ferárum gelidá stabŭla forém.

These verses were, indeed, intended to be wild and fiery; but still, if the accent had been of the nature of ictus, no one could have felt the rhythm at all.

3. Thirdly, syllables are sometimes contracted in such a way that the accent is removed to another syllable, and the accented syllable itself even vanishes. For instance, in the dramatic authors, the forms of 'súus' and 'túus' (or more properly 'suos,' 'tuos') are very often read as one syllable, the *u* becoming a consonant. Many examples of similar abbreviations of other words occur. So Virgil writes 'arje te,' 'abjete, 'tenvia,' for 'aríete,' 'abíete,' 'tenúia.' And very often, especially in derivatives, a vowel which has the acute accent becomes modified in a way which renders it scarcely credible that much stress could have been on it in its modified form, as '*cap*io,' 'ac*cip*io,' '*can*deo, ac*cen*do,' etc.

§ 3. Having thus shown that the recognition of accent in poetry could not have been due to its having greater stress than the Greek accent, I shall now present in a general way what appears to me to have been the cause of its being recognized at all; for in the sequel I shall show that it certainly was recognized under certain circumstances.

1. Each word receives its individuality, so to speak, from its accent, by which one syllable, and one only in each word, is distinguished from the rest by a special pronunciation. Hence, it is naturally more agreeable to the ear to hear the ictus on that syllable which is already marked by a distinction, especially as that distinctive peculiarity contains a slight element of ictus, as has been shown. For if any other syllable receives a peculiar pronunciation, the word necessarily sounds unnatural. Now, in Greek the accent occupies so many different positions in words of the same metrical form, that the introduction of a variation from the usual pronuncia-

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tion does not seriously mar the form of the word. Moreover, as quantity had to be regarded, it would have been almost impossible to regard accent also, as this stood in so many different relations to quantity. It was even necessary for a Greek to learn the accent of each individual word; and before doing this, he could only assign the accent within certain limits. The Romans, on the contrary, were accustomed to an entirely fixed accent in all words of the same metrical form, and the first time they heard a word uttered, their ears demanded that the accent should fall on a certain syllable. Consequently a word with the ictus on an unaccented syllable sounded much more unnatural to a Roman than to a Greek. Hence, to put it briefly, one reason for avoiding conflict of accent and ictus was *the uniformity of Latin accent.*

2. Again, the laws of accentuation along with the structure of verses often causes the ictus and accent to fall together, whether the poet chooses, or not, to have them together, as in the Iambic trimeter just before and after the caesura, and in the closing feet of the heroic hexameter. This frequent accidental or necessary coincidence of ictus with accent, having become familiar to the ear, caused a verse in which it did not exist, to sound rough and unnatural. Consequently, in those parts of verses where such a coincidence naturally occurred very often, the poets in the course of time sought to make it universal.

3. Finally, the last syllables being uttered very obscurely before the time of Ennius, an attempt was made to keep the ictus off them as modifying the words too much; but to keep the ictus off the final syllable is, in very many instances, to place it upon the accented syllable. This subject will be discussed more at length in another place.

§ 4. I shall now proceed to investigate the relations of ictus to accent in Iambic Trimeters; and first I shall discuss the subject in a general way, and then shall examine individual feet and words.

In order to ascertain whether the Romans regarded accent in poetical composition, we have four tests which we may apply. First, we can compare verses written by ancient

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authors with verses which we know to have been composed without any reference to accent; and as it happens that I once wrote some Iambic Trimeters myself, both in Latin and in Greek, I shall make use of them to show that while in the Latin ones certain conflicts of ictus with accent occur which are not to be found in the Latin authors, the Greek ones differ in no respect from the Greek Tragic verse. Whether these verses, as poetry, are bad or execrable, does not matter; they have the *structure* of the ancient verses, except that no attention was paid to accent. The Latin verses will be recognized as a translation from Mark Antony's speech in "Julius Caesar," and the Greek ones as a translation of Cato's soliloquy in Addison's "Cato." (They are appended to this article for reference.) Secondly, we may read Greek verses *with Latin accent*, and see whether the relations of ictus to accent are the same as in Latin verses; but in doing this we must not, like Langen, overlook the fact that words of the various metrical forms do not occur in the same numerical ratio to each other in the two languages. Thirdly, we can examine into the disposition of words in the verses themselves, and see whether or not those conditions are avoided which would produce conflict of ictus with accent. Finally, we may compare with each other the verses of authors who wrote at different periods, and see whether we can trace any progress or modification in the relation of ictus to accent. In the hexameters this can be done very successfully; but in trimeters the want of the works of comic writers after Plautus and Terence renders this mode of investigation less important. I shall apply these various methods indiscriminately, as the circumstances may seem to require.

I have compared the number of those ictuses which come in conflict with accent, with the total number of ictuses, in many different authors. In counting the conflicts, I have not taken into account every instance where an ictus falls on an unaccented syllable, nor where an accent fails to have an ictus fall upon it (in dissyllables and monosyllables); but only those instances where the word has no ictus on the tone syllable, but *at the same time has an ictus on an unaccented*

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*syllable.* Thus, where words of the forms ⏑, —, ⏑⏑, have no ictus, I count it no conflict; and words of the forms ´ ⏑ ´ ⏑̄, ´ ⏑ ´, with two ictuses, one of which falls on the accent, are not regarded as presenting conflicts.

It is a little remarkable that Corssen, in discussing this subject, compares the conflict between ictus and the *Greek* accent in Greek verses with the conflict between ictus and Latin accent in Latin verses, and then goes into an elaborate investigation of relations that exist between the two systems of accentuation, in order to show that the Latin system was better adapted to bring about coincidence of ictus and accent; while he might have dispensed with the whole discussion by applying the Latin accentuation at once to the Greek verses. He even produces Greek verses in which the ictus falls upon the accent in nearly every foot, as,

> ὀρθὴν κελεύεις ἢ τὸ δένδρον φαίνεται;
> ἐφ' οἷς ἀεὶ γελῶσιν οἱ θεώμενοι.

How much, then, it would have strengthened his position, if he had observed that this occurs just where the Greek accent happens to conform to the Latin laws! Yet a still more careful inquiry will show that his position is untenable.

In Plautus about the fifth part of the ictuses stand in conflict with the accent, and in Terence a little less, but not quite so few as the sixth part. In Aristophanes, if we apply the Latin accent, we find conflict with about one fourth of the ictuses, and in Aeschylus somewhat more, but not quite so much as a third. From this we could infer at once that conflict was avoided to some extent by Plautus and Terence, unless there be something else which tends to produce coincidence in Latin more than in Greek; and if we adopted Langen's mode of procedure, we should conclude the matter at once, and we shall presently see that our conclusion would indeed be correct, but not because the method of proving it would be correct; for Latin verses are better suited than Greek ones for bringing about coincidence, just as the Greek comedy, not avoiding the incision in the fifth foot (⏑̄ | ´ ⏑ ´), shows a smaller ratio of conflicts than the tragedy, which,

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to some extent avoiding this incision, more frequently presents the ending ⏑̄ ´ | ⏑ ´, where conflict is inevitable.

Nearly all Latin Trimeters have caesura in the third or fourth foot. In order to be brief, I shall examine only the one in the third foot. Now, caesura brings about this form ´ ⏑̄|, which prevents conflict of ictus and accent unless a monosyllable immediately precedes the caesura; and if we inspect the form of a whole verse, ⏑̄ ´ ⏑̆ ´ ⏑̄ ‖ ´ ⏑̆ ´ ⏑̄ ´ ⏑ —, we shall see that coincidence must take place *after* the caesura in all words of the following forms:

| | | | |
|---|---|---|---|
| ´ ⏑ | ´ ⏑ ⏑ | ´ ⏑ — | — — ´ ⏑ |
| ´ | ´ ⏑ | ´ ⏑ ⏑ — | — — ´ — |
| | ´ — | ´ ⏑ ⏑ ⏑ | — ⏑ ´ ⏑ |
| | | ´ ⏑ ⏑ | — ⏑ ´ — |

Why Corssen counts the molossus (— ´ —) here, I don't understand: perhaps his printer did it for him. It will be observed that words of four syllables must receive two ictuses, which is not to be regarded as a case of conflict as long as one of them falls on an accent. The remaining possible forms for this position are — ´ ⏑, — ´ —; — ´ ⏑ ⏑, — ´ ⏑ —, — ´ ⏑ ⏑, ⏑ ´ ⏑ ⏑, ⏑ ´ ⏑ —, in which the ictus would fall as follows: ´ — ⏑̄, ´ ⏑ ´ ⏑ (which is not tolerated), ´ ⏑ ⏑ ⏑ (which with elision of final syllable is very common), ´ ⏑ ⏑ ´ (which is very common: in fact Ritschl and others hold that in ante-classical times words of the form ⏑ ⏑ ⏑ ⏑̄ had the accent on the first syllable, as 'fámilia,' 'hábuerim': a subject which will be discussed below). Hence we see that only words of these forms — — ⏑̄, — ´ ⏑ ⏑̄, — ´ ⏑ ⏑, ⏑ ´ ⏑ ⏑̄ (?), can cause conflict immediately after the caesura; for words of the forms not already mentioned, viz.: ⏑ ´ ⏑ —, ⏑ — ´ —, ⏑ ´ ⏑ ⏑, ⏑ — ´ ⏑, — ´ ⏑ —, cannot possibly occupy this place; and words of five or more syllables may be neglected as being of rare occurrence.

Further, as incision in the middle of the verse is very rare (⏑̄ ´ ⏑̆ ´ ⏑̄ ‖ ´ | ⏑̆ ´ ⏑̄ ´ ⏑ ´), it follows that the first *two* ictuses after the caesura will generally coincide with the accent, or at least will not come into conflict with it; for a

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word of two syllables creates a caesura in the fourth foot which in its turn prevents conflict in the next word, and a trisyllabic word, ⏑́ ⏑ ⏑́, has two ictuses, one of them on the accented syllable. The same happens when a word of four syllables with a long penult follows (⏑́ ⏑̄ ⏑́ ⏑̄).

Now let us examine the beginning of the verse. If the verse begins with a monosyllable, the first and second ictuses bear the same relations to the accent that they do after the caesura in the third foot. A trisyllabic word does the same thing, and besides causes the first ictus to coincide with the accent.

From these considerations it becomes evident that harmony of ictus and accent very frequently exists of necessity, or by accident, if we may call it accident. Consequently, the same harmony is tolerably common in Greek verses read with Latin accent, as I have above shown. But how does it happen that it is rarer than in Latin verses? This I shall now attempt to show. In the first place, it has already been proven that just before the caesura there will be coincidence, unless a monosyllable precedes. Now in Greek there are many more monosyllables adapted to occupy this place than in Latin, as *μέν*, *δέ*, *γάρ*, *γε*, *τε*, etc. Consequently there are very many such verses as EURIP. Hel. 837, 845, 847, 874 (all taken from fifty lines):

*ταὐτῷ ξίφει γε· κείσομαι δὲ σοῦ πέλας.*
*τὸ Τρωικὸν γὰρ οὐ καταισχυνῶ κλέος.*
*ὅστις Θέτιν μὲν ἐστέρησ' Ἀχιλλέως.*
*ἥκει πόσις σοι Μενέλεως ὅδ' ἐμφανής.*

If any one will examine a few verses of any poet, he can have no doubt about this fact.

Secondly, I have already alluded to the fact that incision in the middle of the verse tends to prevent coincidence. But as nearly all, or at least very many, verses have caesura in the third foot, nothing but a monosyllable can usually stand just before incision in the middle (⏑̄ — ⏑ — ⏑̄ ‖ — | ⏑ — ⏑̄ — ⏑ —). Now, in Greek there are many more monosyllables adapted to begin a new clause, or to follow caesura, than in Latin, the

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frequent use of the article alone being sufficient to give Greek the preponderance in this respect, as EURIP. Bacch. 853, 1063:

ἔξω δ' ἐλαύνων τοῦ φρονεῖν ἐνδύσεται.
τοὐντεῦθεν ἤδη τοῦ ξένου τι θαῦμ' ὁρῶ.

But such peculiarities of Greek do not account for all the difference between Latin and Greek in this respect, as I shall presently show. It may be asked why I am unwilling to attribute this entire difference to the effort on the part of the Romans to avoid discord between ictus and accent. My only reason is that it is not true that this entire difference is due to that cause; for I have just shown other causes to exist, and whether the causes can be pointed out or not, the *fact* is nevertheless established that coincidence resulting from accident or necessity is much more frequent in Latin than in Greek; for the Latin verses appended to this article exhibit nearly as frequent coincidence as the verses of ancient authors do, while the Greek verses do not differ perceptibly in this respect from the ancient Greek verses. To what then is the more frequent coincidence in the Latin than in the Greek verses to be attributed, as they were both written without the slightest reference to accent? It is true, indeed, that the Latin verses differ from those of the Roman Comedians in having the even feet pure, which increases the chances of accidental harmony; for if in the line,

vidête quántam fĕcerít Casca ínvidús,

we place a word forming a molossus, say 'formarit,' in the place of 'fecerit,' we shall have discord; but this only affects the comparison of these verses with ancient ones, and does not account for the difference between these Latin verses and the Greek ones I wrote, for I followed exactly the same scheme in the latter, except that I regarded the Porsonic law, which does not account for the whole difference. (Let it not be forgotten that when I speak of harmony or discord in Greek verses, I mean harmony or discord of ictus with *Latin* accent applied to the Greek words.)

From this discussion so far we can only draw the conclusion that harmony of ictus and accent is more frequent in Latin



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than in Greek trimeters, but that we should expect this to be the case to some degree, even if accent was entirely disregarded, but hardly to the same degree. Hence it is clear that we can come to no very definite conclusion until we examine individual words, or, as they are sometimes called, word-feet, in their relations to the feet or verse-feet; that is, we are not to compare all the ictuses in Latin verses with all in Greek, but we are to compare words of particular metrical forms in the one with words of the same forms in the other; and from this comparison it will appear that certain sorts of conflict were avoided by the Romans, but that this happens so rarely in comparison with the whole number of ictuses or even of coincidences, that the effect is hardly appreciable in a general comparison. And we shall even find that certain relations of ictus to accent were not allowed, although in the same word it was not demanded that they should coincide, as, for instance, in this verse,

insúltat ín nos prŏ́dĭtĭ̄ŏ trucíssimá,

the discord in the word 'proditio' is intolerable, while in

insúltat ín nos saéva prŏ́dĭtĭŏ́ furéns,

the relation of ictus to accent in that word is quite admissible, and yet neither ictus falls upon the accent, and consequently the number of coincidences in this verse, which is a correct one, is no greater than in the former one, which is incorrect; whence it is evident that a general comparison would not throw any light upon such facts, nor even detect their existence.

§ 4. But before speaking of the individual feet, it will be necessary to look into the form and nature of the verse. I shall regard the Iambic trimeter as a verse consisting of three dipodies, in each of which the stronger ictus falls on the second foot, not on the first. This is distinctly taught by ancient writers, and is accepted by Westphal, Geppert, and others. It does not matter if this verse *was* originally trochaic with anacrusis, thus — ⁞ ´ ⏑ — — | ´ ⏑ — — | ´ ⏑ — ⏑̄, in which the stronger ictuses fell on the first foot of each dipody; for after this verse had been long employed in the drama, and

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was declaimed on the stage—not sung, it is well established that they got into a habit of reciting thus, — — ⏑ ´ | — — ⏑ ´ | — — ⏑ ´. This seems to have been the usage already among the Greek Tragedians, since Aeschylus and Sophocles, when a proper name did not otherwise suit the verse, did not hesitate to place a choriambus instead of the first dipody, as Aesch. Theb. 488, Soph. Frag. 785 Dind.:

> Ἵππŏμέδŏντος σχῆμα καὶ μέγας τύπος,
> Ἀλφĕσĭβŏĭαν ἣν ὁ γεννήσας πατήρ,

which verses clearly cannot be read with anacrusis, unless, indeed, we adopt the view of Dindorf and others that the second syllable of such words was arbitrarily lengthened, which appears to me a rather arbitrary way of removing the difficulty. In other instances Sophocles admits the anapaest, which I believe Euripides always does, when it is necessary to employ such words. This is not due to a change in the character of the verse, but to the fact that the anapaest was becoming more tolerable, which enabled Euripides to avoid the insertion of the choriambus in place of the first dipody. But whatever may be the true way of reading the verse, the following way, which is very prevalent, is certainly wrong:

— ´ ⏑ — | — ´ ⏑ — | — ´ ⏑ — ;

for the stronger ictus of each dipody must fall on that foot which contains the short* thesis, so that we must either read thus:

— | ´ ⏑ — — | ´ ⏑ — — | ´ ⏑ — ⏑̄,

or without anacrusis thus:

— — ⏑ ´ | — — ⏑ ´ | — — ⏑ ´ ;

that is, the greater ictus must fall on the pure foot, whether the verse is treated as being trochaic or iambic. I shall, therefore, first treat it as iambic, with the greater ictus on the

* I follow established usage with regard to the words *thesis* and *arsis*, although the ancients used them in exactly the opposite sense, *thesis* being the syllable that receives the ictus, and meaning the planting of the foot in marching, while *arsis* was the raising of the foot—not the voice.

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second foot of each dipody, and then briefly adapt the investigation to the trochaic scheme.

§ 5. I now proceed to the laws themselves which govern the relations of ictus to accent; and first I shall discuss those which relate to words containing long syllables only, then those relating to words containing both long and short syllables, and finally those affecting words which contain only short syllables.

1. (*a*) In words containing only long syllables we find scarcely any influence of accent in the odd places (first, third, and fifth), which places I shall first discuss. In these places the accent is disregarded, because the ictus is much weaker on these than on the even feet, and does not modify the natural sound of the word as much. This seems to be the proper place to speak of the relative force of the different ictuses. The stronger ictuses are not all of equal stress, and the same is true of the weaker ones. The strongest ictus of all is on the second foot, the next strongest on the fourth, and next on the sixth; and of the weaker ictuses the strongest is on the fifth foot, the next strongest on the third, and the weakest ictus of all on the first. If we represent the stress of each ictus numerically, they will be something like the following:

— — 1 ◡ — 6 — — 2 ◡ — 5 — — 3 ◡ — 4,

where the numbers merely illustrate, and do not pretend exactly to represent the relative stress. This is because the strongest ictus being on the first dipody, and the force of an ictus depending as much on its ratio to its fellow-ictus as on its own absolute stress, the other ictus in the same dipody would naturally be weakened, and just in proportion as the stronger ictuses are weakened as you proceed in the verse, the weaker ictuses would be strengthened, until the fifth would be nearly as strong as the sixth. A full investigation of this subject would lead us into too long a discussion of music and general phonetics. From the above it is evident that a conflict of ictus with accent would create a maximum discord in the second, and a minimum in the first foot.

(*a*) It is evident that in the fifth foot the accent is entirely neglected; for there are very many such verses as PLAUT. Cas. 14:

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huc mihi venísti sponsam *praéceptúm* meám.

($\beta$) In the third foot an influence is claimed for the accent by some able scholars, nor will I deny that there may be some truth in their opinion; but if you examine the scheme of the verse, you will see that the word must have the form ⏑́ — or — ⏑́ —, in order that conflict may occur:

— — ⏑ — | — ⏑́ | ⏑ — — — ⏑ —,
or — — ⏑ | ⏑́ — ⏑́ | ⏑ — — — ⏑ —;

but either supposition not only requires the verse to be without penthemimeral caesura, but also divides it in the middle—faults which would, anyhow, prevent the existence of such verses. But if a trisyllable or a longer word in that position suffers elision, the conflict remains, while the caesura is not prevented, and this not unfrequently occurs, as PLAUT. Cas. 326, TER. Hecyr. 412, HOR. Epod. v. 97,

quam id *expettvisse* ópere tám magno seném.
vereor si *clámorem* eíus híc crebro audiát.
vos turba *vícatim* hínc et hínc saxis peténs.

Here the conflict is really between the accented thesis of the third foot and the ictus of the second, as the ictus of the third still falls on an accented syllable; but this also is a result of necessity, unless a trisyllable with long penult or a still longer word follows; but a trisyllable cannot follow, because it would create conflict in the fourth foot where a strong ictus falls, thus:

— — ⏑ ⏑́ —' | ⏑́ — ⏑́ | — — ⏑ ⏑́;

and verses with a word of four syllables in that position actually occur, as TER. Adelph. IV. 7, 11:

audivi. et dúcenda *índotátast.* scilicét.

This shows clearly that the conflict in the third was not so much avoided as prevented by the scheme of the verse and the harmony demanded in the fourth foot. This same peculiarity of the scheme (I mean the demanded caesura and the forbidden incision in the middle) even prevents a cretic word (⏑́ ⏑ —) from including the third foot *although there results no conflict;* thus:

— — ⏑ | ⏑́ ⏑ ⏑́ | ⏑ ⏑́ — — ⏑ ⏑́,

a form of verse which is extremely rare.

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The conflict rendered possible by elision in the middle of the third foot led Hermann (Epit. § 79) to the remarkable conclusion that the long penult, which by elision became the ultimate, was shortened, as 'cónce*de* huc,' 'sé*ce*de huc,' etc. But (in addition to an explanation which will presently be given) such elisions secure caesura, and so are introduced, in spite of the fact that they create a conflict of a certain sort. Even in the few verses which I have appended to this article, one example of this elision occurs:

**Signi usque *Pompei* ipse Caesar maximus.**

And I find instances of this among other verses which I have written, but never a conflict (otherwise) in the third foot. Therefore such verses as TER. Andr. 718, etc.:

**amicum amátorém, virum ín quovis locó,**

are faulty only because they have incision in the middle, and lack the proper caesura; for, as to the second foot, the syllable *mā* has sufficient stress to receive ictus, as it is long and is preceded by a short grave syllable. Bentley's views on this subject are as remarkable as Hermann's. He holds (De Met. Ter. *sub fin.*) that the third and fourth feet must not close with the ends of words, because that would cause ictus to fall on final syllables. Now, it is true that ictus, under certain circumstances hereafter to be investigated, must not be placed on the ultima; but in the case before us it is the caesura which prevents the incision in question, and such lines as

**persuasit nóx, amór, vinum, ádolescent(en)iá,**

are faulty because of incision in the middle and absence of proper caesura. It may be argued that the caesura is a result of the other principle, instead of *its* being a result of caesura; but a little reflection will convince any one that caesura was made on its own account, particularly as it was employed by the Greeks who had certainly no objections to placing ictus on a final syllable. It is true, the caesura penthemimeral or hephthemimeral is more uniformly observed in Latin than in Greek, but this results from the more frequent use of the spondee, and *in the fourth* foot, as I shall show, the discord

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caused by ictus on the ultima where the penult is long is avoided.

(γ) Having spoken of the fifth and third feet, I now come to the first. Since the strongest ictus of all falls on the second foot, the first foot, which, so to speak, is under the shadow of the second ictus, is subject to less stringent laws than any other foot in the verse. Accordingly we find in this foot synizesis, ecthlipsis, conflict of ictus with accent, and all licences more frequently than anywhere else. I shall produce a few illustrations taken from TER. Phorm.:

> addo istúc: inprúdens timuit ádulescens: sinó.
> non fuít necésse habere: set íd quod lex iubé
> nempe Phórmiónem. D. istunc patrónum mulierís.
> ego deós penátis hinc salútatum domúm.
> senectútem obléctet: respice aétatem tuám.

But the fact is a well known one, and there is no need of further illustrations. Even such forms as 'vilicus' (— ⏑́ ⏑), 'filius' (— ⏑́ ⏑), 'hominem' (⏑ ⏑́ ⏑), which involve a conflict of ictus and accent scarcely tolerated elsewhere, sometimes occupy the first place.

(*b*) I shall now examine the even places. In them, on account of the stronger ictus, the discord is greater when ictus conflicts with accent.

(*a*) Of course, as the sixth foot is always an iambus, and we are now discussing words which contain only long syllables, we pass this foot by for the present. It may be asked *why* this foot is always an iambus. To this it is sufficient to reply that the penultimate foot of nearly every kind of verse was, for reasons which it is unnecessary to mention here, kept pure, and that the Iambic trimeter was originally trochaic with anacrusis and catalexis:

— ¦ — ⏑ — — | — ⏑ — — | — ⏑ — ⏑̄ ̂,

so that the sixth iambus results from the original fifth trochee.

(β) In the fourth foot, where the next to the strongest ictus of all is found, conflict is very rarely tolerated; yet examples are not entirely wanting, as PLAUT. Cas. 322:

> meis inimícis voluptatóm creaverím,

where it will be observed, by the way, that the greater ictus

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of the fourth foot excuses the loose pronunciation in the other foot of the dipody where *volup-* appears as one syllable *v'lup-*.

Further examples are:

> Ennius: Palam mutíre plebeió piaculum ést.
> Plaut. Mil. II. 6, 22: nisi mihi supplícium virgarúm de te datúr.
> Liv. Andr. 40 (R.): puerarum mánibus confectúm pulcerrimé.
> Ter. And. Prol. 7: veteris poétae maledictís respondeánt.

But the instances are very rare compared with what would naturally take place, so that we must suppose that this conflict was, for the most part, avoided. Nor is it difficult to see why it should be. The Roman poets were accustomed to the Greek verses, which never admit the spondee in the fourth place; consequently when they introduced it themselves, they still shortened the thesis as much as the nature of words would admit, that is, they made the thesis "irrational." Besides, as has been shown, the difference between arsis and thesis must be as great as possible in the even feet, to prevent the verse from losing its iambic character. Moreover, whenever this discord occurs, the ictus falls upon the ultima and at the same time on the antepenult if the word has more than two syllables, which must be the case to prevent incision in the middle. Now, it is true, the ictus on the antepenult would not, of itself, be objectionable, as that syllable is generally a stem-syllable, as in 'plébeió,' or a prefix requiring emphasis, as in 'cónfectúm'; but when an ictus falls also on the ultima, so that the accent lies between two ictuses, on a syllable which must be to a great extent shortened and rendered obscure, the natural sound of the word is too much altered; and especially was this the case in the times of Plautus when the last syllables were very obscure—so much so that the Latin language was in danger of losing its terminations to a great extent, as actually occurred in the case of its sister languages, the Oscan and Umbrian: a danger from which it was rescued by its contact with Greek, and especially through the influence of Ennius, who reduced to fixed laws many things which he found entirely unsettled. Nearly all final syllables, for instance, had been treated as short. Some of these, though originally and properly long, he left short, as in 'benĕ,' 'malĕ'; others he left common,

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as in 'mihĭ,' 'tibĭ,' 'ubĭ,' but the most of this class he restored to their legitimate quantity, as in 'domī,' 'malō,' 'metū,' 'brevī,' 'velī,' 'maxumē.' This obscure sound of the final syllable is shown, further, to have existed by certain forms of the language, especially as they are found in inscriptions of an early date. In the ordinary forms a relatively obscure pronunciation is shown by a comparison of the forms *datas* (Sanskrit), δοτός, *datus*. Consequently, if the ultima is elided, the discord is to a great extent removed, even if the ictus falls on the antepenult—not, as Hermann says, because the penult (thus rendered ultimate) is shortened, as "ácced' húc," but because, as shown above, the syllable on which the ictus falls is always one adapted to receive stress, and the ictus which follows falls either on a stem-syllable or on a prefix demanding stress. Cases of this sort are much more frequent than those where no elision takes place, and to attribute this to chance is simply to believe that the majority of words suffer elision. I give here some examples of this elision:

PLAUT. CAS. 468: si neget adésse exánimatum ámittat domúm.
" " 470: sed uxorem ante aédes écciliam (?). heí misero mihí.
" " 496: tuam arcessitúram esse úxorem meám.
" " 497: ergo arcessívisse ait sese ét dixisse té.
TURPIL. 90 (R.): propter peccátum paúsillum índignissimí.
ENN. 242 (R.): Asta atque Athénas, ánticum, ópulentum oppidúm.
LIV. ANDR. 14 (R.): procat, tolerátis témploque hánc deducitís.
TER. Phorm. II. 3, 76: praeterierát iam ad dúcendum aétas. omnia haéc.
" " IV. 1, 7: quaeso ígitur cómmorábare úbi id audíverás.
" " " 11: quid gnato optigerit me absente, audistin, Chreme.

Many more could be produced, but these suffice to illustrate.

(γ) In the second foot the law is most stringent, but even here exceptions can be found, as PLAUT. Cas. 402 (unless the verse is corrupt):

quinque hanc omném rem meae erae iám faciam palám.

If the ictus falls before the accent, the discord does not belong to this foot, as I have already shown, but to the third where the weak ictus on the ultimate is tolerated, but is rare for reasons already explained, that have no reference to accent. An example is TER. Heaut. Tim. 147:

decrevi tántispér me mínus incuriaé.



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Here the stem-syllable 'tan-' is well suited for ictus, and as the ear is accustomed to the word 'tantis,' the conflict with the accent in 'tantísper' gives no offense. But even if there were any discord, it would belong to the third foot; for the law merely excludes the second ictus from the ultima, when the penult is long. Hence it may even happen that the thesis of the second foot be an accented syllable, as Heaut. Tim. Prol. 11; NAEV. 37 (R.):

> orátorem é-se voluit mé non prologúm.
> dic quó pacto eúm potiti: púgnan an doló.

This, however, is rare, and was evidently avoided to a considerable extent. In PLAUT. Cas., as far as I know, there is no instance of it except when a monosyllable forms the thesis, as v. 333 (Gep.):

> sine módo rus véniat ego remíttam ad te virúm.

Since in the second foot the ictus must not fall on the ultima when the penult is long, it follows that if a monosyllable immediately precedes the caesura, another monosyllable must precede that one, or else elision must take place before it, thus:

— — — | ´ | — || — ⏑ ´ — — ⏑ ´,
or — — — ´, | — || — ⏑ ´ — — ⏑ ´.

This use of two monosyllables is usually attributed to an attempt to place the second ictus on an accented syllable to prevent the discord that would result from ictus on an unaccented syllable followed by an accented monosyllable without ictus. This is rather absurd, for the former of the monosyllables is written first. Poets don't begin at the caesura and build the verse backwards. The true explanation is this. When the Latin poets admit the spondee into the second place the second ictus must not fall on the ultima. One way of avoiding this is to have caesura in the second foot:

— — — | — — || — ⏑ — — — ⏑ —;

but as often as this happens the space between this and the main caesura must be filled by either a dissyllable or two monosyllables, and of course the latter would frequently

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happen to be more convenient. The second monosyllable is in another dipody, and indeed in the third foot where the accent is neglected, as I have already shown. A few illustrations will make this clear:

PLAUT. Cas. 51 : nunc ne tu | té mihi || respondére postulés.
" " 231 : sic tandem | sí tu || Iuppitér sis mortuós.
" " 36 : post autem | rúri aut || ervom nísi comederís.

That the monosyllable before the caesura has nothing to do with it, is clearly shown by verses which have the iambus in the second place, for then the ictus *does* fall on the ultima when that monosyllable follows, as Cas. 3:

quid tu, *malúm*, *me* sequere?—Quía certumst mihí.

Now if the discord were betwen *-lum* and *me*, it would be as great in case of an iambus as in case of a spondee in the second foot. Of course, when a monosyllable is under the second ictus, another monosyllable will not necessarily follow if the poet chooses to employ the hephthemimeral caesura; hence we frequently meet with such verses as Cas. 12, 13, etc.:

quin potius | quód legatumst || tíbi negotiúm.
id curas | átque urbanis || rébus abstinés.

In Greek verses the caesura, as has already been shown, is often preceded by a monosyllable, and the result of this is that conflict frequently takes place in the second foot between ictus and (Latin) accent. I mentioned this as one of the reasons why such conflicts would naturally be more numerous in Greek than in Latin; but as I have just now shown that this conflict was really avoided in Latin, it may appear as a contradiction; but a little reflection will convince any one that this is not the case. The number of such verses is so small in Latin compared to the whole number of ictuses, or to the number of such verses in Greek, that in a general enumeration of conflicts, they would produce no appreciable effect, even if the law forbidding the conflict were neglected.

Let us now suppose the verse to be trochaic in its movement. Even with supposition, the laws relating to conflict would naturally be about the same. It is true, the main ictus would then fall in the odd places thus:

— ¦ ´ ⏑ — — | ´ ⏑ — — | ´ ⏑ — ⏑̄ ;

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but the thesis is no longer the syllable preceding, but the one succeeding the arsis, and the conflict between the ictus of the first foot and the thesis of second (iambic) foot is the one to be avoided (as this now becomes the weakest thesis of all), which would bring about most of the results examined. Still, the loose manner in which the first ictus is handled would convince me that the verse is to be considered iambic even if it were not known to be so from other considerations.

2. In words containing both long and short syllables, conflict is scarcely avoided at all, so long as the ictus is on a long syllable, and the accent on a short one. Accordingly, a word ending in an iambus receives the ictus very readily on the ultima, ◡ ⫠. If the whole word is an iambus, of course conflict is inevitable, if it receives an ictus at all; for the ictus cannot fall on the penult, ⫠ ◡ —, where the accent is. Corssen asserts that there is as much discord in ◡ ⫠ as there is in — ⫠; but he ignores the fact that quantity determines stress in Latin, and that the ictus in — ⫠ would have to be stronger than in ◡ ⫠ to give the foot its iambic character. That the second syllable of ◡ — is a much more important part of the word than the second syllable of — —, is shown by the fact that iambic words in the more polished hexameters do not suffer elision, and sometimes even admit hiatus after them.

With regard to the relations admissible between ictus and accent in words which end in a trochee or a dactyl, there has been much discussion among scholars, and appears still to be great diversity of opinion; but the conclusions I came to after an independent investigation made without any bias (as I did not at the time know the subject had ever been discussed), I have no reasons to change, after having read a good deal on the subject. To examine the question would require too much space; but I shall submit my views briefly. In my opinion, the ictus was for the most part excluded from the short syllables (ultima and penult) of such words, and especially from the ultima of dactylic words, but accent was not always (if ever) the sole cause of this restriction. In trochaic words, if the penult is *circumflexed* (which Corssen, *Quintilian,*

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and others affirm, and Langen denies), that is, closes with the low tone, the discord caused by ictus on the ultima is not very great, as an unaccented mora falls between the accent proper (the acute part of the circumflex) and the ictus. The chief objection, then, to this ictus is not its conflict with accent, but the stress it gives to a final short syllable, which must have been very considerable, so as to exceed the natural stress of the long penult. But even this objection is to some extent removed by the fact that the ultima forms only half the arsis, and is quickly passed over, while the other half will nearly always be found on a monosyllable or the accented penult of a dissyllable; and it will presently be shown that the second mora of a resolved arsis is better adapted for the accent than the first mora is. This linking together of two words by a resolved arsis, as in

anguis ín inplúvium decidít de tegulís,

is hardly tolerable unless the words are closely connected in construction, and the second one is a small one, so that the accent may fall under the second mora of the arsis; and at the same time the first one is usually small so as to bear prefixing. Examples seem to be more numerous in trochaic than in iambic verses; but in many of the examples sometimes produced, the ultima is really long, and fills up the whole arsis, as PLAUT. Trin. 714: 'síne doté (*dotei*) neque,' etc.; Capt. 914, 'carní;' Mil. 707, 'morté;' Men. 478, 'parté' or 'partí': cf. ENN. Ann. 420 'sub monté' (Bücheler, Lat. Decl., p. 50).

The unpleasant effect of an ictus on the ultima of a dactylic word has nothing at all to do with accent, for another ictus falls upon the accented syllable: ⏑ ⏑ ⏑; but is due, *first*, to the fact that this ictus gives the very obscure ultima too much stress, and, *secondly*, to the fact that the prefixing of so long a word to a shorter one is disagreeable. Consequently this ictus is rare in Greek as well as Latin. Moreover, although in Greek a word which forms a *first paeon* not unfrequently has ictus on the penult ⏑ ⏑ ⏑ ⏑, as ARIST. Birds, 657:

καὶ Μανόδωρε λαμβάνετε τὰ στρώματα,

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still, this ictus is extremely rare if the ultima is elided so that the arsis belongs to two words. In the Birds there is one instance of this, v. 1123:

*εἰ μὴ παρέξει τ ἀμπόρι' ἀνεῳγμένα,*

where even long words are combined. There is in the Birds also one example of ictus on the ultima of an ordinary dactylic word, v. 202:

*δευρὶ γὰρ ἐσβὰς αὐτίκα μάλ' ἐς τὴν λόχμην,*

where *μάλα*, being placed after *αὐτίκα*, needs to be forced into close connection with it; and even in the previous verse cited, the elision (which can be shown to have been total in Greek) already unites the words closely, which can never happen in Latin, as the elided syllable in that language was not entirely suppressed.

Ictus on the penult of a dactylic word, if very weak as in the first foot, is sometimes allowed, as in PLAUT. Cas., 'vīlícus,' and in the prologue (not by Plautus) 'fīlíus.'

In words which form an antibacchius, the conflict or rather the discord is greater than in trochaic words, when the ictus falls on the ultima, ⏑́ — ⏑́; for, *first*, another ictus must also immediately precede the accent, and, *secondly*, the word is longer and more cumbersome, so to speak, and so is less adapted to being closely connected with a succeeding word. In fact, the second reason alone is quite sufficient to account for the exclusion of ictus from the ultima of such words; for the same law is observed in Greek verses. In the Birds there is not a single example of this ictus. To illustrate the relative usage in Latin and Greek verses with regard to the remaining forms just discussed, I here present a comparative view of the Birds and Casina. In the Birds, which contains nine hundred and thirty Iambic trimeters, we find the following: — ⏑́, 2; — ⏑̀ ⏑́, 1; — — ⏑́ ⏑, 7; — ⏑ ⏑́ ⏑, 12; — ⏑ ⏑́ (⏑), 1. The form — ⏑́ in both examples represents a word closely connected with the following word: v. 1465, *οἷσί τε* (— ⏑́, ⏑) which is hardly a fair example at all; v. 1693, *ἀλλὰ γαμικὴν κτέ.* (— ⏑́ | ⏑ ⏑ ⏑́). In Casina, where there are two hundred and eighty-six Iambic trimeters,

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we find — ⏑́ ⏑, 1 (in the first foot of v. 635, 'vilícus'); — ⏑́, — ⏑ ⏑́, — ⏑ ⏑́ ⏑, none at all. In the prologue — ⏑́ ⏑, 1, in the first foot, 'filíus.' In TER. Phorm., where there are six hundred and seventeen Iambic trimeters, we find — ⏑́, 3, all in the first foot; — ⏑́ ⏑, — ⏑ ⏑́, — ⏑ ⏑́ ⏑, none at all. These statistics demonstrate clearly that such ictuses were avoided in Latin; for words of these forms are not so much rarer in Latin than in Greek, and accident favors these ictuses no more in Greek than in Latin. Still, I have found in other plays of Plautus a few examples of most of these ictuses, unless the verses are corrupt: a point which I had no means of investigating. But the whole investigation leads to the conclusion that the ictus on the short ultima of long words was avoided more on account of the undue stress thus given to the syllable and close union of long words with other words, than on account of discord between ictus and accent.

The form — ⏑ ⏑ ⏓ will be treated along with ⏑ ⏑ ⏑ ⏓.

3. The law affecting words composed of short syllables is this: *The ictus must not fall on the syllable which immediately follows the accent, but may fall with perfect freedom on the syllable preceding the accent;* for the slight elevation of voice which accompanies ictus is extended to the second mora of the arsis and so includes the accent; so that this ictus really harmonizes better with the accent than an ictus does which falls on the accent and extends itself to the next syllable. The ultima, for reasons already given and not pertaining to accent, is not disposed to receive the ictus.

a. (α) An ictus on the ultima of a word forming a pyrrhic (⏑ ⏑́) does not produce much discord, for these words are often almost without accent, and sometimes are employed as monosyllables, and can readily be connected closely with a word following. Hence we need not be surprised to find instances like 'suós amor' (⏑ ⏑́ ⏑ ⏑) in PLAUT. Pseud. 206; for this word frequently suffers synizesis, 'sūos,' from which it is evident that the word may either be deprived of accent entirely, or else the accent may be removed to the second syllable. But as the words themselves are not very numerous,

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and there is still some aversion to arsis divided between two words, and the form ⏑, ⏑́ ⏑ for other reasons is better adapted to the verse, the examples of ⏑ ⏑́ are rare. They are also rare in Greek verses where an influence of accent is out of the question. In the Birds I find only four examples, as v. 71, ὅτε περ, v. 76, ὅτε μὲν, etc., where the words are very closely connected, or the second is even an enclitic. In 'Casina' there is one example, found in the first foot of v. 460:

homínem amatórem ullum ad forúm procederé,

where a tribrach has become a pyrrhic by elision. In Ter. Phorm. there *seems* to be one example, iv. 3, 49:

set mi opus erát, (ut aperte tíbi nunc fabulér);

but who can believe that the ultima of 'erat' was combined in arsis with 'ut' across the pause and parenthesis? It is clear that 'erāt' is here an iambus, the parenthesis and pause justifying the retention of the original quantity of the ending *-at*. The next foot is, then, an anapaest.

(β) The fact that the ictus very rarely falls on the ultima of a word forming a tribrach (⏑ ⏑ ⏑́), is not to be attributed to any influence of accent, for the accented syllable forms the second mora of the preceding arsis, so that there is no conflict: — ⏑́ ⏑ | ⏑ ⏑́; but it is rare because it forces two consecutive arses to be resolved, thus: ⏑́ ⏑ ⏑ ⏑́ thus prefixing *two* words, as it were, to a third one, whereas we have seen that *one* long word so prefixed is unpleasant. Here, again, the Greek usage disproves the influence of accent; for in the Birds there is not a single example. In trochaic verses this resolved arsis does not force the resolution of another, and so some examples of it are found: ⏑́, ⏑ ⏑ | ⏑́ ⏑ ⏑ (but hardly ⏑́, ⏑ ⏑ | ⏑́ ⏑ —). In iambics, therefore, it can only occur when we have a proceleusmatic | ⏑ ⏑ ⏑́ ⏑ | —, and examples of this may be produced, especially in the first foot.

(γ) A word forming a proceleusmatic (⏑ ⏑ ⏑ ⏑) very rarely receives the ictus on the ultima, because, *first*, it unites a long word closely with another; *secondly*, it imparts too much stress to a weak ultima; and *finally*, so many consecu-

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tive short syllables are unpleasant. Hence we have either ⏑́ ⏑ ⏓ (ultima long), or ⏑́ ⏑ (⏑), ⏓ (ultima elided). (Cf. Cas. 44 (Gepp.) and Men. 361).

b. (*a*) The general law I stated, relating to influence of accent, forbids ictus to fall on the penult of a word forming a tribrach: ⏑ ⏑́ ⏑, that is, ⏑ ⏑́⏑. But some examples, especially in the first foot, can be found. The only example I have seen in an even foot is in the fourth foot of PLAUT. Men. 876:

qui vi me cógunt ut valídus insaniám:

a verse which some have tried to amend, and whose authority Ritschl for some reason rejects, although Corssen says he recognizes it (Aussprach., Vokal., Beton., II. p. 994).

In Casina, while ⏑́⏑ occurs thirty times and ⏑́ — thirty-three times, ⏑ ⏑́⏑ does not occur once, and ⏑ ⏑(⏑), ⏑ only once, and that in the first foot. In the Birds of Aristophanes, ⏑ ⏑́⏑ occurs thirty times, and ⏑́⏑ ⏓ sixty-four times; or, to omit the form ⏑́ ⏑ —, we have ⏑ ⏑́⏑ just as frequent as ⏑́⏑ ⏑. That is, we have the following relations:

| | | | |
|---|---|---|---|
| Latin | ⏑́⏑ ⏑ | - - - - | 30 |
| | ⏑ ⏑́⏑ | - - - - | 0 |
| Greek | ⏑́⏑ ⏑ | - - - - | 30 more or less. |
| | ⏑ ⏑́⏑ | - - - - | 30 |

Of the thirty examples of ⏑ ⏑́⏑, twelve, indeed, immediately follow incision in the middle of verses, where they cannot occur so frequently as in Latin verses, as has been shown. But to omit these twelve entirely, the remaining eighteen are nearly all in the second foot, where the scheme of the verse, though hardly so favorable as the scheme of Greek verses, still is not so unfavorable to this ictus as to account for the vast difference of usage. Hence, we must conclude that this conflict between ictus and accent was carefully avoided by the Roman poets. To this conclusion the objection may be made, and has been made, that such words as 'meăque,' 'egŏne,' which have the accent on the penult, have the ictus on the penult just as rarely as other ordinary words which form a



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tribrach. This fact we must admit; but it can be explained without invalidating the law I stated. Nor is it necessary to deny that these words were accented on the penult, as some distinguished scholars have done. Langen says that Diomedes was the first to attribute such an influence to enclitics in Latin, and thinks that he and others who succeeded him imitated the Greeks in this matter. But, in the *first* place, we can hardly believe that such men as Priscian were so deaf or stupid as to be unable to recognize the accent of their own words. *Secondly*, they could not have imitated the Greeks in this view, because the Greek enclitics did not produce this effect, as will appear from a comparison of 'méa,' 'meăque,' with δόρυ, δόρυ τε. *Thirdly*, Diomedes seems to have been the first and only one to attribute "*intentio*" to accent, and Langen accepts this as authority for stress, although Diomedes could have taken the expression from Greek writers, and that too without its meaning stress. Now, if Langen is willing to believe Diomedes in this, why does he not believe him and others in another statement, which is of itself probable and cannot be borrowed from the Greeks? *Finally*, it may be admitted that Diomedes was right, but claimed that at his time the accent in such words had shifted its position; that the classic '*méaque*' had become '*meăque*.' To this we reply: In the space of time that elapsed between Terence and Diomedes it is not probable that the accent should have changed its position in that way, for the accent was more tenacious of its position than of its nature—a fact which we cannot doubt when we consider that even in modern languages derived from it, the accent is, for the most part, still on the Latin tone-syllable, and the same is true of modern Greek compared with ancient, and all admit that the Greek accent has changed its nature, and I think I have established the same in regard to the Latin accent. But Langen denies that accent could have changed its nature so as to possess "*intentio*" at the time of Diomedes, unless it had it already at the time of Terence. How, then, could it have changed its place, especially as in so doing it would have left its customary and legitimate position (´ ◡ ◡) and taken up an unusual and

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strange one (◡ ◟ ◡) hitherto unknown to the language? A further objection is based on the fact that some of the grammarians assert that 'atque' has no accent, from which the conclusion is drawn that '-que' had not the power of imparting a new accent to a word. But if we accept the statement of the grammarians about 'atque,' why not accept their statement about 'meăque'? Moreover, their statement about 'atque' shows that they were not blindly, or rather deafly, following the Greeks, for the Greeks, in similar combinations, employ the accent, as in *εἴγε*, *οὔτις*, *ὅδε*, *ὥσπερ*, etc.

I am aware that these arguments do not establish beyond a doubt that such words were accented on the penult; but if they were not, the exclusion of the ictus from that syllable is all the more easily explained, or rather needs no explanation at all. But I have attempted to prove that the accent was on the penult of such words, merely because I believe it is true; and I can explain the exclusion of the ictus from that syllable without resorting to the arbitrary assertion that in this one particular the Latin grammarians did not know what they were talking about.

Assuming, then, that the accent was on the penult, 'meăque,' we offer the following explanation: *First*, the Romans avoided conflict with accent, chiefly because of the uniformity and fixedness of the position of the tone-syllable; but in the words under consideration, the normal position is departed from, so that the main cause of avoiding conflict is removed. *Secondly*, since the great majority of words forming a tribrach (◟ ◡ ◡) received the ictus on the first syllable, the mere force of habit would cause the poets to treat *all* in the same way. But, *lastly*, there is really a worse conflict when the ictus falls on the accent of such words, than when it falls on the first syllable. For, when on the penult, it includes the ultima, which is always a low-toned enclitic, whilst, when placed on the first syllable, it imparts due stress to the stem-syllable, and carries over some elevation to the tone-syllable, as it is the second mora of the arsis.

But examples of ictus on the penult of such words are not entirely wanting in Plautus and Terence (Langen, Philol. XXXI.), and other dramatic writers, as CAECIL. 232 (R.):

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egónc quid dícam? quid velím? quae tu omniá.

($\beta$) Ictus is not tolerated on the penult of a word forming a proceleusmatic (⏑ ⏑ ´ ⏑). Not only is there an intolerable discord between ictus and accent, but also this ictus presupposes the close connection of the word with the one preceding, by means of resolved arsis, thus: — ⏑´⏑ | ⏑ ⏑´⏑, and forces five short syllables to come consecutively. The ancients, both Greeks and Romans, were accordingly in the habit of causing the ultima of such words to be lengthened by inflection or position, or to be removed by elision before a long vowel: ´⏑ ⏑ —´, or ⏑´⏑ ⏑(⏑), —´. Consequently, if I mistake not, there is not a single instance of ⏑ ⏑ ´ ⏑ in the Birds—certainly none in Casina or Phormio.

c. Now we come to a difficult question. According to Langen, words of four short syllables in Plautus have the ictus eleven times on the *first* syllable to once on the *second*—in Terence seven times to once; that is, we have the following relations:

In PLAUTUS, ´⏑⏑⏑ : ⏑´⏑⏑ = 11 : 1;
" TERENCE, ´⏑⏑⏑ : ⏑´⏑⏑ = 7 : 1.

Hence, ´⏑⏑⏑ in Pl.: ´⏑⏑⏑ in Ter. = 11 : 7.

Bentley, Ritschl, Langen, and others hold that such words in the times of Plautus had the accent on the fourth syllable from the end, but that at the time when Terence wrote, this accent had already begun to vacillate between that syllable and the antepenult. Now, Georg Curtius accepts, or at least formerly accepted, the theory of Langen and others that the Latin accent had more stress than the Greek, and was something between the Greek and modern accent; and if this theory be correct, then it must be true that the words under consideration were accented on the fourth syllable from the end. But then, one of the most important arguments employed by Curtius to establish the existence at one time of a Graeco-Italian language, is the fact that in both Latin and Greek the accent is confined to the last three syllables, whilst in Sanskrit, for instance, this is not the case. Now, either this argument must be abandoned, or else the stress claimed by Langen must be considered erroneous; and which we are to do, I don't see

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how there can be any doubt; for it is incredible that between the age of Terence and that of Cicero the accent should have changed so much that Cicero could assert that it was a "*law of nature*" that the accent should never fall farther to the left than the antepenult (Orat. 18, 58); and it seems the more incredible when we consider that the plays of Plautus and Terence were always of great authority and influence, and by being frequently brought upon the stage were kept alive, and so did much to preserve and transmit the language under the form in which they employed it. Yet, Cicero's remark shows plainly that an accent on the fourth syllable from the end was entirely unheard of in his day.

So we may with confidence assume that such words were, like all other words with short penults, accented on the antepenult; and remembering that the accent was chiefly mere elevation of tone, we shall be able to find a satisfactory explanation of the tendency of ictus to fall on the syllable before the accent.

(*a*) First let us compare the Greek usage. In the Birds there are seventy-six words of this sort, of which forty-eight have the ictus on the first syllable, and twenty-eight on the second: ⏑́ ⏑ ⏑ ⏑, 48; ⏑ ⏑́ ⏑ ⏑, 28. Of these twenty-eight examples, twelve occur immediately after incision in the middle of the verse, which incision we have already seen to be, for other reasons, less frequent in Latin than in Greek. Hence we shall not be far from the truth if we assume that in the verses of Aristophanes modified to suit the Latin scheme, there would be two ictuses on the first syllable to one on the second: ⏑́ ⏑ ⏑ ⏑ : ⏑ ⏑́ ⏑ ⏑ = 2:1. Most of the examples of ⏑ ⏑́ ⏑ ⏑ occur in the second foot, where they can readily occur in Latin as in Greek verses, for the scheme ⏑̄ ⏑́, | ⏑ ⏑́ ⏑ | ⏑̄ || does not interfere with caesura. Therefore Plautus has about five times, and Terence about three times as many ictuses on the first syllable as Aristophanes. This could not happen by chance.

(*β*) The causes of this seem to me to be the following: *First*, the elevation of voice, which accompanies ictus, extends itself to the second mora of the arsis, ⏑́ ⏑ ⏑ ⏑; and as the

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accent was elevation without any considerable stress, it harmonizes very well with the element of ictus on this second mora of the arsis. But when the ictus fall on the accented syllable, ⏑ ⏑́ ⏑, it extends itself to the syllable following the accent, which is objectionable; so that, in fact, the ictus on the syllable preceding the tone-syllable harmonizes with the accent better than an ictus on the tone-syllable itself.

*Secondly*, in all these words the first syllable is either the root, as in '*hab*uerit,' '*fam*ilia' (cf. 'famulus'), or a prefix which distinguishes compounds from each other, or indicates their relations to their primitives, as in '*ab*ierit,' '*red*ierit'; '*in*hibeo,' '*dir*ibeo'; wherefore these syllables, especially as they are short and unaccented, were pronounced with a certain stress, in order that they might retain their significance. As the modern accent includes stress, *it* is employed for this purpose, and a secondary accent remains on the primitive word in compounds, as in '*ún*wahrscheinlich,' '*áb*geschrieben'; and so on the root of derivatives, with secondary accent farther on, as in '*ob*rigkeitlich,' '*Klein*igkeiten.' Now, the Latin accent, being elevation, could immediately follow this stress, and the stress itself was more of the nature of ictus than of (Latin) accent; so that those two syllables were exactly suited for the reception respectively of ictus and accent. There is reason for believing that those prefixes, even when they were long, received stress in the early periods of the language, and it is upon this very theory that Curtius bases his explanation of certain vowel-changes, such as are seen in '*fa*cio,' '*inf*icio,' '*perf*icio'; '*sca*ndo,' '*as*cendo,' '*de*scendo'; '*ca*do,' '*cec*idi' ('ce-,' which is a remnant of the root repeated, denotes completed action and so demands emphasis). Changes like '*a*go,' '*e*gi'; '*fa*cio,' '*fe*ci,' do not invalidate this argument, for they are either contractions from earlier reduplicated forms, or else *of a different nature*, involving the lengthening of the vowel, something like λιπ-, λελοιπ-. Corssen, in order to explain those vowel changes, assumes that in very early times the fourth syllable from the end could be accented, and that a long penult did not require the accent to fall on it. But this will not explain all the changes; and even if they

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were so to be explained, we are by no means to suppose that those accents continued to the times of Plautus; and Corssen himself warns us against understanding him to mean this.

The fact that Terence placed the ictus on the antepenult of such words more frequently than Plautus, may be due to several causes. *First*, the stress on the root or prefix was, of course, a relative thing, because to impart a similar stress to the remaining syllable would be to deprive the stress on the root of its effect. Now this very thing happened, when Ennius introduced the hexameter, and restored the Latin endings, or rescued them from destruction by allowing them quantity and ictus in his great epics, which, for ages, were standard authority. In the times of Terence, the endings, therefore, were not so weak and obscure as in the times of Plautus, which is the same as saying that the roots were relatively weaker. But in the times of Terence, that emphasis on roots and prefixes had not yet vanished, nor even in the times of Virgil had it entirely disappeared, for he employs 'tenuia,' 'ariete,' etc., as dactyls, changing the accented vowel almost into a consonant; for surely no one will claim that such words even as late as Virgil still had the accent on the fourth syllable from the end. Nevertheless, it must be conceded that Virgil did not adhere closely to popular pronunciation, and that words like those mentioned could not in any other way be forced into a dactylic hexameter.

*Secondly;* even if the change of stress on root-syllables could not have taken place between Plautus and Terence, the difference of usage between the two is not so great but that it could result from a difference of individual inclination or local usage, particularly when we remember that the ictus ⏑ ⏑́ ⏑ ⏑ was allowed even by Plautus. In a similar manner Euripides indulges in what are termed licenses, which Aeschylus and Sophocles, *writing at the same time*, avoided; and I know men who pronounce whole classes of words differently from most people. Who has not heard people say 'governmĕnt,' 'complimĕnt' (noun), etc.? And I have in mind a distinguished scholar and scientist who gives the first syllable of many words such an emphasis that many persons declare he

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accents them, as '*Vir*ginia,' '*con*gratulate.' Now, remembering how very different were the earlier associations of the African and the Umbrian, we should expect differences in speaking.

*Finally*, the slight difference of usage between Plautus and Terence could happen by chance, although the difference between their usage and that of Greek poets is entirely too great to be accounted for in this way. The total number of ictuses on the antepenult (⏑ ⏒ ⏑ ⏑) is very small, and when we are dealing with things so rare, not much importance is to be attached to a comparison of numbers. To illustrate this, let us assume a case. Let us suppose two Latin poems to have been discovered, and the question to have arisen whether they were both from the same author. In one, which contains, let us say nine hundred and eight Heroic hexameters, there are ten examples of conflict between ictus and accent in the sixth foot, and six such examples in the fifth foot; in the other, which contains, say, eight hundred and eighteen verses, there are only three examples of conflict in the sixth foot, and none at all in the fifth. If we attribute importance to so wide a difference, where, however, all the figures are small, we should decide at once that the poems were from different authors; but the poems I have described are the tenth and the ninth books of Virgil's Aeneid. At the opening of the discussion of this question, I assumed the data furnished by Langen and compared the ratio between the two sorts of ictus in Plautus, with the same in Terence. If we were to proceed in this way with the two books of Virgil just mentioned, we should find the ratio of conflicts to harmonies to be as follows:

Book x., 6th foot, 91 : 1; 5th foot 151 : 1.
" ix., " 273 : 1; " ∞ : 1;

a result which is absurd, for it is evident that in both books the conflicts in question were to a great, and about the same, extent avoided, while this result gives us for the harmonies in the sixth foot,

x : ix : : 1 : 3,

and for the fifth foot,

x : ix : : 1 : ∞;

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whereas the true relation may be found by omitting the very rare conflicts in both and reducing, and we have, for the sixth foot,

x. : ix. : : 135 : 136,

and for the fifth foot,

x. : ix. : : 272 : 273,

and this is about the true relation subsisting between the two books. Now let us compare Plautus and Terence in the same way. Out of ninety-six instances in Plautus, we have

⏑ ⏑́ ⏑ ⏑, 88 + ⏑ ⏑́ ⏑ ⏑, 8 = 96, for $\frac{88}{8}$ = 11, the ratio in Plautus.

In Terence,

⏑́ ⏑ ⏑ ⏑, 84 + ⏑ ⏑́ ⏑ ⏑, 12 = 96, for $\frac{84}{12}$ = 7, the ratio in Terence.

Now, as the small numbers 8 and 12 do not differ so much from each other but that accident could modify them materially, we have as much right to assume 88 : 84, or 22 : 21 as the ratio between Plautus and Terence, as to assume 11 : 7; and, as I have shown from Virgil, this is the much more rational method of the two. Hence, it is quite possible that the whole difference resulted from accident; for it would have been surprising to me if they had not differed to that extent from each other.

d. Words of the form — ⏑ ⏑ ⏓ receive the ictus somewhat more frequently on the antepenult than on the syllable preceding it, which is easy to understand; for, *first*, if the ultima is short and the ictus falls before the accent, another must fall immediately after it, ⏓ ⏑ ⏑́ ⏑, which is not allowed. In the Birds there are eight examples of this ictus; in Casina and Phormio not one. This cannot be attributed to any peculiarity of the Latin scheme, nor could chance have caused it; for the same relations exist between *all* the Greek and Latin plays I have examined; and if any one will write a few verses admitting resolved arsis, he will find himself employing this ictus by no means rarely. Accordingly, the few verses which I have appended, as they were originally written, contained two instances of it:

tum ego vósque, nosque *décidímus* omnes humí,
dum insultat ín nos próditió trucissimá.



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But as soon as I wrote these verses, I suspected that there was something wrong, because they sounded unnatural and unusual; and it was this which first led me to examine the subject of this particular sort of ictus. These two verses, and these alone, I afterwards changed on this account; but the changed forms exhibit as many conflicts as these (but they are admissible conflicts), so that the whole number of conflicts in the verses is not at all modified by the correction. Even in Seneca, where words containing long syllables often show conflict in the odd places, you will never find this ictus (— ⏑ — ⏑), but always either — — ⏑ ⏑ or — ⏑ ⏑ —.

*Secondly;* if the ultima is long, we find either — ⏑ ⏑ —, or, what is more frequent, — — ⏑ —. But here no influence is to be attributed to accent. The form — ⏑ ⏑ — is rarer for the reason that it is not so well adapted to the scheme of the verse; for it is not possible for the anapaest in — ⏑ ⏑ — to occupy the first place, and in the second, where the strongest ictus falls, it is unpleasant on an ultima unless the foot is pure; while if it occupies the third place, it prevents caesura and causes incision in the middle of the verse. There only remain, then, the fourth and fifth places, the former of which gives the same difficulty in a less degree that the second place gives, while in the fifth place nearly all the examples occur. So that it is manifest that the ictuses — ⏑ ⏑ — are not to be expected as often as — — ⏑ —.

*Thirdly;* the first syllables of these words, being long, have sufficient emphasis without ictus, and if the ictus falls on it, it does not extend itself to the accented syllable as in — ⏑ ⏑ ⏑. Frequently, however, we have elision, whence we obtain — ⏑ ⏑ (⏑), —, which is suited to several places in the verse.

*Fourthly;* in the Latin dramatists, the anapaest is less frequently employed anyhow than the dactyl, even if we omit the examples contained in the words under discussion. In Andria these relations exist:

— — ⏑ : ⏑ ⏑ — = 3 : 2,

but in the Birds,

— — ⏑ : ⏑ ⏑ — = 1 : 2.

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This circumstance alone would make — ⏑́ ⏑ | ⏑̄, ́ | more frequent than ́ | ⏑ ⏑ ́ |.

Among all the early dramatic writers, I find nearly the same usage with respect to ictus on words of the forms — ⏑ ⏑ ⏑̄ and ⏑ ⏑ ⏑ ⏑̄. In Ribbeck's Edition, I find the following relations:

| | | | | | | |
|---|---|---|---|---|---|---|
| Livius Andronicus, | ⏑́ ⏑ ⏑ ⏑ | - - - - | 1, | ⏑ ⏑́ ⏑ ⏑ | - - - - | 0 |
| Statius Caecilius, | " | - - - - | 13, | " | - - - - | 2 |
| Sextus Turpilius, | " | - - - - | 7, | " | - - - - | 1 |
| Lucius Afranius, | " | - - - - | 14, | " | - - - - | 0 |
| Lucius Pomponius, | " | - - - - | 7, | " | - - - - | 1 |
| Novius, | " | - - - - | 7, | " | - - - - | 0 |
| Laberius, | " | - - - - | 12, | " | - - - - | 1 |
| Syrus, | " | - - - - | 3, | " | - - - - | 0 |

Now, if we add to this list:

| | | | | | | |
|---|---|---|---|---|---|---|
| Plautus, | ⏑́ ⏑ ⏑ ⏑ | - - - | 11 × n, | ⏑ ⏑́ ⏑ ⏑ | - - - - | 1 × n; |
| Terence, | ⏑́ ⏑ ⏑ ⏑ | - - - | 7 × n, | ⏑ ⏑́ ⏑ ⏑ | - - - - | 1 × n, |

who will deny that the difference between the two may be the result of accident?

In the so-called "Syri Sententiae," which are of late origin, I find ⏑́ ⏑ ⏑ ⏑ not much more frequent than ⏑ ⏑́ ⏑ ⏑.

§ 6. Briefly to sum up the whole matter, we announce this law: Ictus must not fall on the syllable which immediately follows the accent, except when the arsis is long and the accented syllable short, which happens only in iambic words, as ⏑ ́; and the law is especially stringent when another ictus would also immediately precede the accent, as ́ — ́ or ́ — ⏑́; but the law does not apply to the odd places except when short syllables are concerned.

Many minutiae cannot be included in a brief summary.

I do not assert that the poets ever took conscious notice of the accent; but I have no doubt that if their attention had been called to it by the question why they avoided certain combinations, they would have seen that it was unpleasant relations of ictus to accent that they were avoiding.

These laws were not of an absolute and inviolable nature, but were rather aesthetic; consequently occasional violations of most of them are sometimes found in verses which cannot be changed, or, at least, which there is no MS. authority for changing.

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Here the discussion must be closed. If nothing else worth while has been developed by it, one fact has certainly been demonstrated: that in metrical questions the indolent method of regarding a *post hoc* always as a *propter hoc* cannot be relied upon.

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MARCUS ANTONIUS (loquitur).

Lacrimas si habetis, nunc parate fundere.
Vidistis hanc togam, optimi: tempus bene
Memini quo eâ primum amicuit se Julius:
Æstate erat sub vesperum in prætorio
Illo die quo Nervios devicerat.
Spectate: Cassi sica perforavit hic:
Videte quantam fecerit Casca invidus
Scissuram: amatus Brutus hic ictum dedit,
Et cum nefandum ferrum is inde evelleret,
En sanguis ipse Cæsaris secutus est,
Tamquam foras exiret ut pernosceret
Num tam impulisset Brutus ingrato modo.
Nam Brutum amores scitis omnes Cæsaris
Fuisse: quantum amarit æstima, O deus!
Hæc plaga crudelissima omnium fuit;
Nam cum videret Cæsar ipsum pungere,
Ingratus animus proditorum fortior
Pervicit armis: tum cor amplum rumpitur,
Togaque voltu procidit tecto ad pedes
Atro cruore defluentis interim
Signi usque Pompei ipse Cæsar maximus.
Et quantus ille casus, O cives, fuit!
Ego vosque et omnes cecidimus nos tunc humi
Dum insultat in nos sæva proditio insolens.
Hei fletis! atque cerno iam sentire vos
Vim misericordiæ; hæ benigno ex fonte sunt
Guttæ: pii, quid! Cæsaris nostri togam
Scissam videntes fletis? Huc spectate nunc:
Hic ipse—miserum!—lacer ab infidissimis!

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Ὁ Κάτων τάδ' ἑαυτῷ διελέχθη, Φαίδων Πλάτωνος ἀναγνούς·

Ἀληθὲς εἶναι τοῦτ' ἀναγκαῖον· Πλάτων,
καλῶς λέγεις· πόθεν γὰρ, εἴγ' ἄλλως ἔχει,
ἥ τ' ἐλπὶς ἥδε καὶ ποθῶν ὅδ' ἵμερος
τῆς ἀθανασίας; καὶ πόθεν δέος τόδε
κρυπτὸν, τὸδ' ἐντὸς δεῖμα γίγνεσθαί ποτε
τὸ μηδέν; ἐκπληγεῖσα δ' ἡ ψυχὴ φθορᾷ
τί δὴ παρ' αὐτὴν χάζεται φοβουμένη;
τὸ θεῖόν ἐστιν ἐν φρεσὶν κινούμενον,
αὐτός τε μέλλον οὐρανός τι δεικνύων
καὶ τοῖσιν ἀνθρώποισιν ἐκφαίνων σαφῶς
τὴν ἀϊδιότητ' — ὦ νόημα προσφιλὲς
καὶ δεινὸν, ὡς μὲν πολλὰ καὶ παντοῖα χρὴ
ἡμᾶς ἀπείρατ' ἐκπερᾶν, οἵας δὲ χρὴ
μεταλλαγάς καὶ καίν' ὁράματ' εἰσορᾶν.
πλατεῖα κεῖται κἄμετρος προὐμοῦ θέα,
ἀλλ' ἔστ' ἐπ' αὐτῇ καὶ σκίασμα καὶ νέφος,
σκότος τε. τοῦδε γοῦν ἐγὼ μὲν ἕξομαι·
ὕπερθεν ἡμῶν εἴγε που ἔστι κύριος
(καὶ μὴν ἐν ἔργοις πᾶσιν ἡ φύσις λίγα
ὡς ἔστιν αὐδᾷ) τοῖς καλοῖς οὐκ ἔσθ' ὅπως
οὐχ ἥδεται μὲν, οἷς δ' ἐκεῖνος ἥδεται
εὐδαιμονεῖν χρὴ ταῦτά γ', ἀλλὰ ποῦ; πότε;
οὗτος γὰρ οὖν ὁ κόσμος ἐστὶ Καίσαρος.
ἤδη δὲ κάμνω συμβαλών· νῦν παυστέον.
καὶ κάρτα δίσσ' ὅπλ' ὧδ' ἔχω· μόρος τ' ἐμὸς
ζωή τε, πῆμα καὶ λύσις, πάρεισί μοι.
ὁ μέν μ' ἄγει πρὸς τέρμα, καὶ ταῦτ' ἐν βραχεῖ,
ἡ δ' ὡς θανοῦμαί γ' οὔποτ' ἐμφανῶς λέγει.
ψυχὴ δ' ἔχουσα τὴν ὕπαρξιν ἀσφαλῆ
ξίφει γελᾷ σπασθέντι καὶ θαρσεῖ δ' ἀκμήν.
τὰ μὲν γὰρ ἄστρα φθίσεται, τόν θ' ἥλιον
αὐτὸν σκοτώσει δὴ τὸ γῆρας, εἴς τ' ἔτη
δῦναι πέπρωται τὴν φύσιν· σὺ δ' ἄφθιτον
ἥβην φυλάξεις, ἀβλαβής τ' ἐν τῇ μάχῃ
ἤν τοί ποτε στοιχεῖα συμβαλεῖν χρεὼν,
κτύπῳ τε κόσμων κοὐσίας ναυαγίᾳ.

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## VIII.—*The Algonkin Verb.*

By J. HAMMOND TRUMBULL.

W. von Humboldt maintained that genuine grammatical forms are found only in the "complete" inflectional languages, namely, Indo-European and Semitic, and that neither agglutinating nor incorporating languages can "really attain to the expression of a true conception of one such form."* His classification of the "less complete" languages is founded chiefly on differences in their treatment of the *Verb.* In some — the Malay-Polynesian family, for example, — the peculiar function of the verb has *no* characteristic expression; in others, this function is indicated by pronominal affixes. In latter class Humboldt placed "the Mexican, the *Delaware* and other American tongues." His illustrations of the mode of structure of these languages are mostly taken from the Mexican, the Maya of Yucatan, and the Yarura of New Grenada. For his knowledge of the Algonkin languages, he relied mainly on Duponceau's Correspondence with Heckewelder and translation of Zeisberger's Delaware Grammar, and unfortunately he adopted Duponceau's mistaken notion of American polysynthesis — as effected by "putting together portions of different words, so as to awaken at the same time in the mind of the hearer the various ideas which they separately express." †

Professor H. Steinthal, in his psychological classification, regards the American languages as "formless." Selecting as types the Mexican and the Greenlandish, he decides that the former "has, in its method of word-making, formed nouns, but *no true verbs*," and that the so-called Mexican verb is a noun with a predicate-prefix: e. g. *ni-tlàtlakoāni* 'I, sinner,' for 'I sin;' *ni-kwalli* 'I, good,' for 'I am good.' ‡ In the Greenlandish, Professor Steinthal finds that "the sen-

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* *Ueber die Kawi-Sprache*, I. (Einleitung,) cxlvi.

† Preface to Zeisberger's *Grammar*, p. 20; cited in *Kawi-Sprache*, I. (Einleitung,) cccxxxii, ff.

‡ *Charakteristik d. hauptl. Typen d. Sprachbaues*, 216, 218.

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tence is not founded on subject and predicate," but centres in the object; "the verb is without a copula, and the noun without subjective character;" and, in the process of incorporation, "the objective relation is regarded rather as a peculiar sort of *possession*, and the *possessive* which is also a *substantival* relation dominates the phrase." "The Object," he says, generally, "presents itself so forcibly to the consciousness of an American, that he apprehends the existence of the Subject only in its relations to the Object and hence overlooks the earlier and more important relation of the Subject to the Activity." *

Professor Fr. Müller, in his memoir on the grammatical structure of the Algonkin languages (1867) and more recently in his *Allgemeine Ethnographie* (1873) concedes true verb-forms to the Mexican and Dakota languages, but denies them to the Algonkin and Iroquois. "The Algonkin languages," he says, "do not distinguish the noun and verb from each other. From the etymological point of view they know only a *noun*, which, when equipped with possessive-suffixes, corresponds to the expression of our verb." He regards "the want of distinction between *subjective* and *possessive* pronominal-elements" as an essential characteristic of these and many other American tongues. "They recognize only the *dependent* relation, and hence are in a position to form nouns but no *verbs*." The so-called verb is a "nomen actionis," with or without possessive pronominal affixes: for example, Cree *ni-pimootan* ('I walk') resolves itself into 'my walking'; Algonkin *ni-sakih-a* ('I love him') is 'my love to him'; *ni-sakih-ik* ('he loves me') is 'my being-loved by him'; and *ni-sakih-igo* ('I am loved') is 'my being-loved by somebody.'†

M. Abel Hovelacque, follows Fr. Müller, in making the verb, in Algonkin and many other American languages, "nothing but a noun accompanied by suffixes denoting possession."‡

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* *Ibid.*, 226.

† *Der grammat. Bau der Algonkin Sprachen*, 136, 139: *Allgem. Ethnographie*, 275, 276. [Compare his *Grundriss d. Sprachwissenschaft*, I. (Wien, 1876,) 124.]

‡ *La Linguistique* (Paris, 1876), p. 117.

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A very different conclusion was arrived at by Bishop Baraga, after twenty years' study of the Chippeway, an Algonkin dialect: "This language," he wrote, "is a language of *verbs*. . . . *All* depends on the *verb*. . . . Where other languages will employ a substantive, the Otchipwé uses a verb."* So, Father Lacombe, who has lived as many years among the Crees and has published an excellent grammar of their language, declares that it is "nothing but a language of verbs."† And Mr. Joseph Howse, in his Cree Grammar (p. 15) asserts that "the Indian verb, as respects its *nature* or essence, may, without hesitation, be said to be strictly analogous to the part of speech in European languages, bearing the same name; viz., as *predicating* being, or manner of being or acting." In fact, no one who has learned to speak and write an Algonkin dialect and who has studied its grammatical structure without previous knowledge of its "inner form," as seen by the light of ethno-psychology, has suspected that the Verb — on which "all depends," and to which all other parts of speech are resolvable, — is really *no* verb, but only a disguised noun; that it is not predicative; that its pronominal affixes are merely possessive, not subjective; and that "the idea of time and mode is altogether absent from it."

I propose, in this paper, to examine the grounds on which the genuineness of verb-forms in Algonkin languages has been denied. Before entering on this examination it will be necessary to notice some peculiarities of the grammatical structure of these languages.

1. The distinction between *animate* and *inanimate* objects — or, more exactly, between objects *regarded as* belonging to the one or the other of these two classes — is never lost sight of by the Algonkin speaker. It is constantly recognized, not only in the formation of the plural of nouns but in every expression of relation. It modifies every conjugation-form of the verb, according as the subject or the object is animate or inanimate.

2. Formal distinction of *number* seems to have been earlier established for animate than for inanimate nouns. The char-

* *Otchipwe Grammar*, pp. 15, 28.

† *Grammaire Crise*, p. 53.

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acteristic of the animate plural (*k*, or its sonant, *g*) is the same in all Algonkin dialects; but there is much diversity in the forms of the inanimate plural; in the Cree, it adds *a* to the singular; in Chippeway, *n* preceded by a vowel; in Abnaki, *r* or *l*; in Illinois, *ri*; in Massachusetts and Quinnipiac (or Quiripi), *sh*.

3. Every dialect has a law of euphony which regulates the vocal connection of the sign of the plural with a noun in the singular. In the Chippeway and (Nipissing) Algonkin, if the singular ends in a vowel, *k* is added to form the animate, *n* for the inanimate plural; if the singular ends in *k*, the animate plural adds o*k*, the inanimate o*n*; if the singular ends in any consonant except *k*, the animate plural adds A*k*, the inanimate, A*n*.*

ANIMATE NOUNS.

| | | | |
|---|---|---|---|
| Nipiss. Alg., | *makwa*, a bear, | pl. | *makwak*, |
| Chip., | *makwa*, | " | *makwag*, |
| Cree (Western), | *maskwa*, | " | *maskwak*, |
| " (Eastern), | *mŭskwa*, | " | *mŭskwŭk*, |
| Mass., | *mosq*, *mashq*, | " | *mosquag*, |
| Muhhek., | *m'quoh*, | " | *m'quohk*. |
| Nip.-Alg., | *akik*,† a kettle, | pl. | *akikok*, |
| Chip., | *akik*, | " | *akikog*, |
| W. Cree, | *askik*, | " | *askikwok*, |
| Mass., | *ahkŭhk*, | " | *ahkŭhkwog*, |
| Blackfoot, | *ĭski*, *isk*, | " | *isk-īks*. |

INANIMATE NOUNS.

| | | | |
|---|---|---|---|
| Cree, | *sípí*, a river, | pl. | *sīpīa*, |
| Chip., | *sibí*, | " | *sibiwan*, |
| Abnaki, | *sipu*, | " | *sipuar*, |
| Illinois, | *sipiwi*, | " | *sipiwari*, |
| Mass., | *sipu*, *sĭp*, | " | *sipŭash*. |

4. Certain relations which nouns may sustain to each other, or to the principal action of the sentence, are expressed by changes of termination, that is, by *inflection*. Other relations are expressed by prefixes and suffixes which have not, in composition, entirely lost traces of original independence.

*Cuoq, *Études philologiques*, 36.

† This is one of the nouns which is classed as *animate*, in all Algonkin dialects.



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The modifications of the noun correspond so nearly with those of the governing verb, that case-endings are not easily distinguished from conjugation-forms. M. Cuoq says: "Algonkin nouns are *conjugated*, not *declined*;"* and he includes the relations expressed by cases under the general name of "*accidents* which may modify nouns." Of these accidents he enumerates (in the Nipissing-Algonkin dialect) twelve, only four of which can properly be regarded as *cases:* namely, the locative, the possessive, the obviative ("obviatif"), and the super-obviatif ("surobviatif"). In these, the vocal connection of the case-ending with the noun is regulated by the same euphonic law which prescribes the vowel to be used in forming plurals.

The *locative* case indicates the place or object *to*, *at*, *in*, or *on* which the action of the verb is directed. Its formal characteristic varies in different dialects. In the Chippeway, Nipissing, Ottawa, etc., it is *ng;* in Delaware, *nk;* in Cree, *k* or *g;* in Massachusetts, *t*.

The *possessive* case, by which property or possession is expressed more emphatically than by a pronominal prefix, is formed by adding *m*, *om*, or *im:* e. g. (Chip.) *nind aki* 'my land,' 'my country'; *nind akiM* 'my *own* (piece of) land'; *kid ikwe* 'your woman,' *kid ikweM* 'your own woman,' 'your wife'; *nind akíkoM* 'my own kettle'; etc.†

The *obviative* and *super-obviative* are forms of what Mr. Howse (*Cree Grammar*, 125) calls the "possessive or accessory case," which "serves" to distinguish the *accessory* or dependent from the *principal* or leading third person." Algonkin grammar recognizes *three* third persons, singular and plural. When two nouns (or a noun and a pronoun) in the third person are introduced in the same sentence, one as the subject and the other as the object of a verb, the latter takes the *obviative* — or second third-personal — form. When *three*

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**Etudes Philologiques*, 38.

† "The *possessive rank* of nouns, is when the *person* doth challenge an interest in the *thing*. . . . . And it is made by adding the syllable *eum*, or *oom*, or *um*, according to euphonie, unto the affixed noun. For example, *num-Manittoom*, my God; *num-moskeht eum*" [my meadow].—Eliot's *Grammar*, 12.

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nouns animate or pronouns in the third person are used, the third of which stands in a dependent relation to the second, the subject of the verb has the simple form (first third-person), and of the other two, one receives the obviative (second third-personal) and the other the super-obviative (third third-personal) inflection.

The characteristic of the obviative in the Chippeway dialect is *n* (with a connecting vowel); in Cree, *a;* in Delaware, *l;* in Massachusetts (in Eliot's notation) *h* after a vowel. *Inanimate* nouns do not change their terminations in the obviative. The characteristic termination of the super-obviative in Chippeway is *ni* (with connecting vowel); in Western Cree, *iyiwa* for animate nouns (sing. and pl.), *iyiw* (sing.) and *iyiwa* (pl.) for inanimate.

When the object of a verb is in the obviative, *the verb itself receives the obviative inflection.*

How important a place these "accessory cases" fill, in the construction of Algonkin sentences, will be seen by a few examples. The first are in the (Nipissing) Algonkin dialect, the others from the Chippeway (of Baraga):

1st 3d person, *eebik asapike*, the spider makes a web.
" " *ni-nisa eebik*, I kill a spider.
Obviative, *Jan o-nisan eebikon*, John kills a spider.
1st 3d person, *Ki takona wagosh*, You catch a fox.
Obviative, *Simon takonan wagoshan*, Simon catches a fox.
Super-obviative, *Bazin o-pakitean Pienan o-kwisisini*, Basil he strikes Pierre's son.

Here 'Basil' is in the *first* third-person, 'Pierre' (Pien) in the *second*, and 'his son' in the *third*. If the proper names are omitted, the distinction of these persons is still preserved by the verb form and the case-endings: *o-pakitean o-kwisisini* 'he (1st 3d p.) struck-him (2d 3d) his-son (3d 3d.)*

* Cuoq, *Etudes philologiques*, 43. In the Yakama (Sahaptin) language, "if the governing substantive is itself governed by a verb, the substantive which it governs takes the compound termination, and is put in the same case as that which governs it. The compound terminal sign is only the union of the dative, or the accusative, with the genitive." Example: *miawar-nmi-ow nit-iow nes winasha* 'I am going to the chief's house;' in which *miawar* 'chief' takes the affix of the genitive (*nmi*) and dative (*iow*), and *nit* 'house,' that of the dative only.—Pandosy's *Yakama Grammar*, 29.

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Chippeway:

1st 3d pers. *nin sagia noss*, I love my father.
2d 3d " *kwiwisens o-widokawan* ossan, the boy helps his father.
3d 3d " *Joseph o-gi-odapinan abinódjiian ogini gaie*, Joseph took the young-child (2d 3d p.) and his-mother (3d 3d p.)
3d 3d pers. *Paul o-gi-nissan ossaieian, wiwini gaie*, Paul killed his-brother, and his [brother's] wife.
2d 3d pers. *Paul o-gi-nissan ossaieian, wiwan gaie*, Paul killed his brother, his [Paul's] wife also.

Eliot observed, but did not clearly understand, the use of the "obviative" in the Massachusetts dialect. "There seemeth," he says,* "to be one *cadency* or *case* of the first declension, [i. e.] of the form *animate*, which endeth in *oh*, *uh*, or *ah*; viz: when an animate noun followeth a verb transitive, whose *object* that he acteth upon is *without himself*:" e. g. *neemun namohs* "take up the fish" (Matt. 17. 27), but *quagwashwéhtau*au *mishe namohs*oh "he prepared a great fish" (Jonah, 1. 17), where the noun receives the case-ending of the second third person.

5. All Algonkin nouns whose derivation can be traced are formed either directly *from verbs* or on distinctively *predicative* roots,† and retain much of the nature as well as of the forms of verbs. They express relations of time and manner by affixes and internal change of vowels. Many, including all names of action, have distinct *active* and *passive* forms. They are qualified by adverbs, not by adjectives. The author of *Études Philologiques* scarcely exceeds the fact, in the assertion that "les noms algonquins ne se déclinent, ils se conjuguent." But though the noun so commonly assumes some of the forms it does not usurp the office of the verb. It is always a *verbal*, but cannot — without change of termina-

* *Indian Grammar Begun* (1666), p. 8. Eliot has but one form for the obviative, in the singular and the plural. He wrote, *togkomau ketassoo*toh "he smote the king" (2 K. xv. 5), and *wehkomau nishwe ketassoo*toh "he called three kings" (2 K. iii. 10). So *ayim anogqs*oh "he made the stars" (Grammar, p. 8)— *anogqs* (pl. *anogqsog*) being classed with animate nouns.

† This holds true even with respect to onomatopoeic names. Mass *konkontu* 'a crow' has the predicative termination, 'he *konkonts*'; *quequckum* 'a duck' (an abbreviation, as the plural form shows, of *quequekummaü*) has the characteristic verbs expressing *action of the mouth*: 'he *says quequek*.'

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tion, to express the predicative function — become a *verb*. Examples of several classes of nouns, showing their relations to and modes of derivation from their respective verbs, will be given further on.

6. Analysis of the structure of Algonkin speech exhibits, as its final result, elements exactly corresponding to the *roots* of Indo-European languages; elements which must have "existed prior to the whole development of the means of grammatical distinction, before the growth of inflection, before the separation of the parts of speech," and which "indicated each some conception in entire indefiniteness as concerns its relations, neither viewed as the concrete name of an object, nor as attribute only, nor as predicate; but as equally ready to turn to the purpose of any of the three." * The number of pronominal or demonstrative roots seems to be, relatively, greater than in Indo-European speech. Nearly all the predicative, as well as the demonstrative, are resolvable to monosyllables. Modification of the meaning of a root by the addition of a consonant — with or without a connecting vowel — is a prominent feature of this group of languages. The consonants so employed have, each, a determinable and constant value, independent of the principal roots with which they are associated, and serve as "characteristics" of various forms of conjugation, "indicating the *manner* of being, doing, or acting."† They are properly to be regarded as roots or representatives of *roots*, and not (as Mr. Duponceau and others believed them to be) as mnemonic symbols of *words* which had received grammatical forms.

These uniliteral elements are used as prefixes as well as affixes. Volney ‡ observed in the Miami (an Algonkin) dialect that, "generally, *m* begins all words which denote what is *bad* or *ugly*." Duponceau adopted this statement and drew from it an inference which helped give W. von Humboldt a wrong notion of Algonkin word-making and of American polysynthesis generally: "The mere initial *m* of *machit* 'bad' or

* Whitney, on Indo-European roots, in *Life and Growth of Language*, p. 201.

† Howse, *Cree Grammar*, 37 ff.

‡ *Tableau des États-Unis*, ii. 527.

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*medhick* 'evil' gives," says that eminent scholar,* "a bad and depreciatory meaning to a word." Whatever "bad meaning" a prefixed *m* imparts comes from the root of which *m* is the representative and not from the derivatives *machit*, *medhick*, or *matta* 'not,' or even from the particle *mo* of which *m* is the base.

The most simple forms under which Algonkin roots assume grammatical relations are — the pronouns and a few demonstrative particles excepted — *verb* forms. "The making of a verb is nothing more than the establishment of certain combinations of elements in an exclusively predicative use, the supplying a copula in connection with them and not with others;"† and this is accomplished in Algonkin as in Indo-European speech, by associating a pronominal with a verbal element. The predicative function receive as distinctive expression and the verb-form is as really attained in Chippeway *AB i* (Cree *AP-iu*, Abnaki and Massach. *AP-u*,) 'he stay-s, abid-es, remain-s,' as in Gr. MENει or Lat. *MANet*. It is true that we find in Algonkin verbs — even among those which convey elementary ideas and which, presumably, are of very early formation — considerable diversity in the formal expression of the predicate relation. This diversity is owing partly to the distinction between animate and inanimate subjects, and partly to the want of a *complete* independent verb-substantive. Hence come a great number of conjugation-forms unknown to Indo-European grammar. Baraga reckons *nine* conjugations, for the Chippeway verb, distinguished by the termination of the third person singular of the indicative present; Zeisberger makes eight for Delaware verbs; Howse and Lacombe distinguish seven forms of conjugation of Cree intransitives and the latter gives eight to verbs transitive-animate.

For the purposes of this paper, we need not look beyond the most simple forms — those of verbs neuter (subjective), active-intransitive, and transitive-absolute (without an expressed object).

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* *Die Kawi-Sprache*, i. (Einleitung) cccxxxiii. † W. D. Whitney.

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In the conjugation of intransitive (primary) verbs, the pronominal element, which indicates the predicative function, is *suffixed* to the verbal root, in the *third* person, in direct assertion, and is *prefixed*, in the *first* and *second* persons. In conditional or indefinite assertion, in command and prohibition, the verbal root is without pronominal prefix, and the grammatical persons, singular and plural, are distinguished only by the *terminations*, which retain no trace — except, perhaps, in the first person singular of the subjunctive — of original independence. "The conjugational form of the Cree intransitive verb has"— as Mr. Howse expresses it*— "in the indicative mood [the third person excepted], the *prefixed* nominative of the French; and in the subjunctive, the *inflected* personal termination of the Italian and Latin." The 1st and 2d persons of the indicative are distinguished only by their pronominal prefixes, both having the same termination, which is, usually — in the Cree language, always — *n* (with a connecting vowel: but in some dialects — particularly, those of the Chippeway group — many verbs whose 3d person ends in a vowel, have lost the characteristic termination of the first and second persons; e. g.

| | | |
|---|---|---|
| † Cree, *ap-iü* 'he stays,' | 1st p. *n't ap-in,* | 2d p. *k'tap-in* |
| Chip. *ab-i* | *nind* ‡ *ab* | *kid ab* |
| Mass. *ap-u* | *n't ap* | *k't ap,*— |

though Eliot occasionally uses the forms *n't ap-in*, *k't ap-in* (*nut appin*, *kut appin*, in his spelling). So, Cree *posiü* 'he gets into a canoe, embarks,' (1st p.) *ni posin;* Chip. *bos-i*, *nin bos;* Abn. *poos-u*, *ni poos-i:* Cree, *nip-iü* 'he dies,' (1st p.) *ni nip-in;* Alg. *nip-o*, *ni nip;* Chip. *nib-o*, *nin nib;* Mass. *nŭpp-u*, *n' nŭp* [*nunnup*, El.]; with which compare, Cree

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* *Cree Grammar*, p. 77.

† The authorities on which I have chiefly relied are, for the Chippeway (Chip.), Baraga's Grammar and Dictionary, and for the closely related Nipissing Algonkin (Alg.) Cuoq's *Etudes Philologiques*, etc.; for the Cree, Lacombe's and Howse's Grammars and Lacombe's *Dictionnaire de la langue Crise;* for the Abnaki (Abn.), Rasles' Dictionary (of the Kennibé or Caniba dialect); and for the Massachusetts (Mass.), Eliot's Grammar and his version of the Bible.

‡ "To the pronouns *nin* 'I' and *ki* 'thou,' 'we,' a euphonical *d* is attached when the following verb commences with a vowel."—Baraga's Grammar, 43.

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*nip-aü* 'he sleeps,' (1st p.) *ni nip-ān;* Alg. *nip-e, ni nip-a;* Chip. *nib-á, ni nibá.*

*Imperative.* There is much diversity, in Algonkin dialects, in forms of the imperative. The 2d person plural, in all dialects, ends in *k* (or g). For the 3d person, singular and plural, the Chippeways and Crees use the 3d person of the indicative present, with a prefixed particle. The Massachusetts has special forms for the 2d and 3d persons, in both numbers: Sing. 2d p. ap*sh.* 'sit thou'; 3d p. ap*itch* 'let him sit'; pl. 2d p. ap*ik* or ap*igk*; 3d p. ap*ihettitch.*

The *Subjunctive* "or rather the *Supposing* or *Suppositive* mode," says Eliot (Grammar, 19, 26), "when the action is only supposed to be,—usually flats the first vocal and lays by the *affix*," i. e. the pronominal prefix. Mr. Howse calls this "flattened vowel of the subjunctive, the sign of an *indefinite tense* or time,"—"signifying that the action combined with the attribute is generalized, or rendered indefinite in respect to *time*, and hence, secondly, implying sometimes *custom*, or *habit*, in the subject of the verb" (Cree Grammar, pp. 71, 73). Baraga gives a full account of this "vowel change," which, he says, "embarrasses much the beginning learner of the language" (Otch. Gram. 128–141). The change is not accurately described as a *flattening*. It is in fact a *strengthening* of the first vowel of the root, or stem,—or of the tense-particle prefixed,—by changing *

| | |
|---|---|
| *a* long to *aiá*, | *e* long to *aié*, |
| *o* long to *uá* (*wa*), | *o* short to *ué* (*we*), |
| *i* long to *á*, | *ă* or *ĭ*, short, to *é*. |

The author of *Etudes Philologiques* (p. 49) terms the modes in which this change takes place, the "eventual," the "simultaneous," the participial, and the gerundive. Baraga says that it takes place (1) in all *participles;* (2) in sentences expressing *periodical* actions, events, or states of being, or (3)

* For the Chippeway and Nipissing-Algonkin (Baraga, Otch. Gr., 129; Cuoq, Etudes, 49). I substitute *u* for Baraga's *w* and Cuoq's 8 (Eliot's *ω*). Lacombe, for the Cree, has a somewhat different notation (Gram. Crise, 155): *a* long, to *iya* and *eya; e* long, to *iye; o*, after a consonant, to *iyo; o*, beginning a word, to *we; i* long to *iye; a* short to *e* long; *i* short, to *e*.

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actions *just past;* (4) after certain interrogations, meaning, how? what? when? where? etc.; (5) in sentences expressing *comparison;* and (6) in sentences that express *quality* after adverbs meaning 'all,' 'all that,' 'whatever,' 'whosoever'; (7) in some tenses of the subjunctive mood in the *dubitative* conjugation; (8) after *mi* 'so'; and (9) in sentences which in English would contain a *relative pronoun* before the verb, 'he who' or 'that which,' etc. For our present purpose, Mr. Howse's more comprehensive statement of the effect of this vowel-change is sufficiently accurate: it "signifies that the *action* combined with the attribute is *generalized*, or rendered *indefinite* in respect of *time*, and hence implying sometimes *custom* or *habit* in the *subject;* whence it also often becomes the Indian *equivalent of English nouns ending in* er *and implying an actor.*"

Edwards, in his "Observations on the Language of the Muhhekaneew Indians," and Zeisberger, in his Delaware Grammar, name this form of the verb, a *participle.* "Though the Mohegans," says Edwards, "have no proper adjectives, they have participles to all their verbs: as *paumseet*, the man who walks [from *pumissoo*, he walketh]; *waunseet*, the man who is beautiful [from *w'nissoo*, he is beautiful]; *oieet*, the man who lives or dwells in a place [= *āyit*, Eliot, from *ayeu*];" etc. "They have no *relative* corresponding to our *who*, or *which.* Instead of 'the man who walks,' they say, 'the walking man' or 'the walker.'" A more accurate translation is, 'he *when* walking,' i. e. *conceived as* walking, or 'as a walker.'

The "vowel change" is of very frequent occurrence in Algonkin speech. "It occupies us in all our conjugations," says Baraga, who has devoted more than twenty pages to its illustration. Lacombe observes that it presents one of the greatest difficulties of Cree Grammar, but that he who has not learned when and how to use it is a bad Cree scholar. It characterizes every participle, is employed in almost every conditional clause, and commonly in interrogation; it supplies the place of relative pronouns, and serves to form all general names of *actors* or *doers.* No view of the structure



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of Algonkin language which leaves out of sight the *changed vowel* can be even approximately complete: and yet this feature seems to have escaped the observation of Humboldt, Steinthal, Fr. Müller, and many other less eminent writers on American languages.

The grammatical persons of the subjunctive or "suppositive" mood are distinguished by *terminations* — and *not* by pronominal prefixes. In the Massachusetts dialect, the first and second persons plural are distinguished only by the accent or quantity of the vowel of the last syllable: e. g. "*naüm-og* (*o* as in *log*), if we see; *naüm-óg* (*o* as in *vogue*), if ye see." (Eliot's Gr., 3.) In the Chippeway, the first and second persons singular are similarly distinguished: "the termination of the first person, *iàn* or *àn*, is pronounced long; whereas that of the second person, *ian* or *an*, is very short." (Baraga, 118.) The 3d person sing. (animate) ends, in all dialects, in *t* or *d*. For example: Chip. *ăbi* (Mass. *ăpu*) 'he sits,' makes in the suppositive, with vowel-change,

sing. 1. *ĕpi-ïàn*, if (or, when) I sit, I who sit, I sitting.
2. *ĕpi-ïăn*, if (or, when) thou &c.
3. *ĕpid*, if (or, when) he &c., and, *as a noun*, a *sitter*.
pl. 1. *ĕpi-ïàng*, if we &c. (*exclusive*).
*ĕpi-ïăng*, if we &c. (*inclusive*).
2. *ĕpi-ïeg*, if you &c. Mass. *ĕpiòg*.
3. *ĕpidjig*.*

From Chippeway *mawi* 'he weeps' is formed, in the 3d person, suppositive, *mēwid* 'he who (habitually, or *sometimes*) weeps,' 'a weeper'; from *niba* 'he sleeps,' *nēbad* 'one who sleeps'; from *ākosi* 'he is sick,' or 'in pain,' *aiakosid* 'a sick person'; from *wābi* 'he sees,' *waiabid* 'one who habitually sees'; *nishiwé* 'he kills, murders,' *neshiwed* 'a murderer'; &c.

*The Copula.* Algonkin languages possessing no independent verb-substantive, bare existence cannot be affirmed as a general attribute alike of *persons* and *things*. The formal

* The Chippeway, according to Baraga and Cuoq, has, besides the "eventual" or "participle" forms, a simple subjunctive—'if I sit' &c.—*without the vowel change*. The personal terminations of the two modes are the same except in the 3d person plural, in which Baraga gives *-djig* for the "participle," and *-wad* for the subjunctive proper.

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expression of predicate relation is modified by the fundamental distinction between *animate* and *inanimate* being. The functions of the verb 'to be' are performed (1) by pronominal elements, in combination with predicative roots, (2) by tense particles, expressing past, present, and future relations in *time*, and (3) by primary verbs which — independently, or in composition with other verbal roots — affirm *modes* of being, animate and inanimate, active and inactive.

(1.) "We have no complete distinct word for the verb-substantive," says Eliot (Indian Grammar Begun, p. 15), "as other learned languages and our English tongue have, but it is *under a regular composition*, whereby many words are made Verb Substantive. . . . The first sort of verbs substantive is made by adding any of these terminations to the word: *yeuœ*, *aœ*, *oœ* [= *iu-u*, *e-u*, *o-u*], with due euphony: and this is so, be the word a noun, or adnoun, or an adverb or the like"; e. g.

| | |
|---|---|
| *woskelomp* 'a man' [vir], | *wosketomp-o œ* 'he is a man.' |
| *wompi* 'white,' | *wompi-yeuœ* 'it is white.' |
| *nux* 'yea,' | *nux-yeuœ-utch* 'let it be yea' (James v, 12). |

"The second sort of verb substantives is when the *animate adnoun* is made the third person of the verb, and so formed as a verb: as *wompesu* 'white,' *nœ-wompes* 'I am white,' *kœ-wompes* 'thou art white,' *wompesu* 'he is white.' *

Baraga (Otch. Grammar, 87, 410,) gives nearly the same rules for forming "substantive verbs" in the Chippeway. If the substantive ends in a vowel, add *w* [= *œ* of Eliot] — if it ends in a consonant or a nasal, add *iw* or *ow* — to form the *first person* sing. of the indicative present: e. g.

| | |
|---|---|
| *inini* 'man,' | *nind' ininiw* 'I am a man.' |
| *ogima* 'chief,' | *nind' ogimaw* 'I am a chief.' |
| *assin* 'a stone,' | *nind' assiniw* 'I am a stone.' |

The third person adds *i:* e. g. *ininiwi* 'he is a man,' *ogimawi*, *assinawi*, etc.

In the Cree, "by adding *wiw*, for the third person singular, you have the auxiliary 'to be'" (Lacombe, Cr. Gr. 88): e. g.

* What Eliot denominates "the animate adnoun" is really an animate intransitive verb, which corresponds to our predicate adjective with the copula. See page 164, post.

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*mistik* 'wood,' *mistiko*WIW 'he is wood'; inanimate, *mistiko*WAN '*it* is wood.' Howse gives (p. 17) *nápáyoo* 'a man' (vir), *napawoo* 'he is a man'; *hóokemow* 'a chief,' *hóokemowoo* 'he is a chief'; *níppee* 'water,' *nippee-wun* 'it is watery, i. e. possesses the nature of water'; etc.

Generally,— an Algonkin noun or adjective becomes predicative by affixing *u* (*ω*, *w*,) with or without a connecting vowel. If the subject is inanimate, *n* preceded by a short vowel is added to the affix.

(2.) Eliot found in the Massachusetts language certain tense particles which supply the place of auxiliary verbs and particularly that of the verb 'to be' in expressing relations in time. For time present, he uses the *personal pronouns* in their independent — that is, in their *predicative* — form, for the 1st and 2d persons (*nēn*, *kēn*); and the demonstrative *noh* (inan. *ne*), for the third person:* e. g.

*nen uosketomp*, "I *am* a man," Matt. 8. 9.
*nen, nen n'noh* "I, even I, *am* he," Deut. 32. 39.
*nen, nen Jehovah* "I, even I, *am* the LORD."
*ken noh wosketomp* "thou art *that* man," 2 Sam. 12. 7.
*sun* KEN NOH *woh paont*, "*art thou he that* should come? Matt. 11. 3.

For the other tenses, we have, in the same dialect,—

*koh, kô*, connecting *past* with *present* time; expressing *continued* being or activity, 'was *and is*.'
*nô*, preterit absolute, and emphatic; 'was *and is not*.'
*woh*, "expressing the notion of a *possibility to be*" (El. Gram. 20); corresponding to the auxiliaries 'may,' 'can,' 'must,' etc.
*toh*, "properly signifieth *utinam* 'oh, that it were,' and is to be annexed to every person and variation of the optative mood" (El. Gram. 34).
*pish* 'he or it will'; simple future.

Examples: *noh* KOH MO, *noh* KOH, *kah noh paont*, "who *was*, and [who] *is* and [who] is to come," Rev. 4. 8: *nen* NUKOH [= *noh koh*], "I am he," Mark 13. 6: *ken nu*KOH, *kah ken* NUKOH MÔ, thou "who *art*, and *wast*," Rev. 11. 17.

Eliot occasionally (as in this last example) combines *koh* with *mo*—the former expressing continued *being*, the latter past *time*. So (in Gen. 1. 9, 11), *kah mónkó* [= *mô koh*]

* He makes only incidental references to these particles in his Grammar, but frequently employs them in his version of the Bible and other translations.

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*n'nih* "and it was [and *continues to be*] so," literally, 'it (was) that (which) is so.'

Resolving *noh* to *n* + *ω*, *koh* to *k* + *ω*, and *mô* to *m* + *ω*, we have, as expressions of time — or affirmations of *being* in relation to time — present and past, three pronominal elements, *n*, *k*, *m*, associated with the general demonstrative and *predicative ω* (or *u*).

In the same way, the Yaruras — Indians of New Granada, living near the Meta and Casanare rivers (tributaries of the lower Orenoco) — "construct their whole conjugation," says W. von Humboldt* (on the authority of the Jesuit father Forneri), "in the most simple manner, by combining the pronoun with tense particles. . . . These combinations constitute the verb 'to be,' and, annexed to a word, supply its conjugation-forms. The verb 'to be' has no articulate root peculiar to itself and which belongs neither to the pronoun or the tense-particles; and since the *present* tense has no particle of its own, its persons are expressed merely by the persons of the *pronoun* itself, which are distinguished only by abbreviations of the independent pronouns. The three persons of the verb 'to be,' in the singular, are *que*, *mé*, *di*, literally 'I,' 'thou,' 'he,' merely. For the imperfect, the syllable *ri* is prefixed: *ri-que* 'I was'; and, in combination with a noun, *ui ri-di* 'water was' (preterit absolute); as a true verb, *ʃura-ri-di* 'he ate.' Thus *que* signifies 'I am,' and this form of the pronoun *distinctively expresses the function of the Verb.*"

The place of the verb 'to be' is similarly supplied in other American languages — by an abbreviated personal pronoun, with affixes for past and future time. Humboldt† notices the Huasteca and, particularly, the Maya, which "possesses a pronoun that, of itself, constitutes the verb 'to be,' and which manifests very remarkable carefulness to express the true function of the verb, by a distinct element appropriated to that purpose." In the Tupi of Brazil, *poéra* and *ráma* — expressing past and future time — combine with personal pronouns to form the auxiliary verb:

* *Die Kawi-Sprache*, i. (Einleitung) s. cclxxxi.

† *Ibid.*, cclxxxiii, ff.

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| | |
|---|---|
| *xe-poêr*, I was (have been), | *xe-râm*, I will (or must) be, |
| *nde-poêr*, thou wast, | *nde-râm*, thou wilt be, |
| *y-poêr*, he was: | *y râm*, he will be.* |

Such constructions are not peculiar to languages of the new world or to such as Professor Steinthal regards as "formless." "In the Demotic Egyptian, the copula is usually expressed — as it is always in the Coptic — by the demonstrative *pu* (femin. *tu*, plur. *nau;* Copt. *pe*, *te*, *ne*), when it is not left absolutely unexpressed."† In Coptic, additional expression is given to the verb in the first and second persons, by repetition of the pronoun, in a modified form: e. g. *anok u uro* 'I (am) a king,' or *anok ang u uro* 'I, I, a king,' i. e. I am a king, or *ang u uro anok* 'I a king I:' with which compare Mass. *nen nen n'noh* 'I, I (am) he,' etc., page 160, ante. In all Semitic dialects personal pronouns are employed, as in Coptic, to supply the copula.

The office of the Algonkin tense-particles is less distinctly marked in modern dialects than in the Massachusetts. In the Chippeway group, Mass. *koh* is represented by *ki*, a sign of the past tense; *ka* (or *ga*), the sign of the indicative future; and (Nip. Alg.) *ke*, for the future of the subjunctive: Mass. *toh* becomes *ta* (or *da*), "denoting condition"; Mass. *woh* is Chip. *wi*, "denoting intention, will," etc.; and these particles are treated as portions of the verbs to which they are prefixed — not as independent words — and receive the vowel-change, in the suppositive mood:‡

(3) There are several primary verbs which — independently or in composition with other verbal roots — affirm modes and conditions of being, animate and inanimate, active and inactive. An *animate* subject 'moves,' or 'sits' or 'abides,' or 'possesses,' or 'is in this or that place'; an *inanimate* subject 'is at rest,' or 'is moved,' or 'is placed,' or 'grows,' or belongs to,' or 'is such as.' By prefixing a verbal or a demonstrative root to one of these primary verbs, an "adjective verb" is formed, corresponding to our predicate

* Anchieta, *Gramm. d. Brasil, Sprache*, hrsg. Platzmann, 87.

† Steinthal, *Charakteristik*, 240.

‡ Baraga, *Otch. Grammar*, 134; Cuoq, *Etudes philologiques*, 54, 55.

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adjective with the copula (ante, p. 159). Two or three verbs, much employed in composition, have, in some dialects, lost their independence and are reduced to mere formatives of adjective verbs.

The verbs most frequently used as substitutes for the verb 'to be' are (in the Massachusetts dialect) the following:

*Ap'u* (*appu*, El.) 'he stays, abides, remains, *sits*': *toh kut-apin?* "where *art* thou" (staying), Gen. 3. 9; *na kut-apin* "there thou art," Ps. 139. 8; *noh apit* [*ēpit*] "he that sitteth," Ps. 2. 4, "he that abideth" (ὁ μένων), 2 John, v. 9; *na apsh* "be there," Exod. 24. 12.

*Ai-iu* (*ayeu*, El.) 'he is *there*, is in this or that place' (Fr. il y est); hence, 'he *dwells*': *ayeu kah appu* "he dwelleth and abideth," Job 39. 28: *nut'ai* "I dwell," Ezek. 43. 9: *matta ayeu-ω-og* (negative form) "they *were* not" (there), Jer. 31. 15. This verb is so commonly used in place of the verb 'to be' that it has been regarded as a genuine verb-substantive, by several writers on Algonkin grammar. Mr. Howse (Cree Gr. 136) calls it (Cree *iów*, in his spelling; *ayaw*, Lacombe,) "the verb-substantive in its absolute form, 'he, or it, is *being*, or *existent*,' Anglice 'he, or it, *is*.'" Baraga translates it (Chip. *nind aiá*, 3d pers. *aia*) by "I am, I exist," and Schoolcraft gave its paradigm* (as "*i-e-au*, to be") in proof that the Chippeway language possesses a true substantive verb. That it affirms *being in place*, and not *being, absolutely*, is shown by its derivatives, in all Algonkin dialects. Its root is a demonstrative (*yeu*, El.; Chip. *aw*), and its verbal noun, Mass. *ayeu-onk* 'placing,' 'being here, or there,' was used, objectively, for 'a place' or 'a dwelling place.' The participial (3d pers. suppositive), with changed vowel, Mass. *ayit*, El. (=*ēiit*), Chip. *eiad* (*i-ád*, Schoolcraft), Moh. *oieet*, means (as Edwards translates it) "the man who lives or dwells in a place," hence, generally, 'dweller' or 'inhabitant' of a place or country.

*Unni* (and *n'nih*, El.) 'it is so,' the predicative form of the general demonstrative, *ne* 'this,' becomes, in the personal conjugation, *wut-inni-in* 'he is *such as*,' or 'of *the kind of*,'

* Indian Tribes &c., ii. 436–441.

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and expresses the relation of an individual to a species or a class. In this sense, Eliot used it in such expressions as *neâne unnantog ut wuttahhut*, *ne wuttiniin* "as he thinketh in his-heart *so is he*," i. e. of that kind is he,* and in the much-discussed translation of "I am that I am," Exod. 3. 14: *Nen nuttinniin nen nuttinniin*, 'I myself am of the kind I myself am of the kind of,' i. e. I am like myself and no other, I am such as I am such as,— Ego sum talis qualis ego sum.

*Us-u* (*ussu*, *usseu*, El.) 'he moves, acts,' Lat. *agit*, expresses *animate activity*: Abn. *ned-a$^{n}$sœ* "je fais, j'agis" (Rasles); Cree √ *osi*, *oshi*, "faire, créer" (Lacombe); *áche-oo*, *aje-oo* "he moves, has the faculty of moving" (Howse). This verb or its predicative root is the base of a great many animate intransitive and adjective verbs. In composition, the short vowel of the root becomes, in the Massachusetts dialect, *i* (in Eliot's notation, *e*), and in the Cree and Chippeway it is modified by or coalesces with a preceding vowel, giving the terminations (in 3d person sing.) *-isiu*, *-īsiu*, *-issu*, *-êsu*, etc. The consonantal element, *s*, is constant, and may be regarded as the characteristic of verbs which express manner of *animate* being. It affirms *life*, or existence as life. As examples of adjective verbs animate ("animate adnouns"), Eliot gives, in the Massachusetts,

| | |
|---|---|
| *menukk-i* (it is) strong, | *menuhk*ESU, he is strong. |
| *womp-i*, " white, | *womp*ESU, he is white. |
| *wunn-i*, " good, well, | *wunn*ESU, he is good: |

and in 1st person, *num menuhk-es*, I am strong, *noo-womp-es*, I am white, etc.

In Western Cree (Lacombe): rad. *kin* 'long,' adj. v. *kinosiw* 'he is long, tall'; *miyo* 'pleasing, good,' *miyosiw* 'he is good'; *ak* 'painful, unpleasant,' *akosiw* 'he is in pain, sick'; *maskaw* 'it is strong,' *maskawsiw* 'he is strong': to which correspond in (Howse's) Eastern Cree, adj.-verbs *kinwoosu*, *methosu*, *awkoosu*, *múskówissu*. In (Baraga's) Chippeway, *gin-osi* 'he is tall,' 1st p. *nin gin-ós* 'I am tall'; *ákosi* 'he is sick,' 1st p. *nind akos; mashkawisi* 'he is strong,' 1st p. *nin mashkawis*; *nókisi* 'he is weak, soft'; 1st p. *nin nokis*.

* See *Trans. Am. Philol. Association*, 1869-70, p. 115.

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Mass. *Ohtau* 'he possesses, *has*,' and intrans, *ohtaeu*, *ohteau* (El.), *ohtu* (Cotton), 'it *has itself*, it *is*,'—is used often by Eliot, to supply the place of the verb 'to be' when the subject is *inanimate;* e. g. *moskeht . . . uttiyeu yeu kesukok* OHTEAU "the grass which to-day IS," Matt. 6. 30; *pish ohteau keyausut* "it shall *be* in your flesh," Gen. 17. 13: *nuppœonk ohteau ohkuhqut* "*there is* death in the pot," 2 K. 4. 40; etc. Baraga treats the corresponding verb, in Chippeway, *até* "there is of it, it is," as "unipersonal" (3d pers. sing. and pl.), and Lacombe gives it, in Cree, an *animate* form, "*ittaw*, il existe, il est," while Howse regards it as "the relative form of the verb-substantive *i-ów* [*ai-iu*, p. 163, ante], having for its attribute the relative prefix of place, *it*"; giving, with anim. subject, "*it ów* 'he is there' (Fr. *il* y *est*), and inan. *it-akwun* 'it is there.'"

*The Noun, a Verbal.* Putting aside all speculation concerning the priority of noun and verb and the relative prominence of the conceptions of objective and predicative relation, in primitive Algonkin speech, and looking only to the structure of the language at its present stage of growth, I repeat, that Algonkin nouns, so far as their derivations can be traced, are formed *from verbs*, or *as verbals* on predicative roots. Every name manifests consciousness of subjective relation—affirming that the person or thing *is*, or *moves*, or *acts*, or is *acted upon by* another animate or inanimate agent. This is seen to be true even in names which must have been of very early origin and which certainly were in use before the separation of existing dialects. Take the following examples:

1. RIVER. Mass. *sīp-u*, *sīp*, Abn. *sipu*, Chip. *sibi*, *sipi*, etc. From the root SĪP 'stretching,' 'extending,' hence 'to be long.' In the Massachusetts dialect, *sīpi* (*sepe*, Eliot) is used adverbially for 'long' or 'extended' in *time* or *place*: e. g. *sepe-pomantam* 'he long lives,' 'is long-lived,' "stricken in years," Gen. 24. 1; *sepe kodtantum-up* 'he had long desired' &c. Luke 23. 8; *sepohtae* "long continuing," Jer. 30. 23. Lacombe (Diction. Crise) gives as the meaning of the root, "that which draws itself out, extends, enlarges itself; which



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lasts a long time," etc., and for the verb *sipiw* "it extends, draws out," but he has not observed that *sipiy* (*seepee*, Howse) 'river,' is from the same root and a form of the same verb. In Chippeway, a difference of pronunciation distinguishes—and partly conceals the radical identity of—*sibi* 'river' and *jiba* "durable, lasting, extending." The former is in Chippeway, as *sīpu* is in Massachusetts, a predicative form: 'it stretches, extends, *str*eams.' Compare Chip. *jibi* "he stretches himself"; *jib-issin* "he lies stretched out" (Mass. *sīpsin*); *jib-an* "it is tough" i. e. stretches. From the same root are formed Mass. *sīpagham* 'he sails,' i. e. goes by stretching a sail; *sīpagke-og* 'they are spread, spread themselves'; and *sipaghunk* 'a sail,' lit. 'that which *is* extended.'

2. Father. Mass. *n'œsh* 'my father,' Narrag. *n'ôsh*, Chip. *n'oss*, Cree *n'œtawi:* literally, 'I come from him' or 'he *from*s me' (giving the preposition the office of a verb transitive): in the 3d person (obviative), Mass. *œsh-oh*, Chip. *oss-an*, Cree *œtáwi-a* (*ottâwiya*, Lac.), 'his father'=̇'he comes from him,' 'he *from*s him.' The root ū (Eliot's *œ*) means 'going out' or 'proceeding from.' It takes verb form in

Mass. *n'oom* 'I come (proceed) from,' 3d p. *oom*, *woom* (El.)
Del. *n'ūm*, " " " " *wūm* (Zeisb.)
Abn. *n'oom-en* 'I came thence.'

With the vowel-change, in the subjunctive, Mass. *wag*, Abn. *wek*, 'he when coming (or, who comes) from' a place other than that of the speaker.

When the idea of motion or of animate energy is associated, *œ* becomes *œch*, *œsh*, or *œtche*, and these, in some dialects, by nasalization of the vowel, have passed into (Del.) *ūntch*, or (Chip.) *ūndj*. Thus, with inan. subject,

Mass. *waban ootsh-oh* "the wind bloweth" (comes from) &c. John 3. 8.
(subj.) *toh wâdsh-ont* "whence it [may] blow."
Del. *ta ūndch-en?* "whence blows [the wind]?"

With an animate subject,—

Mass. *n'ooch-ai* 'I come from, am *of*' (a place): 3d p. *wutchau*.
*n'oochai wohkumaieu* "I am (come) from above," John 8. 23: *n'ooch-i Jehovah* "I am the Lord's" (from or of him), Is. 44, 5: suppos. *wudchiit*, *wachiit*, 'he, proceeding from,' 'he who is from.'

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Chip. *nind ôndji* 'I come from': suppos. and particp. *wendjid.*
E. Cree, 3d p. *ooch-ïü* (*ooche-oo*, Howse,) "from-eth he," "he proceeds from."

With the transitive form,—

Mass. *ooche-un* 'he produces from it': e. g. *ne muhpeteóg. . . . ooche-un mittamwossissoh* "that rib. . . . made he [from it] a woman." Gen. 2. 22: with inan. obj. direct and remote, *ooch teau-un* 'he made of it,' Ex. 38. 8.
W. Cree, *osi-hew, oji-hew*, 'he makes (produces) *him.*'
*osi-ttaw, oji-haw*, 'he makes (produces) *it.*'
Chip. *nind oji-a* "I make" some anim. object: 3d p. *od-oji-an.*
*nind oji-ton* "I make" it. " *od-ojiton.*
*nind ojig* "I am made" (produced), " *ojig-i.*

Impersonally — serving as preposition and as adverb — '*it is* from, of, out of, because of,' and 'for that cause,' 'thence,' '*that from*,' etc.,—

| | | | |
|---|---|---|---|
| Mass. | *ooch-i*, | with vowel change, | *wŭtchi*, |
| Abnaki, | *oots-i*, | | *wetsi*, |
| Cree, | *ootchi*, | | *wetchi*, |
| Chip. | *ondji*, | | *wendji*, |
| Del. | *ŭntchi*, | | *wentchi.* |

From this verbal-particle—which receives conjugation forms — comes the name for 'father,' Mass. *n'œsh*, Del. *n'œch*, &c., meaning 'I am *from* him' or 'he produces me'; 3d p. *œsh-oh* 'his father' = 'he is from him.' (Compare Mass. *waban œtshoh* 'the wind comes from.')

3. Manit, i. e. the *Preternatural;* often translated by 'God,' 'Spirit,' and the like, is regularly formed as the suppositive (3d pers. sing.) and participial of a primary verb meaning 'to surpass, exceed, be *more than.*' The verbal root, an 'surpassing, going beyond,' is the base of Mass. *anuï* (*anue*, El.) 'it surpasses, is more than'; Del. *aluï* (*eluwi* "most," Zeisb.), West Cree *a'iu* (√*ayiw*, more, surpassing, etc., Lac.), Chip. *ani-* ("in composition, marks 'going on'"; Baraga). Eliot employs *anuï* to form the comparative degree, as in Matt. 18. 8, 9, *anuï wunnegen* "it is better." It takes personal forms in Mass. *an-in* 'it is *rotten*,' 'decayed,' i. e. goes beyond, is *more than* ripe, mature, or fit for use; and, with anim. subj. *aninnu* 'he is corrupt, rotten' (John 11. 39, Ps. 14. 1); inan. ptcpl. *ne aneük* 'that which is corrupt' (Mal. 1. 14, Prov. 12. 4); anim. *noh anit* 'he who exceeds, surpasses, is more than' — the natural, common, or normal:

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with the indefinite and impersonal pronominal prefix — *m'anit* 'somebody or something that exceeds, is *preternatural.* With the predicative affix, *manit-u* (*manitto*, *manittw*, El.) 'he, or, it *is manit': nen manitto* "I am God." Is. 43. 12. "They cry out *manittóo*, that is, 'it is a God,'"— says Roger Williams — "at the apprehension of any excellency in men, women, birds," &c.; and so "they say of *every thing which they cannot comprehend.*" In composition, the prefixed *m* is not retained: e. g. Del. *Get-annito* (Zeisb.) = Mass. *keht-annit*, 'the greatest *manit*' (for "the LORD God," Gen. 24. 7); Narr. *Kesukquánd* (=*kesukqui-anit*) "the Sun god," *Wompanánd* (*wompan-anit*) "the Eastern god," &c. (R. Williams).

The impersonal prefix, *m*, is similarly employed — for forming an appellative from a verb or verbal root — in Mass. *may* 'a path,' literally, 'a going-to' (*aditus*), from *a u* 'he goes to,' suppos. *e-i.; minni*, contr. *min*, 'a berry,' or other small fruit, literally 'a growth' or 'what is grown'; *maskeht* 'grass,' form *askeht-u* 'it is green,' i. e. not yet mature or full grown; etc.

Names of actions and of instruments are formed from the third person singular of the indicative present. If the verb is transitive, the 'nomen actionis'— corresponding nearly to our participial infinitive — has both *active* and *passive* forms: Ex., Chip. *dibaamáge* 'he pays'; *dibaamage-win* 'paying,' payment, *given* to another; *dibaamág-owin* 'being paid,' payment *received: ságiiwe* 'he loves'; *ságiiwewin* 'a loving' (love *given*); *ságiigosiwin* 'a being-loved' (love received); and from other conjugation-forms of the same verb come *ságaadisiwin* 'self loving' and *ságiidiwin* 'mutual loving'; *pakitéige* 'he strikes,' *pakitéigewin* 'striking,' a beating given, *pakitéigowin* 'being struck,' a beating received. More than twenty other forms of verbal nouns are noticed by Baraga (*Otch. Gram.* 29–32) and Lacombe (*Gram. Crise*, 19–25). In each of these forms, we find a distinct recognition of the relation of the activity to the *subject* — animate or inanimate. For example, *pakitéigan* 'a hammer' (striking instrument), from *pakitéige* 'he strikes,' is formally distinguished as the instrument with which an

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*animate* subject strikes an *inanimate* object: *tchigatáigan* 'a broom,' from *tchigatáige* 'he sweeps,' is 'that with which *he* (some *person*) sweeps *it*,' etc.

Professor Fr. Müller* selects, "as an example of the Algonkin *nomen actionis*, [Nipissing-] Algonkin *pimosé*, Cree *pemootan*, to walk (gehen)." As a verb, *pimosé* 'he walks' has, in the indicative present, the forms—

| | Nipis. Alg. | Chip. | Mohegan. |
|---|---|---|---|
| Sing. 1 | *ni-pimosé* | *nin-pimossé* | *n'pumseh* |
| 2 | *ki-pimosé* | *ki-pimossé* | *k'pumseh* |
| 3 | *pimosé* | *pimossé* | *pumissoo.* |

Professor Müller maintains that these are forms of a noun with possessive affixes—not true *verb*-forms,—and that they mean, respectively, not 'I walk, thou walkest, he walks,' but 'my walking, thy walking, (his) walking.' We find, however, that all Algonkin dialects have true "nouns of action" or participial infinitives formed from the indicative present of their verbs, and have also nouns denoting the (habitual or conceived) actor or agent, formed from the suppositive mode of the verb, with a change in the vowel of the root. Thus, from Chip. *pimossé* 'he walks,' are regularly formed *pimossewin* 'walking, a walk' and *pēmossed* (Moh. *paumseet*, Edw.) 'one who walks, a walker.' If we examine more closely the form *pimossé* (Moh. *pumissoo*, Abn. *pemussé*, Mass. *pomushau*) we find it to be that of an animate active-intransitive verb, composed of the primary verb *ŭssu*, predicating *animate energy* (see page 164, ante) and a verbal root (Chip. and Cree PIM, Mass. PŎM, PŬM,) meaning 'passing' 'going by,' found as a prefix to a great many derivative verbs, in all dialects. The literal meaning of *pimossé* is 'he passes *actively*' or by the exertion of vital energy.

Cree *pemootayoo* (*pimuttew*, Lacombe) has a different formation, *-ŭttew*, in composition, representing the verb *ituttew* 'he goes' (on foot, or by walking): e. g. *nestuttew* 'he is tired of walking,' *kākāyāh*uttew 'he walks strongly, or briskly,' *as*ettew 'he walks behind,' *newokat*ettew 'he walks on all fours' (lit. 'on four legs'), etc. To Cree *-uttew*

* *Grammat. Bau der Algonkin-Sprachen*, 146.

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corresponds Chip. *-ossé* (= *ussu* 'agit') as the base of verbs of walking: e. g. *mito*ssé 'he walks (indef.), goes on foot,' *babám*ossé 'he walks about,' or, from one place to another, *aje*ossé 'he walks backwards,' *min*ossé 'he walks well,' etc. The Algonkin name for the *foot*, Mass. *-ŭssit*, Chip. *-sid* (*o-sid* 'his foot'), Cree *-sit*, — never found without a pronominal prefix — comes from *ussu*, or from the verbal root ŭs 'agere.' The foot is 'the doer' or 'the worker.'

In *pimosé* we have (1) the verbal root *us* 'agere,' (2) the *subjective* pronominal element *e* (Mass. *u*) expressing the predicative function, and (3) the associated root, *pim*, designating the mode of activity — in other words, qualifying the primary verb of animate activity.

To discuss the nature and origin of the so-called "transitions" — by means of which not only the *subject* but the *object*, animate or inanimate, direct and remote, may be incorporated with the verb — would extend this paper beyond reasonable limits. I have purposely omitted reference to these more complicated conjugation-forms, and have considered only such as may be assumed to be of earlier origin — older at least than the stage at which the tendency to extreme polysynthesis attained its largest expression. In the analysis of these, I can discover nothing that suggests doubt of their *genuineness*. The facts of language are seemingly opposed to the conclusion at which Professors Steinthal and Fr. Müller have arrived *a priori*. *Seemingly* opposed, I say, because I am not unmindful of Professor Steinthal's warning — that "some languages know how to supply the want of true form by devices so artful as *completely to attain the appearance* of real grammatical forms." (*Charakteristik*, 326.) The evidence may be summed up, very briefly:—

1. Algonkin verbal roots correspond, in general character, to Indo-European. As examples, take the following, found, with more or less phonetic modification, in all Algonkin dialects: *ap* 'sitting,' 'staying,' *pi* 'approaching,' *au* 'going to,' *ω* 'coming from,' *nad* 'bringing,' 'bearing,' *sīp* 'stretching,' *sok*, *sag* 'out-going,' *an* 'going beyond, exceeding,' *wab* 'seeing,' *pāk* 'opening,' *bok* 'breaking,' *pim* 'passing by,' 'crossing'; etc.

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2. The predicative function is distinctively expressed, by the combination of a pronominal or demonstrative element with a verbal root.

3. The pronouns prefixed to the first and second persons singular of the indicative, are *subjective*, not possessive. In the subjunctive and imperative the distinction of grammatical persons is marked by *inflection* — not by pronominal affixes.

4. The distinction between noun and verb is formally established; though, as the noun is always derived from a verb or formed on a verbal root, it retains nearer likeness to the verb than in Indo-European languages. The distinction may be occasionally obscured by the subordination of the *noun* — in common with all other parts of speech — to the verb, but not by the prominence of the conception of objective relation, or by the want of formal expression of the predicative function of the verb.

5. The "nomen actionis" (corresponding to the participial infinitive) and the "nomen agentis," are formed from the verbal root — the former being distinguished from conjugation-forms by its termination and the latter being characterized by a *change of the vowel of the root.*

6. Modal and temporal distinctions are marked in all conjugations: the former by changes of termination and by internal modification of the root; the latter by tense-particles prefixed to the verb (see page 160, ante,) and, for one tense — corresponding nearly to our continuous preterit — by a special termination (*-p* or *-b*, with euphonic connective; Ottawa, *-ba*; Chip. and Cree, *-ban*;) which seems to have come from root *ap* 'sitting, staying:' e. g. Mass. *n't ap* 'I sit,' *n't ap-ip* 'I sat, or used to sit.' This termination is given to *nouns* as well as to verbs: Chip. *n'oss* 'my father,' *n'ossiban* 'my father *who was*, my deceased father;' *nind akik* 'my kettle,' *nind akikoban* 'the kettle *which was* mine.'

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## IX.—*On a supposed Mutation between L and U.*

By S. S. HALDEMAN.

PROFESSOR OF COMPARATIVE PHILOLOGY IN THE UNIVERSITY OF PENNSYLVANIA.

It is often stated that in French an 'u' has been derived from a Latin *l*, of which supposed examples are cited in 'sâlsus' French *sauce;* 'fâlsus' *faux*, *fausse;* 'bâlsamum' *baume*, in which the Latin â merely closes to *o*, independentli of *l*, as shown in cases where the *l* and orthographic 'u' occur together, as in 'săl-ix' *saule;* 'sâltus' old Fr. *sault*, modern *saut.* Similarli, Latin *o* closes to French *ou*, as in 'molînus' *moulin;* 'mŏrĭri' *mourir;* 'mŏvēre' *mouvoir;* or Latin *u* remains as French *ou*, as in 'fulgur' *foudre.* The facts remain the same whether the 'au' is regarded as ô, or as representing a diphthongal sound in some of the dialects.

In some localities the 'e' of *beau* is pronounced, leaving *au* as a recession of *u* in 'bēllus,' influenced by the ablative form 'bello,' like the ō of English cō*u*lter from Lat. 'cūlter' (coulter, knife). We have a like case in Latin -um, ablative -o, in going from 'mantelum' to Fr. *mante-au;* and final -a of 'bētula' (birch) gives ō in French *b..ouleau*, Lat. 'et' being dropt and 'u' (ou) retained. So 'cūltēllus' gives French *cou..te..au* (= coo-to); 'aqva' *eau;* 'alter' *autre;* 'altus' *haut;* 'astur' *au..tour*, where Latin *s* is lost, *a* becomes *o* (or perhaps a diphthong in some dialects), and *u* (= oo) remains in its usual French orthographi.

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# American Philological Association,

1875-6.

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# AMERICAN PHILOLOGICAL ASSOCIATION.

NEW YORK, Tuesday, July 18, 1876.

The Eighth Annual Session was called to order at 3 o'clock P. M., in the Chancellor's Room of the University of New York, by the President, Professor Albert Harkness, of Brown University, Providence, R. I.

An address of welcome was made by Chancellor Howard Crosby, Chairman of the Local Committee, to which the President replied.

The Secretary presented a report from the Executive Committee, announcing that the persons whose names follow had been elected members of the Association:

Professor B. H. Engbers, Mt. St. Mary's Seminary, Cincinnati, O.; Mr. L. H. Buckingham, English High School, Boston, Mass.; Dr. Alexander Fleischmann, Cazenovia Seminary, Cazenovia, N. Y.; President D. C. Gilman, Johns Hopkins University, Baltimore, Md.; Professor B. L. Gildersleeve, Johns Hopkins University, Baltimore, Md.; Mr. E. G. Sihler, Johns Hopkins University, Baltimore, Md.; Professor Lawrence Rust, Kenyon College, Gambier, O.; Mr. A. Duncan Savage, Rhinecliff, N. Y.; Mr. Joseph Alden Shaw, Highland Military Academy, Worcester, Mass.; Dr. John G. Shea, New York City.

The Treasurer presented his report, showing the receipts and expenditures of the past year to be as follow:

RECEIPTS.

| | |
|---|---|
| Balance in treasury, July 13, 1875, | $832.10 |
| Fees of new members and annual assessments, | 340.00 |
| Interest, | 74.38 |
| Sales of publications, | 21.75 |
| | $1,268.23 |

EXPENDITURES.

| | |
|---|---|
| Printing Transactions, 1874, | $357.90 |
| Printing Proceedings, 1875, | 145.92 |
| Postage, expressage, stationery, etc., | 71.89 |
| | $575.71 |
| Balance in treasury, | 692.52 |
| | $1,268.23 |

An investment of $500 is not included in the balances of this report.

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On motion, Professor A. C. Kendrick and Professor Thomas D. Seymour were appointed Auditors of the Treasurer's report.

The Secretary read a paper by Professor W. W. Goodwin, of Harvard College, Cambridge, Mass., on "Verses 453–455 of the *Antigone*."

*οὐδὲ σθένειν τοσοῦτον φόμην τὰ σὰ*
*κηρύγμαθ' ὥστ' ἄγραπτα κἀσφαλῆ θεῶν*
*νόμιμα δύνασθαι θνητὸν ὄνθ' ὑπερδραμεῖν.*

In this paper I wish to suggest, as a question for scholars to consider, whether we cannot make these verses mean what we all want to have them mean—viz.: that Antigone herself cannot transgress the laws of the Gods on the authority of a human proclamation—by considering *θνητὸν ὄντα* (sc. *τινά*) as the subject of *ὑπερδραμεῖν*, and referring it to Antigone in the sense of "one who is a mortal" (like myself). In Eurip. Med. 1017, 1018, we have:

*οὗτοι μόνη σὺ σῶν ἀπεζύγης τέκνων.*
*κούφως φέρειν χρὴ θνητὸν ὄντα συμφοράς.*

Here *θνητὸν ὄντα* (sc. *τινά*) refers impersonally to Medea, in the sense "one who is a mortal," i. e. "like you," very much as I have supposed it to refer to Antigone in the passage in question.

This view is strongly supported, I think, by Aesch. Agam. 923, 924:

*ἐν ποικίλοις δὲ θνητὸν ὄντα κάλλεσιν*
*βαίνειν, ἐμοὶ μὲν οὐδαμῶς ἄνευ φόβου.*

Here *θνητὸν ὄντα* is clearly taken as I propose, and *ἐμοί* makes it absurd to supply *ἐμέ*. It is clearly "a mortal like myself." Unless it is thought that the gender of *θνητὸν ὄντα* here would have been changed if the speaker had been a woman, we can hardly object on the ground of gender to the interpretation proposed for the same words in the *Antigone*. And yet it seems to be entirely on this ground that all editors (I believe) since Erfurdt have referred the words to Creon, thus making Antigone abandon her own justification to charge Creon with impiety. But what act of impiety has Creon yet attempted to justify on the ground of his proclamation? Thus far, the proclamation itself is his latest offence. It is far different when Antigone declares that she could not consider Creon's proclamation sufficient authority to justify *her* in defying the laws of the gods.

Dr. George R. Entler, of Franklin, N. Y., read a paper on "The Origin of the Hebrew Article הַ."

The theory which derives the article from הַל, as הַשָּׁמֶשׁ for הַלְשָׁמֶשׁ, the final liquid ל being assimilated to the initial letter, which takes the *dagesh-forte*, has found advocates in Ewald, Gesenius, Uhleman, Stuart, and Bush; but it is more in keeping with the principles of correct philology, and with the teachings of the older grammarians and of Nordheimer and Green, that the article is a derivative of the personal pronoun הוּא, which is also derived from the verb of existence הָוָה = הָיָה. There are a few instances in which the ו is retained; as יָלַד for

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וָלֶד, יָשַׁב for וָשֵׁב. In Eccles. ii. 22 and Neh. vi. 6 the ו is restored in the participle הוֶה, as it is also in the imperative הֱוֵה in Gen. xxvii. 29, and הֱוִי Is. xvi. 4. In Eccl. xi. 3, in the future, the ו is restored and ה changed into א: יְהוּא, the root corresponding to the exact form assumed by the pronoun. The ו is retained in the cognate dialects: in the Chaldee הֲוָה for הֲיָה, Syriac *hevo*, Samaritan *hevo*. As the labial ו is the essential part of the verb of existence, the initial and final weak letters are necessary to its completion. In forming the second person singular masculine, the lingual ת alone is to be employed. In Greek we have *σύ*, Doric *τύ*, Latin *tu*, German *du*. The labial ו is an ingredient element in all the Semitic, and also in the principal Indo-European languages, which employ the labials *b*, *v*, *f*, to express the idea of existence; e. g. Sanskrit *b'hu*, Persian *budan*, Russian *bhut*, Latin *fui*, German *bin*, English *be*. Bopp has shown in his *Vergleichende Grammatik*, and also Professor Harkness in his article on "The Formation of the Tenses for Completed Action in the Latin Finite Verb," that the labials appear in the imperfect, future, and perfect of the verb *amo*: e. g. *amabam*, *amabo*, *amavi*. The labial *v* in *ama-vi* is a compound of *fui*, corresponding to the Sanskrit *āsa*. The ו of the root וה is changed into its corresponding י on account of its heterogenous vowel ֶ: as in יָלַד for וָלַה. Thus the personal pronoun הוּא is a derivative of the verb of existence הֲוָה, the ו taking the vowel *u*, and the א taking the place of ה. In Gen ii. 19, xx. 7, xxiv. 65, the personal pronoun is substituted for the verb: e. g. שְׁמוֹ, הוּא 'his name was.' It is claimed that neither the Arabic personal pronoun (*huwa*) nor the Hebrew ל is employed in the formation of the definite article. The Arabic article *al* is formed from the preposition *le* and the prosthetic *alif*; and the demonstrative and relative pronouns are derived from the personal pronoun *huwa*. In the Hebrew the article הַ is derived from the personal pronoun as has been shown, and the demonstrative pronoun is from the preposition לְ 'to,' 'towards,' and the prosthetic א; so that we have אֵל *these*, with ה affixed, thus אֵלֶּה. But in the formation of the Arabic demonstrative pronoun *háza*, is a compound of *huwa* and *za*; the *vav* of *huwa* is changed into *alif*, as it is accompanied by the vowel *fetha* (*a*), which is displaced as indicated by a perpendicular *fetha*: *háza* for *huwaza*. But the Arabic relative pronoun is formed from the article *al* and the demonstrative *háza*, the *ha* being displaced on account of the *le* taking *tashdid* in order to preserve the initial mixed syllable; and final *alif* is changed into *ya*, being preceded by the vowel *kesva* (*i*). We have then *allazy* = *alhazy*. Now as the Arabic article *al* is not an element of the demonstrative pronoun *háza*, which is made up of the personal pronoun *huwa* and *za*, it is evident that the Hebrew article was not originally הַל, as the ל is employed in the formation of the demonstrative plural.

Professor S. S. Haldeman, of the University of Pennsylvania, Philadelphia, read a paper on "A Supposed Mutation of *l* and *u*."

This paper attempted to show that there is no mutation of *l* to *u* in going from Latin *fâls-us* to French *faux*, *fausse*, from *bâlsamum* to *baume*, etc., where A (of *arm*) merely closes to O independently of *l*, as in *sāl-ix*, *saule* (where *l* remains), or as in *mōrĭri*, *mourir*; *mŏvēre*, *mouvoir*. The facts remain the same, whether the *au* is regarded as the vowel *ô*, or as representing a diphthong in some of the dialects. In old French the *au* and *l* might be concurrent, as in *salt-us*, *sault*, modern *saut*.

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Professor W. D. Whitney, of Yale College, New Haven, Conn., read a paper on "The System of the Sanskrit Verb."

The system of forms constituting the verb in Sanskrit is less generally and clearly understood than it should be. The fault lies in great measure with the Hindu grammarians, and with the influence exerted by them upon their European successors. The Hindus arrived at no clear apprehension of the distinction between mode and tense; and the sense of historical relation and of proportion is wanting to them here as almost everywhere else. By the aid of the kindred languages, and especially by the labors of men like Curtius and Delbrück, the aspect of the subject has been much changed; but the results of such labors are hardly to be found as yet in the grammars: the latest of these (M. Müller's) even retrogrades in some respects from Bopp's, and appears to rejoice in renewed and heightened subserviency to Hindu ideas and methods.

It is believed that a scheme of arrangement like the following, with a few added explanations, will contribute to a better comprehension of the Sanskrit verb as a system. The example selected is the root *bhû*, 'be,' and there is given the first person singular of each of the ten sets of personal forms recognized by the Hindus as making up the conjugation of the simple verb—and each in its active and middle form, as distinguished by the constant difference of personal endings—along with those participles which attach themselves directly to the tense-systems.

*Synopsis of Conjugation of the root* bhû, '*be.*'

| | PRES. SYST. | PERF. SYST. | AORIST SYSTEMS. | | FUTURE SYSTEMS. | |
|---|---|---|---|---|---|---|
| *Active.* | | | *Simple Aorist.* | *S-Aorist.* | *Old Future.* | *Periphr. Fut.* |
| Indicative, | bhávâmi | babhû'va | | | bhavishyâ'mi | bhavitâ'smi |
| Optative, | bháveyam | | bhûyâ'sam | | | |
| Imperative, | bhávâni | | | | | |
| Participle, | bhávant | babhûvâ'ṅs | | | bhavishyánt | |
| Aug.-Pret., | ábhavam | | ábhûvam | | ábhavishyam | |
| *Middle.* | | | | | | |
| Indicative, | bháve | babhûvé | | | bhavishyé | bhavitâ'he |
| Optative, | bháveya | | | bhavishîyá | | |
| Imperative, | bhávâi | | | | | |
| Participle, | bhávamâna | babhûvâná | | | bhavishyámâṇa | |
| Aug.-Pret., | ábhave | | | ábhavishi | ábhavishye | |

I. The PRESENT-SYSTEM corresponds closely to what is called by this name in the Greek verb, consisting of a present tense (having "primary" endings), along with its modes and participle and with an augment-preterit (with "secondary" endings), or so-called "imperfect." Of modes, there is an optative and an imperative; and the first persons of the latter are really a relic of the subjunctive, of which the whole inflection is found abundantly preserved in the earlier dialect of the Vedas. This system of forms is in only a small class of verbs made directly from the root; in most, it comes from a secondary "base," or extension of the root; and the best opinion now holds this base to be in general a derivative noun, so that the forms are by origin denominative: in the verb taken as example it is *bháva*. There are a considerable number (toward a dozen) of ways in which the base of the present-system is formed from the root; all of these have their analogues in the related languages (especially the Greek), but they are more definite and regular in Sanskrit than elsewhere. In the Vedic

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dialect, especially, the same root not seldom makes two or more present-systems, on as many different bases. According to the mode of formation of the present-base, the Sanskrit verbs are divided into so many "conjugations," or "conjugation-classes." Hence the present-system is ordinarily called the "conjugational tenses," or the "special tenses"; and their bewildering variety of detail, coming at the threshold of the Sanskrit conjugation, is an especial difficulty to the learner. With the details of their formation we have here no concern.

II. The PERFECT-SYSTEM is, with minor discrepancies, formed in the same manner from all roots; it is characterized, as in Greek, by a reduplication and a somewhat peculiar system of (prevailingly primary) endings. It is called "perfect" in compliment to its analogy with the Greek perfect: in actual use, in Sanskrit, the "imperfect," "perfect," and "aorist" tenses are so many undistinguishable, or hardly distinguishable,* past or preterit tenses. In the Vedic dialect the perfect shades off, on the one hand, into the conjugation-class of reduplicated presents, and, on the other hand, into the intensive conjugation, and it often requires to be rendered as a present; and, in virtue of this relation, it has also a certain number of mode-forms there, and Delbrück (*Altindisches Verbum*) even assigns to it a few forms of an augment-preterit, and calls them "pluperfect"; but it is, at the best, a bastard formation; the Sanskrit verb has not at any period real modes of the perfect, or a real pluperfect.

III. The AORIST shows, up to a certain point, a very close analogy with the Greek. Thus, there are two general modes of formation, corresponding to the Greek first and second aorists. The one (Gr. 2d aorist) is a simple augment-preterit from the pure root, or from the root with added union-vowel only (like ἔβην, ἔταμον): a simpler preterit than the so-called "imperfect," and without corresponding present. The other (Gr. 1st aorist) has for characteristic an added *s*, of uncertain origin (usually regarded as coming from the root *as*, 'be'); but, unlike the uniformity of the Greek, it has four sub-varieties of form. Often (as in the example, *bhû*) the active aorist is of the one formation and the middle of the other; and in the Veda the irregularities are even much greater. In the Veda, too, modal forms, especially of the simple aorist, are abundant; in the classical Sanskrit only relics of them remain: the most important of them being the so-called "precative" or "benedictive." This is really an optative of the aorist, and is so classified in the scheme, though it possesses also some as yet unsolved peculiarities of ending; in the active it is always a simple-aorist mode; in the middle, always an *s*-aorist one (hence by no means always agreeing, as in our example, with the indicative aorists).

IV. Of the two FUTURES, the one, in *syâmi* or *ishyâmi*, is the old Indo-European future. It has an augment-preterit, of very rare occurrence, known as the "conditional," and standing to it in the relation of our *would be* to *will be*, or French *serais* to *serai*. The other future is the most recent of all the verbal formations, being entirely post-Vedic: it is made by adding the present tense of *as*, 'be,' to a derivative noun of agency in *tar*. It has no modal forms; and modal forms are only of the rarest and purely sporadic occurrence as belonging to the older future.

There are, as the scheme shows, three participles belonging to the tense-sys-

* Delbrück is on the point of bringing out the results of a detailed examination of their relations in Vedic use, and expects to be able to establish among them a certain degree of distinction.

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tems: a present, a perfect, and a future. The "passive" (or, in neuter verbs, simply "past") participle in *ta* (*bhûtá*, 'been') is from the simple root; the future passive participles, or "gerundives," are also purely adjectival derivatives (in part of secondary formation) from the root. A single infinitive in *tum* (*bhâvitum*, 'to be'), and a "gerund" in *tvâ* (*bhûtvâ'*, 'having been') complete the whole scheme of the simple verb.

The Sanskrit uses its "middle" forms in passive sense also, except in the present-system, where there is a peculiar base, in *yá*, analogous with the other bases of this system, and closely akin with one of them. The peculiar passive is thus hardly more than an additional "conjugation-class," parallel with the active-middle one from the same root. And not a little of the same character or aspect belongs to the other derivative conjugations: the desiderative, the intensive, even the causative. In all these, forms outside the present-system are rather sporadic and of recent make. The causative, to be sure (to which, it is to be noticed, a distinctively causative sense so weakly clings that the Hindu grammarians treat the formation also as an ordinary conjugation-class), has got attached to it, rather accidentally, an aorist of peculiar formation, a reduplicated aorist, which is tolerably abundant in the Veda. But of the peculiar periphrastic perfect eckoned as belonging to all these derivative conjugations (*bhâvayâm-cakâra*, etc., 'he caused to be,' lit. 'he made a causing-to-be'), there is not a single example in the Rig-Veda, and only one in the Atharvan. And futures in the Vedic Sanskrit are nearly as rare.

In actual occurrence, indeed, the forms of the present-system everywhere greatly predominate.* Even in the Rig-Veda they form three quarters of all (the finite or personal forms); and later the proportion grows still more unequal: in the Çakuntalâ, for example, they are as eight to one; in Manu, as twenty-five to one. This helps to give that peculiar conspicuousness to the "special tenses" which makes the Sanskrit verb seem so unlike the Latin and the Greek.

The perfect and the aorist, in the Rig-Veda, divide nearly equally between them the remaining quarter, the simple aorist being more than twice as frequent as the *s*-aorist; futures are infinitesimally few (only twenty). In the later texts the aorists almost disappear; in Manu, the Çakuntalâ, the Nala, and the Bhagavad-gîtâ, all together, there are less than fifty aorist forms. The future grows decidedly more common later; but the conditional is, through the whole history of the verb, the rarest tense of all: the Rig-Veda presents but a single example, the Atharvan not one; in all the texts specially examined, indeed, there are but seven instances of it (in a total of 35,000).

The Secretary read a communication from the Rev. Robert Ellis, B. D., of St. John's College, Cambridge, England, entitled "Observations on Dr. Trumbull's 'Numerals in North American Indian Languages.'"

"On perusing some of Dr. Trumbull's valuable contributions to the science of American languages, I found," writes Mr. Ellis, "that one of them [Transactions, 1874, pp. 41–76] was written in opposition to my own view, that numerals are generally derived from names for 'finger,' or some similar member of the

* See the Tables at the end of Prof. Avery's article on Verb-inflection in Sanskrit, Journ. Am. Or. Soc'y, x. 219 *et seq.*

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human body. This has led me to give some further consideration to the question of the origin of numerals, with especial reference to America; and it may, perhaps, conduce to a better knowledge of the subject, if I examine some of the explanations which my opponent has given of certain numerals, and particularly of those numerals which, in his opinion, contain no names at all for 'finger,' &c."

Of the two Algonkin names for 'one,' Dr. T. regards one (Massachu. *n'qut*, Chip. *ningoto*, Cree *nikut* 'some one') as "probably of earlier origin than finger-counting." That *ne* is a demonstrative pronoun in Algonkin affords, in Mr. Ellis's judgment, but a slight foundation for such an opinion, and if, as is always necessary in comparative philology, we make the field of enquiry sufficiently large, we shall have little difficulty in perceiving that such 'ones' as *ne-qut* and *nin-goto*, with the Adaihe *nan-cas* of Louisiana, belong to a class where each of the two elements implies a limb or member of the human body, and that it is probably as 'finger,' or 'hand-bone,' that they have come to signify 'one.' Such at least would be the writer's inference from the South American parallels to these North American words, as those parallels have been collected by him, chiefly from the vocabularies of Martius, in his *Ethnographie Amerika's*. "If the Chippeway *ningoto* 'one' be = Macuni *nhimco'o* 'finger,' it cannot well 'be of earlier origin than finger-counting'; nor can we assent at once to Dr. Trumbull's axiom (p. 45): 'Admitting the original unity of American speech, it is yet certain that its division into widely separated families must have preceded the origin, not of numerals only, but of the verbal or nominal roots from which names of numerals in the several families were derived.' Why so? It is a matter of common observation that the uncivilized and uneducated count by the aid of their hands and fingers; and, if I may borrow an example that will be familiar to us all, in the play of Henry V., the first words in our language that Katharine of France learns are 'hand,' 'fingers,' and 'nails.' But, setting aside such obvious illustrations as these, surely a principle like that of Dr. T. ought to be settled by induction, and not by assumption. We ought to compare the numerals themselves in widely separated families, and thus judge *a posteriori* instead of *a priori*." Mr. Ellis proceeds to do this, for the first three numerals, comparing the Algonkin with other North and South American names, particularly with the Cotoxo of Brazil.

Referring to Dr. Trumbull's explanation of names for 'two,' as derived, apparently, from roots denoting separation or distinction, as 'that,' 'the other'; likeness, equality; 'putting to' or 'putting with'; coupling, pairing, or the like—Mr. Ellis "cannot believe that an uncivilized mind invariably names the abstract before the concrete, or that savage tribes cannot begin to count until they have thus devised a score of terms expressive of various abstract conceptions." Other reasons forbid his acceptance of the derivation of Dakota *nape* 'hands' from *napin* 'a pair, they two'; "for the elements in each case, *n-*, *p-*, are limb-names, found as such in a vast number of words in Riggs's Dakota Dictionary" and (as is shown by an appended table) in many other American languages.

The *p* element has, in several instances, been followed by a *t* element, as in (S. A.) Machacali *pata* 'foot,' Patacho *pata* 'bone,' and Cariay *pata* 'nail,' etc. By prefixing this *t* element, instead of the *n* element, to the *p* element, we may obtain as 'one-one' or, ultimately, as 'finger-finger' or 'bone-bone,' the Algonkin 'twos' (p. 47) *ta-bu*, *te-pa*, *tu-pou*, and the S. American 'twos,' *ta-boe*, *ta-pu*, *da-bui*, and *tyu-wa*.



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By other lists of words, "the *p* root for 'hand,' etc., which is so common in both Americas, and which seems to occur in the Mandan 'ten,' *pirakh*," is shown in a great number of American languages, in words meaning 'hand,' 'foot,' 'finger,' 'arm,' 'toe,' 'thigh,' 'leg,' 'five,' 'ten,' etc.; and apparent correspondences are pointed out in Australian and African dialects.

The Algonkin 'ones,' *ne-qut* and *nin-goto*, as well as the Adaihe *nan-cas* 'one' and the Macuni *nhim-coto* 'finger,' would, like the Dakota *nap-tshupe* 'finger,' and the Chibcha *yta-quyn* 'finger,' have been originally equivalent to 'hand-bone'; *-qut*, *-goto*, and *-coto* being identical with the (S. A.) Bare *makuty* 'one,' the Marauha *nokoty* 'my toe,' and the Arawak *dacuty* 'my foot.' Dr. Hunter's *Non-Aryan Languages of India and High Asia* supply us with several bones like *-qut*, *-goto*, *-coto*, *-kuty*, *-koty*, and *-cas*, the nearest forms being the Amoy *kut*, the Canton *kwat*, the Gyami *kutho*, and the Pahri *kusa*. These forms come very near the West Australian words for 'bone.' Koelle's *Polyglotta Africana* gives us these words for 'bone': *hu*, *yuh*, *yuhu*, *ku*, *koko*, *kote*, *hote*, *kashi*, and *keeze*, as well as *kod* and *koko* 'nail'; and presents us with African parallels to other American forms—which are exhibited in a table.

The second form of the Algonkin 'one,' in Chippeway *pay-zhik*, when resolved like Chip. *nin-goto* 'hand-bone' or 'arm-bone,' also finds parallels in Africa.

Such analogies render Mr. Ellis unable to admit "that the language of America, granting it to have been originally one—and his inability extends, not merely to America, but to the world at large—must have ramified into widely separated families of speech before the formation of numerals, or of their component parts." "But," he remarks, "if I have not been shaken in my acceptation of Grimm's words, *Alle zahlwörter gehn aus von der Fingern der Hände*, by anything which Dr. Trumbull has said against it, yet neither can I hope to influence his views by anything that I can suggest. For the force of all the parallels which I have brought forward, whether here, or in my *Numerals*, or in my *Peruvia Scythica*, must be unavailing against the position he has taken (p. 48): 'Rigidly examined, these and a host of other coincidences which Mr. Ellis with much ingenuity presents, would prove to be less remarkable than they seem to him. It is not my purpose, however, to discuss them in detail, or to seek for them, collectively, any other explanation than the one which I am assured in advance "is not satisfactory"—namely, that so far as they are not imaginary, they "are merely accidental."'"

Dr. Trumbull offered some remarks upon Mr. Ellis's paper.

He said that he must still decline to discuss, in detail, the verbal coincidences detected by Mr. Ellis in widely separated languages, some of which are known only through meagre vocabularies. It would be hardly worth while, even were it possible, to demonstrate the radical unlikeness of Dakota *nonpa* 'two,' Barba *nombowa* 'lower arm,' and Bulam *nimbul* or *bul* 'one'—or of Chippeway *ningoto* 'one,' Ekamtulufu *nigita* 'toe,' Canton *kwat* 'bone,' Manao *nuku-ita* 'my foot-sole,' and Macuni *nhimcoto* 'finger.' In the absence of other evidences of relationship, correspondences of this sort, however numerous, prove nothing. In the paper which is the subject of Mr. Ellis's criticism, it was the expressed aim of the writer to show by examples taken from a few of the best known American languages, "that it is unsafe to assume uniformity in the *conception* or the *expres-*

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*sion* of numbers, even in the dialects of the same language, much less in languages whose affinity is not yet proved," and that the value of such coincidences as Mr. Ellis has pointed out, "must depend upon the analysis of the names and the ascertained meaning of their respective components or roots."

If, as Mr. Ellis observes, "it is always necessary, in comparative philology, that we make the field of enquiry sufficiently large," it is equally necessary that certain definite boundaries be recognized, beyond which the comparative method is not applicable. The general likeness of the vocabularies of two languages may be such as to give presumptive evidence of their genetic relationship; but no sound induction can be based on apparent correspondences of a few words in two dialects, known to belong to two distinct families of speech. Such correspondences are peculiarly untrustworthy in languages of the holophrastic or incorporating type. We have copious and reliable vocabularies of many American dialects and are enabled to analyze their words and distinguish their principal roots from affixes or formatives: yet no one has succeeded in establishing, by comparison of vocabularies, the genealogical affinity of any two American families of speech—of the Algonkin and Dakota, for instance, or of the Dakota and the Athapascan. It is only in their general plan of thought and in some peculiarities of structure that we have any evidence of their common origin. Of many of the dialects from which Mr. Ellis's apparent correspondences are taken, we know nothing of the grammar or modes of synthesis, and next to nothing of the vocabularies. In so vast a field, even the most cautious of comparative philologists must be in danger of losing his way.

A recess was then taken till 8 o'clock.

## Evening Session.

The Association met in the chapel of the Fourth Avenue Presbyterian Church, Professor S. S. Haldeman, Vice-President, in the Chair.

The Annual Address was delivered by the President, Professor Albert Harkness, on "The Progress and Results of Philological Studies during the Century."

One hundred years ago, philology had reached a stage in its course which began to foreshadow the coming of a new era. It had already traversed a wide field and gathered rich stores of fact and material for the future scholar. Four centuries had elapsed since the Italian philologists began to bring to light the literary treasures of antiquity. Germany had recently given birth to a generation of scholars second to none that the world had ever seen. Ernesti, in the ripeness of a good old age, was still lecturing at Leipsic; Heyne was in the midst of his brilliant career at Göttingen; Wolf, soon to occupy so proud a place among the scholars of the world, was enthusiastically poring over the classics at Nordhausen; Bentley, the pride and boast of English scholarship, had closed his labors a generation before; Porson, soon to acquire fame in the same field, was still a student at Eton; Hemsterhuis had been borne to his tomb at Leyden ten

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years before; but Ruhnken and Valckenaer, his ablest disciples and followers, at the height of their fame and in the midst of their usefulness, were winning golden honors for themselves and their master.

The first quarter of the nineteenth century was a memorable era in the history of philology. During this period there gradually grew up in Europe in close connection with classical studies an entirely new school, that of Comparative Philology. In its *development*, it belongs wholly to our century, but in its *origin* it claims connection with the distant past. It is the result of agencies set in motion thousands of years ago by the students of language on the Ganges, the Ilissus, and the Tiber. In the long ages of preparatory work which preceded it, ancient India, Greece, and Rome, modern Italy, France, England, Holland, and Germany have one and all performed in turn an important part.

Comparative Philology began to take definite form about fifty years ago. Under its guidance, the critical method of study and investigation, which had hitherto been confined to the Greek and the Latin, has since been extended to all the cognate tongues. I must not, however, weary you by attempting to describe the learned labors of those distinguished scholars by whom this great linguistic movement has been effected—of Francis Bopp, the illustrious founder of Indo-European Philology; of Augustus Frederic Pott, whose comprehensive genius spanned so large a portion of the field of linguistic research; of Frederic Diez, whose learned investigations in the Romance tongues mark an era in the study of language; of Jacob Grimm, whose Teutonic Grammar is a model in comparative philology; of Augustus William von Schlegel, whose Indian Library attests his devotion to Oriental studies; of William von Humboldt, the distinguished philosopher and statesman; of Burnouf, the famous interpreter of the Zend Avesta; of Curtius, of Steinthal, of Schleicher, of Corssen, of Whitney, of Max Müller, and of others scarcely less earnest or efficient.

But while these accomplished scholars have opened to the world a new field in the investigation of language, the older departments of philology have lost none of their interest or importance. Classical studies have received a new impulse, and have been cultivated with renewed zeal. In Semitic scholarship too, and indeed in almost every department of linguistic research, an immense amount of valuable and productive labor of the very best quality has been performed during the period of which we now speak.

But in the second place we must consider the results attained by the linguistic studies of the century.

Comparative Philology has established the relationship of all the languages and dialects now classed in the Indo-European family; has thrown light upon many obscure points in our own vernacular; has explained many remarkable forms and constructions in the classic tongues; and has placed the whole subject of etymology upon a sure and permanent basis. It has furnished us the means of tracing the history of words through countless centuries with unerring certainty, and has, in fact, given us a science of language, a science with well-defined principles and methods and with a wide field for its generalizations, a science which aims to comprehend language as the appropriate embodiment of thought, to discover the processes by which it has become what it is, to analyze its complex forms, to explain its countless phenomena, and in fine to penetrate the secrets of its inner life.

But linguistic studies also greatly aid us in the interpretation of ancient

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mythology. Mr. Gladstone, in endeavoring to solve the problem of the Greek myths without the aid of comparative philology, has attempted an impossibility. The Vedas of India, however, bring us so very near the primitive Indo-European period, that we discover not a few of the ancient myths in the very first stage of their development. As now interpreted by trustworthy scholars, the gods and goddesses of Greece and Rome were originally mere physical forces, blind and inexorable, while ancient myths were simply poetical representations of the action of these forces in the varied changes and movements constantly taking place in nature around us, whether in the regular alternations of day and night, of summer and winter, or in the more sudden and startling exhibitions of power as seen in the tempest and the whirlwind, the thunder and the lightning. The incarnation of these powers converted them into gods and made them personal actors in many a poetical legend originally intended to describe natural events.

Again, comparative philology and linguistic science are rapidly becoming our chief dependence in the revision of ancient history. Many of the views of our fathers upon ancient life and ancient civilization rested upon a foundation which is beginning to give way. In the light of more recent studies, the historian is now called upon to revise his conclusions upon many points long deemed settled. Mommsen's brilliant success in restoring to us important chapters in early Roman annals, attests the value of linguistic studies in historical research. Many lost tongues have been recovered, and their secrets revealed. The Egyptian hieroglyphics and the cuneiform inscriptions of Persia, after having remained long centuries absolutely unintelligible to every scholar on the face of the earth, are now read with as much ease and certainty as Homer or Virgil. The opening of the Sacred Vedas of India, and of the Zend Avesta of Persia is one of the grandest triumphs of linguistic scholarship. These remarkable works give the historian a new point of observation a thousand years nearer the beginning of our race than any previously occupied.

But our list of the contributions which linguistic studies have recently made to history, is not yet complete. In our survey we have gone back into the past only as far as written monuments carry us; but language guides us with a steady hand through the dark labyrinth of antiquity long after all written records fail. To the scholar thoroughly versed in linguistic science, language itself is an historical monument upon which he reads the annals of the distant past. To his eye it is richly inscribed in every part with clear and legible characters. It is thus by the aid of language itself that modern science is beginning to throw its light over that distant antiquity from which no literary monuments have reached us.

In view of the wonderful achievements of the past, the rare opportunities of the present, and the unparalleled inducements of the future, the practical question of our own duties to linguistic science forces itself upon our attention as we now pass the threshold of the new century. The heirs of countless generations, we have ourselves received a glorious inheritance of knowledge and of opportunity, and it now becomes our highest duty and our choicest privilege, as we stand here to-day between the past and the future, to transmit that precious heritage not only unimpaired, but vastly enlarged and enriched. In the light of recent researches, we cannot fail to see opening before us a career rich in discovery and achievement without a parallel in the annals of linguistic study. The problems which are now waiting for solution at the hands of the student of

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language are of sufficient difficulty and importance to entitle them to his best thought, and of sufficient number and extent to occupy the linguistic scholarship of the entire century upon which we are now entering.

Never since the revival of learning have the students of language had stronger reasons for encouragement than at the present time. Never were their prospects brighter. The horizon of linguistic knowledge is rapidly widening in all directions. The investigator now enters upon his work with a completeness of preparation and a richness of professional appliances never before possessed. At his bidding, one tongue after another which has been silent for ages begins to speak in clear and distinct tones from the distant past; one monument after another delivers up the secret which thousands of years ago was committed to its keeping.

But with all these records open before us, with all the recent revelations from the past fresh in our memories, how little do we really know of the history of the human family! It is only in regard to a few centuries out of all the ages that have elapsed since the creation of man that we have anything like a respectable knowledge, and even within this narrow range of time our information is limited to a few nations and races. In view of such a fact, the recovery of lost history becomes the problem of the age. How transcendently important then must be the work of that devoted band of scholars and philologists who are so industriously collecting, deciphering, and explaining the various records of the past; how numerous and vital the issues involved in their full and complete success! Let us hope that so long as a single language or dialect remains to be deciphered, a single inscription to be read, or a single literary monument to be interpreted, they will never lose heart or desist from their noble work.

The Association thereupon stood adjourned to 9 o'clock Wednesday morning.

### Wednesday, July 19—Morning Session.

The Association met at the Chapel of the Presbyterian Church, the President in the chair.

The Secretary reported the election of new members:

Mr. W. G. McCabe, Petersburg, Va.; Professor John A. Broadus, Southern Baptist Theological Seminary, Greenville, S. C.; Mr. Thomas C. Murray, Johns Hopkins University, Baltimore, Md.; Professor William T. Gage, Hartford Female Seminary, Hartford, Conn.; Mr. John Storer Cobb, 69 William St., New York City.

On motion, it was

*Resolved,* That fifty copies of the volumes of Transactions recently published be placed at the disposal of Mr. John R. Leslie, Secretary of the Local Committee at Newport, for distribution to contributors to the funds of that committee.

On motion, it was

*Resolved,* That a committee of five be appointed by the chair to recommend a suitable time and place for the next meeting.

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The President appointed as such committee, Dr. A. C. Kendrick, Professor W. D. Whitney, Professor C. H. Toy, Professor S. S. Haldeman, and Professor J. B. Weston.

Mr. E. G. Sihler, of Johns Hopkins University, Baltimore, Md., presented the first paper of the morning, on "The Historical Aspect of the Old Attic Comedy."

Having called attention to the ultimately historical character of all study of literature, and the subsidiary relations of most disciplines of philology towards history, the author quoted several instances of the beneficial help to historical investigation from several non-historical Greek writers. He treated the theme chosen in a theoretical chapter and a practical one. In the former one he analyzed the literary-historical traditions concerning Attic Comedy which had been handed down from the Alexandrine, the Hadrianic, and the Byzantine period of literary studies, and severed the autoschediastic figments, attributing the origin of comedy to the denunciation by the country people of evildoers from the city, from the small amount of real information conveyed in these "prolegomena."

In accounting for the origin of Comedy from the merry and lascivious vintners' festival of fall-time, he pointed (with Grote) to the analogous origin of a species of poetry which, however, never (unlike Attic Comedy) rose from its homely character—the *versus Fescennini* of Ancient Italy.

Two characteristic literary phenomena of all the ancient Attic Comedies sprung from the origin mentioned, namely the tendency towards the γελωτοποιία and the ἰαμβικὴ ἰδέα, as Aristotle calls the latter. These should modify in a certain manner our utilization of the comedy writers as historical sources of information.

This theory of utilization was illustrated by the writer in the second chapter, in which he gave concrete instances, extending from B. C. 440 to 392. For instance, it was pointed out, how grossly incorrect and unfair is the view conveyed in Aristophanes's Acharnians about the real cause of the Peloponnesian war, as well as about the occasion of its outbreak and about the part Pericles played therein. But here, as well as in the treatment of Socrates, Aristophanes gives us a clear proof of the consciousness of the masses and the view they held of him. The difficulty of attaining to a positive knowledge of real facts was exemplified in the instance of Cleon; the main value was shown to consist in the indirect evidence given about the state of mind and of things at given times, the conclusion of peace in 421, the desperate state of public affairs in 411 and 405.

In an appendix, the author essayed to show that the communistic theories caricatured by Aristophanes in his Ecclesiazusae are really those of Plato. The detailed coincidences were too strong, and an earlier publication of these theories by Plato was highly probable; for many of his earliest dialogues showed how much Plato was occupied with helping towards political reformation and reorganization by an entirely new theory on which to arrange the life of society.

The Secretary read a paper by Professor W. W. Goodwin, of Harvard College, Cambridge, Mass., on "'Shall' and 'Should,' and their Greek Equivalents."

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Professor Sewall, in his paper on Greek Conditional Sentences in the 'Transactions' for 1874, and Professor Morris, in his paper on the same subject in 1875, have criticized especially the point in my classification of conditional sentences in which I maintain that the optative in ordinary protasis is "merely a vaguer or less vivid form than the subjunctive for stating a future supposition," and they both maintain the justice of the distinction commonly made between the two forms, based upon the greater or less *possibility* or *probability* that is implied, or upon the presence or absence of an *anticipation* of the fulfilment of the condition.

One great gain has thus far come from the discussion,—the clearer statement of one of the important points in controversy; for I understand it to be agreed that the difference between ἐάν with the subjunctive and εἰ with the optative is essentially the same as that between 'if he shall' and 'if he should' in English, and that if we can determine the principle that underlies the latter distinction, we have the key to the former. And I am perfectly willing to leave the question to this test: Do ordinary people distinguish the English 'shall' and 'should' in protasis as most grammarians distinguish the Greek subjunctive and optative? I have never found a person not prejudiced in favor of some theory of Greek distinctions, who ever thought of such a principle in English as the one in question. I cannot, however, admit in one case more than in the other the disturbing element of "rhetorical effect," by which supposed anomalies are often explained. If on this principle one form can be used for another at pleasure, I feel very strongly that scientific accuracy demands that we should carefully re-examine the foundation on which the distinction in question is supposed to rest. For example, I cannot admit that there is any such "supernatural intervention," (as the physicists would say) in Ter. And. II. 1, 10: *Tu si hic sis, aliter sentias*, where the future condition is evident enough as soon as the foreign element of "possibility" is eliminated, the sense being: 'If you should ever be in the same scrape, you would take a different view of things.' The fact that the same man might have said a slightly different thing (of course, in a slightly different sense) with the same general effect, does not prove that the two expressions would have been identical in force.

I notice that Professor Morris gives as a rule confirmed by his observation, that ἐάν with the subjunctive never can express a future supposition "demanding for its fulfilment a violation of physical laws," and he therefore denies my statement that ἐάν with the subjunctive must be used to translate the proverb: "If the sky falls, we shall catch larks." One of the instances adduced by him to illustrate this rule is Plat. de Repub. II. 359 c, where the supposition of a man possessing the ring of Gyges with its miraculous power comes clearly under the category in question; and he asks whether I think the subjunctive could have been used in this case. I run no risk, surely, in saying 'Yes' here, as I find another reference to the ring of Gyges in the Republic (x. 612 b), in these words: ἐάν τ' ἔχῃ τὸν Γύγου δακτύλιον ἐάν τε μή, and the having the cap of Hades is now added to the first miracle! I cannot think that any such principle as Professor Morris suggests can have existed, if Demosthenes can say (as he does, Phil. III. § 68): ὥστε μήδ' ἂν ὁτιοῦν ᾖ δεινὸν πείσεσθαι, where clearly ὁτιοῦν is a sort of *x* for which we can substitute anything imaginable.

I think the mistake here lies in confounding a very common (perhaps the *most* common) use of the distinction between the optative and the subjunctive with the distinction itself, or confounding the largest species in a genus with the

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genus itself. On this point I must refer to the 'Transactions' for 1873 (pp. 69–73), where I have discussed this matter at length, and where I have urged at least one new argument in favor of my view of the subject which is not answered in either of the papers to which I have referred.

I must also refer to the same paper (p. 68) for my special grounds of objection to the classification adopted by Professor Morris and Professor Sewall, both of which (as it seems to me) fail to recognize the "present general suppositions" expressed by ἐάν and the subjunctive. I do not refer here to the "general" character which can be given to any class of conditions without essentially changing their nature: this, as I have already said (*ibid.* p. 66), has been recognized by Bäumlein and other grammarians, and is doubtless included by Professor Sewall under "suppositions relating to contingent fact" and by Professor Morris under the "expectant form," as it certainly is by Bäumlein under his single principle of "Tendenz zur Wirklichkeit" which he applies to all subjunctives. I refer, on the contrary, to the use of the Greek subjunctive to express a general condition which the Latin regularly (and the Greek itself occasionally) expresses by the *present indicative*,—a form of condition in which the relation of ἐὰν ἔλθῃ, 'if he ever goes,' to ἐὰν ἔλθῃ, 'if he shall go,' is precisely analogous to that of εἰ ἔλθοι, 'if he ever went,' to εἰ ἔλθοι, 'if he should go' The former is a "variation" (so to speak) of the ordinary present condition expressed by the present indicative, just as the latter is a variation of the ordinary past condition expressed by the past tenses of the indicative. It seems to me that these "quasi-present" conditions find no proper place in either Professor Sewall's or Professor Morris's scheme of classification, any more than they do in the older scheme of Bäumlein.

Professor B. L. Gildersleeve, of Johns Hopkins University, Baltimore, Md., presented a paper on "The Use of εἰ with the Future Indicative, and that of ἐάν with the Subjunctive, in the Tragic Poets."

The ἐάν condition was divided thirty years ago by Bäumlein into particular and generic. Accepting this division, which is naturally suggested by the character of the apodosis, it is proposed to examine the use of the former class of the ἐάν condition as compared with the form in εἰ with the future indicative.

The ἐάν conditional sentence differs in future relations from the form εἰ with the future indicative—

1. In its greater temporal exactness;
2. In the absence of special tone.

For these reasons it is preferred in prose to εἰ with the future indicative, which is used chiefly—

1. In minatory and monitory conditions;
2. When the future is used in a modal sense, with translation 'is to', 'must', etc.;
3. In connection with verbs and phrases of emotion (semi-causal), such as αἰσχύνομαι, ἐλεῶ, δεινόν, αἰσχρόν, κ. τ. λ.

As the harsher, sterner form of the future condition, εἰ with the future indicative is far more frequently found in tragic poetry than in normal prose, and occurs in diminishing ratio in the three great tragic poets. In Aeschylus over seventy-three



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per cent. of future conditions are in the indicative; in Sophocles, over fifty-four per cent., or, if we leave out the Oedipus at Colonus and the Philoctetes, sixty-two and a half per cent.; while in Euripides only about forty-three per cent. have this form. A striking contrast to the usage of the tragic poets is presented by Aristophanes. In his future conditionals little more than twenty per cent. take the form εἰ with future indicative, and of sixty-seven such conditions fifty-three are minatory or unfavorable. Prose writers vary according to theme and individuality. In the speeches of Thucydides the proportion is nearly the same as in Euripides. In Lysias, judging by twelve speeches, εἰ with future indicative seems to occur with exceptional frequency. In Isocrates there is a marked variation in different discourses. In Xenophon's Anabasis the proportion is about the same as in Aristophanes.

When ἐάν with the subjunctive and εἰ with the future indicative are used in antithesis, which does not occur very often, the tendency is to put the more favorable hypothesis in the subjunctive, the less favorable in the indicative. We may compare with this the common elliptical use of εἰ δὲ μή, after ἐάν μέν.

Professor Lewis R. Packard, of Yale College, New Haven, Conn., read a paper on "Grote's Theory of the Structure of the Iliad."

The principal German criticisms of Grote's theory (by Friedländer, Düntzer, Ribbeck, W. Bäumlein) were briefly noticed, the last of the four as covering nearly the same ground with the following examination. The purpose of this paper is simply to test the validity of Grote's views and arguments, and to urge what can be said against them.

Grote's first argument is that in the Achilleid "the sequence of events is more rapid, more unbroken, and more intimately knit together in the way of cause and effect" than in the other books. On this point the impressions of different readers will vary. If tested by the number of lines required for the events of a single day, there is no great difference between the Achilleid and the rest of the poem. Four days cover three, five, six, and eight books respectively, while the other days in the action occupy much less room or are passed over in a single line. These four days contain the critical points on which the whole action turns. The same variation appears in the Odyssey, the unity of which Grote admits.

The next general argument is that "the consequences of the anger of Achilles do not appear until the eighth book." It is answered that the method of art in the construction of a poem admits such delays as occur in real life and uses them to increase the suspense and interest of those to whom the poem is delivered. Again, the prompt fulfilment of the promise of Zeus which Grote demands is not in harmony with the character of Zeus as conceived by the poet, nor with his action in the Achilleid itself. Again, the delay may properly be explained by the intent of the poet to display the inability of the other Greek heroes to make up for the absence of Achilles. The scenes where one after another is tried and fails to bear the burden of the fight, are not confined to the parts of the poem that Grote rejects. Grote's objections to this familiar idea of the poem were examined and found to have no weight.

The false dream in the second book and the wall-building in the seventh which Grote considers as marks of unskilful joining of the enlarging material

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to the original Achilleid, were next considered and defended against some of his criticisms. The seventh book is admitted to be open to serious criticism, but the particular objections which Grote makes to its account of the wall-building were shown to be unfounded.

Grote's rejection of the ninth book was then discussed by itself. The disasters of the Greeks in the previous books appear sufficient to cause alarm, and such variations in courage as Agamemnon shows between this and the eleventh book are not unusual in the poem. The obstinate desire for revenge in Achilles is not to be ganged by modern morals and seems to find its nemesis in the death of Patroklos. What Achilles asks in the first book is not that the Greeks may be humbled, but that they may be driven back in defeat to their ships, before which event he will not fight. This purpose he repeats in the ninth and sixteenth books. Hence the embassy in the ninth book does not bring what ought to be a full satisfaction to his anger. The subsequent passages, viewed in the light of this theory, are not inconsistent with the previous occurrence of the embassy, as Grote considers them. One line in particular (xiii. 115) he seems to mistranslate.

Hence on the whole, to pass over some minor points, Grote's view of the structure of the Iliad seems to be open to serious and unanswerable objections.

Professor Milton W. Humphreys, of Vanderbilt University, Nashville, Tenn., read the next paper, on "Negative Commands in Greek."

A negative command, when addressed to an individual with reference to a special thing, is likely to offend, because it does violence to the will, and assumes that the person commanded would do the thing prohibited unless forbidden; but a general command is not as likely to give offense, as it generally presumes nothing with respect to what the person commanded is going to do, and often is such that he does not apply it to himself at all. Hence, general commands represented by the present imperative remain unmodified when negative, while special commands represented by the aorist are mollified by substituting the subjunctive for the imperative. In the third person, which often relates to an absent individual, the usual courtesy is not necessary, and the aorist imperative may be used. A similar influence of politeness is found in Latin, Sanskrit, Hebrew, and many other languages, ancient and modern.

(Some special considerations are omitted here.)

Professor C. H. Brigham, of Ann Arbor, Mich., read the last paper of the morning, on "Cornill's Examination of the Aethiopic Book of the 'Wise Philosopher.'"

Maṣhafa Falâsfâ Ṭabîbân, 'The Book of the Wise Philosopher,' has been studied from the original Aethiopic manuscripts by Dr. Carl Heinrich Cornill, who has translated the text and illustrated it by copious notes from other Oriental tongues. The book itself is of Greek origin, and has been many times translated into Oriental languages. The Aethiopic version was made from an Arabic version now lost. The Aethiopic translator was a Bishop, Michael by name, of whose life and other work no account has survived. Of this Aethiopic version

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there are several manuscripts in the libraries of Europe. The two which Dr. Cornill studied are the codex of Frankfort and the codex of Tübingen. Of these the Frankfort codex is the better preserved and more complete, written on two hundred and nineteen large octavo parchment pages, in double columns. Marginal notes in red ink have been added, and alterations and erasures made, by a later hand. The Tübingen codex, of inferior execution, is written in flowing script on eighty-four pages of paper. It contains only three-quarters of the book. Other manuscripts of the book, not used by Dr. Cornill, are in the National Libraries of London and Vienna. The book is alluded to by the Dutch author, Theodore Petraeus, in a note in his translation of the Aethiopic Book of Jonah into Latin, published in 1660. He speaks of the book as of high authority among the Aethiopians, as well for its thought as for its style.

Of the Arabic original nothing is known. It was probably written in the jingling prose which the Arabs prize as the most beautiful literary style. The compiler was evidently a Christian, familiar with the church fathers and classical Greek and Roman writers, as well as with unknown Arabic authors. His quotations are various and numerous. The introduction to the book is a grandiloquent glorification of wisdom. It rehearses the benefits of the wise counsels of the book, and asserts that it will save readers from heresies as well as from sins. It praises the Scripture as the source of all true knowledge, and exalts the capacity of the human soul. The book itself has no logical order, but is only a loose collection of disconnected sayings, of proverbs, stories, and prayers. There is a long list of the proverbs of Haikar, some of which are striking. The proverbs of Sextus, six of which are given, are sound moral advice. The citations of Greek wise men in the Aethiopic book are not in their thought or their phrase. Plato's talk is unspiritual, and Aristotle utters what sounds like Hebrew proverbs. The citations from Gregory and Basil are not in the style of their genuine writings. In the Frankfort codex there is a story of Socrates and the King, in which the philosopher takes the part of the cynic Diogenes. Another saying of Socrates seems to have been borrowed from the Arabic of Hussaim. The book has several sayings of Diogenes, and also of Alexander the Great. It has also a short prefix, in four parchment leaves, in which are two prayers to Christ and one to the Virgin Mary; one of the prayers is in verse. There is a suffix of ten parchment leaves, which contains an Aethiopic translation of the Creed of Jacob Baradaeus.

As a literary work the book is not of high value. Its chief importance is in illustration of the Aethiopic language and its relation to cognate dialects.

A recess was taken until 3 o'clock.

## Afternoon Session.

The Secretary reported the election of new members:

Mr. H. H. Smith, Shelbyville, Tenn.; Professor W. B. Carr, Petersburg, Va.; Mr. H. C. G. Brandt, Johns Hopkins University, Baltimore, Md.; Professor D. S. Martin, Rutgers Female College, New York City; Mr. James W. Shearer, Liberty Corner, N. J.

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Professor D. S. Martin presented an address of welcome from the New York Academy of Sciences; and it was, on motion,

*Resolved*, That the thanks of this Association be offered to the New York Academy of Sciences for its address of welcome; and that this Association takes pleasure in reciprocating the kind sentiments therein expressed.

On motion, it was

*Resolved*, That a Committee of five be appointed by the Chair to nominate officers for the next year.

The President appointed as such Committee, Chancellor Howard Crosby, Professor Schele De Vere, Professor J. B. Sewall, Professor Tracy Peck, and Professor De Witt Reiley.

Professor Samuel Porter, of the National Deaf Mute College, Washington, D. C., read a paper on "The Terms 'Substantive Verb,' and 'Verb of Existence,' and the Nature of the Distinction of Subject and Predicate."

Certain recent lexicographers have misapplied the terms in question, restricting them to the signification of simple existence, in contradistinction to the use as copula: namely, Freund, followed by Andrews and by Riddle ("substantive verb"); as likewise Liddell and Scott, but without authority from Passow; and Robinson ("verb of existence") in his "Greek and English Lexicon of the New Testament."

I.—History of these Terms.—In Priscian (*Inst. Gram.*, viii., x., 51) we read, ". . . . excepto *sum* verbo, quod *ὑπαρκτικὸν* Graeci vocant, quod nos possumus substantivum nominare;" he employs the term repeatedly (see viii., xviii., 101; xi., i., 3; xiii., iv., 18, 19, 20; xiii., vi., 34; xvii., v., 35; xviii., i., 2, 5, 6, 10, 15). In most of the instances, we have "verba substantiva vel vocativa" as in similar construction. In all it is *sum* as copula. Priscian, doubtless, took the designation from Apollonius Dyscolus, in whose extant work, *περὶ σύνταξεως*, there is a reference (p. 219, ed. Bekker) to the *ὑπαρκτικὰς σύνταξεις*, with *εἰμί* as copula. Theod. Gaza (Book iv.) divides neuter verbs into "*τὸ μὲν ὑπαρκτικόν, τὸ δὲ οὐ.*" He resolves the *ῥῆμα* (finite verb) into the cognate noun or participle and the *ὑπαρκτικόν* of the same finite form. He uses the term, also, in treating of certain constructions with *εἰμί* as copula. J. C. Scaliger (*De Causis L. L.*, Cap. cxii., on the Verb *sum, es, est*), after remarking that we can say "Cæsar is white," as well as "Carbo is a dog," proceeds: "Quare hoc verbum tam accidens quam substantiam quum significet, pessime a grammaticis verbum substantivum dictum est." He goes on to say that it can be used either simply added to a noun, as "Cæsar est," or as copula, as "Cæsar est albus."

The designation "verb substantive" is much older than "noun substantive." The origin of the latter is hard to be determined. Priscian distinguishes the "nomina adjectiva" as a class, and says (ii., v., 25): "Adjectiva autem ideo vocantur, quod aliis appellativis, quæ substantiam significant, vel etiam propriis,

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adjici solent";—but he never designates the opposite class as *substantiva*, or recognizes it as a class that need have a name. Gaza, also, marks a class of nouns as adjective, and two others as proper and as appellative, but none as substantive; though the proper and the appellative he distinguishes, as the one signifies first, or particular, and the other second, or general, substance (οὐσίαν). Scaliger is the first grammarian of note who employs the designation. He speaks of it as current then and previously, and as the opposite of "noun adjective"; though some would prefer, he says, *firum* and *mobile;* and he himself, *essentiale* and *denominativum* (*De Causis*, XCI., CXXVII., CXXX.). Some forty or more years earlier, in a school-book, *Fundamentum Scolarium* (Basiliae, 1499), nouns are described as of two sorts, "substantiva" and "adjectiva"; and the *Exercitium Grammaticale Puerorum*, 1495, treats "de generibus nominum adjectivorum, substantivorum, et participiorum." So that Dr. K. E. A. Schmidt (*Beiträge zur Gesch. der Gram. d. Gr. u. d. Lat.*) is not quite correct in what he states on this point.

G. J. Vossius says (*De Analogia*, III., ii.) of verbs that are neither active nor passive in sense, "quaedam neutrum, ut *sum, fio, morior, intereo*, quæ dicuntur substantiva, quia notat substantiam esse, fieri, vel esse desinere"; a quite arbitrary extension of the term on the part of Vossius. See, further, his use of the designation in *De Constructione*, XXIV. John Milton (Grammar) gives the rule of syntax for "the verb substantive *sum* and such like." Dr. John Wallis (*Inst. Logica*, II., ii.) says the copula is the "verbum substantivum (quod dici solet) *sum, es, est,*" and the essence of every verb as such. Wolff (*Philosophia Rationalis*) says, "Copula non est nisi verbum substantivum praesentis temporis." The *Grammaire Générale de Port Royal* makes affirmation the essence of the verb, and proceeds: "Selon cela, l'on peut dire que le verbe de lui-même ne devait point avoir d'autre usage que de marquer la liaison que nous faisons dans notre esprit des deux termes d'une proposition; mais il n'y a que le verbe *être*, qu'on appelle substantif, qui soit demeuré dans cette simplicité," etc. (Chap. XIII). It says further (Ch. XVIII.) that all other verbs, as conjoining some attribute with the affirmation, might be called *adjectifs*. It makes of the verb *to be* an "adjective verb" when it signifies simple existence. Beauzée, instead of the terms substantive and adjective, applied to verbs, prefers *abstrait* and *concret;* and Du Marsais, *simple* and *composé*. James Harris (*Hermes*, I., vi.), specifying "the verbs *is, groweth, becometh, est, fit,* ὑπάρχει, ἐστί, πέλει, γίγνεται," says: "The Latins have called them *verba substantiva*, 'verbs substantive,' but the Greeks ῥήματα ὑπαρκτικά, 'verbs of existence,' a name more apt," etc. The principal of those verbs is the verb ἐστί, *est, is.*"

In some German Cyclopædias—Zedler, Walch, and probably Chauvin—*habe* is given besides *bin* as verb substantive. Heyse, in his *Schulgrammatik*, says that *sein* as copula is called "verbum abstractum (weniger gut, v. substantivum) oder reines verbum"; and that *werden* and *haben* when mere form-words are "verba abstracta," and that all others are "verba concreta (auch v. adjectiva genannt) oder gemischte verba." In Zumpt's Latin Grammar (Eng. version), the verb *esse*, as copula, is uniformly spoken of—improperly so—as "the auxiliary *esse*." Ueberweg (*Syst. der Logik*), also, calls *sein*, in the same sense, "Hülfsverbum."

To conclude: It appears that the term "substantive verb" originated in an inadequate and faulty rendering from the Greek, since οὐσία, not ὑπαρξις, is the Greek for substance. Neither term was applied with any special reference to the

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notion of bare existence. Priscian gives nothing in the way of reasons for the original term or the rendering. The English language probably owes the expression "verb of existence" to the author of *Hermes*; there is nothing corresponding with this in the term *Existentialsätz* (proposition of existence), used sometimes by German logicians, and applicable only when the verb is used not as copula. The citations given above make the *usus loquendi* of the terms in question sufficiently evident.

II.—The Distinction of Predicate and Subject.—Every proposition gives us a complex object of thought, involving as components at least two thought-objects in mutual relation. E. g., 'Brooklyn is near New York.' Here we have one object, Brooklyn, as subject, and a relation to the other, New York, as predicate. The total object remaining the same, New York might be the subject, and the relation to Brooklyn the predicate. It is just so, when the relations are distinctly contrasting, as in the case of a bridge over a river, or a statue on a pedestal. Again, we may have, e. g., 'The horse is black.' Here a part of the total object, related to the rest and to the whole, is a something, whether we say it is a quality, or a sense-presentation, or a sense-product. Again, e. g., 'The man walks.' Here the total object includes the feet and limbs, the ground, and places from and to which the man walks. And we can make, for instance, feet the subject, and say: 'The man's feet carry him.' Every transitive verb requires a distinctly prominent second object, which can take the place of the first as subject.

What the total *object of thought*, as such and by itself, thus gives is, however, merely the distinction of subject and attribute—rather it should be said, merely the foundation for even this distinction. For that of subject and predicate, we have to look further, namely, to the *mode of thought*. This is meant to include not only knowledge assured, belief, doubt, supposition, etc., but any desire, emotion, or intent of the speaker, as respects the objects or the words uttered. We have not a predicate, till we come to language as used for communication. The mode of thought, as respects the predicate, is a desire to make known something, and such as leads to the use of the word, or sign, as a means to the end. Or, more accurately, it is a desire to make known one's own knowledge or belief of some fact, and leading to the expression as a means. This is the mode of *affirmation*. As there is no predicate without a subject, another mode of thought is involved as concerns the latter. This may be called the mode of *designation*, as presuming that the object is already known, and simply indicating or designating it as a subject of the attribute affirmed. These two modes of thought, thus existing together, are the primary ground of the distinction of predicate and subject.

It accords with this explanation that the words earliest set apart formally as verbs, or predicative words, should be those signifying temporary actions or events. Verbs and nouns are alike connotative of attributes, temporary or permanent; but there would be far more frequent occasion for affirming the temporary than the permanent, while the latter would, in most cases, be simply designated as subjects of an attribute affirmed. It would be only ordinarily, not necessarily, so. Two primitive men seeing an animal in the distance running, one might say to the other, 'That—there—run—wolf,' making 'run' the designative word, and 'wolf' the predicative,—neither, it is to be observed, being yet a noun or a verb, but each merely connoting an attribute. The ordinary, not the exceptional,

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would, however, determine the earliest application of the formal. The designative and predicative would thus coincide, to a large extent, with being and activity, with substance and accident, with the more and the less concrete, yet without having any of these as the real ground of the distinction of subject and predicate, or of noun and verb. These relations are, however, not without influence when in the progress of language the primary ground falls away in part, as that it does will presently be shown.

Many propositions give us two nouns, equally known beforehand, and alike subjects by designation; e. g.: 'Brooklyn is near New York'; 'John loves his father.' Here we have to seek for a further reason why one stands as subject and the other is subordinated to the predicate. We find this in precedence, or prominence, as the center of interest, as that about which there is the desire to know or to make known. Also, as between a proposition and its logical converse, this may often be the sole ground of the distinction; e. g., 'Some Frenchmen are learned men;' conversely: 'Some learned men are Frenchmen.'

But the forms of language are so accommodating, or elastic, that in many cases neither of the foregoing reasons will hold good for the grammatical form that is actually employed.

Thus, under any conceivable circumstances, we should probably say, 'Brooklyn is near New York,' rather than 'New York is near Brooklyn'; for the reason, doubtless, that Brooklyn is the smaller place. If, in view of the dead body of a man, one should say, 'An Indian shot him,' the subject in thought would be the same as if it were said, 'He was shot by an Indian.' In Latin and Greek, often there would be no reason for calling the nominative and not the accusative the subject, were it not that in the simplest form of sentence the subject is in the nominative case. The use of the article in Greek in instances such as *Μακάριοι οἱ πραεῖς*, etc., accords with the primary ground of the distinction as stated above; but, when prefixed to an object accusative, the article serves to bring that also really under the designative mode, and may thus sometimes give it the precedence in this respect over the nominative. In propositions of identity, as, 'Thou art the man'; 'To-morrow is our wedding-day'; the predicate-nominative is the real subject in thought as often as otherwise. In numerous instances the predicate verb itself is no predicate in thought. If, in sight of a person riding, one says, 'He rides well,' the quality, not the fact, of the riding is what is affirmed. To say, 'The emperor sits on the right,' might mean: 'The one sitting on the right is the emperor.' If one should say, 'Mr. A. B obtained his appointment by corrupt means,' the real predicate would be the word 'corrupt,' formally indicated thus: 'The means by which he obtained his appointment were corrupt.' Consider the different forms employed by different languages for the very same thought. The forms of language have not a constant value as exponents of thought-relations. On this head see Dr. Steinthal in various places. See also Dr. Wallis (*Inst. Log.* II., ii.). See also articles by Geo. v. d. Gabelentz, in the *Zeitschrift, etc.*, of Messrs Lazarus and Steinthal (Vols. VI. and VIII.), in which he distingishes what he calls the psychological subject and predicate from the grammatical.

In short, all that can be claimed universally for the finite verb is that it is necessary to give predicative value to the normal sentence; though sometimes itself signifying no part, and at other times not the main part, of the predicated notion. While the forms of language have their ground in forms of thought,

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the forms, once moulded and fixed, accommodate themselves to a great variety of content. The form actually employed in a given case is often determined by reasons different from the ground of its origin as a form.

The writer of this paper does not accept the dictum that every expression of thought requires both a subject and a predicate. In impersonal verbs, the pronoun or pronominal termination is a formal element that serves merely to give affirmative value. As in *tædet me vitæ* the real subject is *me*, so in *it rains*, *pugnabatur*, etc., there is no real subject. And, there being no subject, there is, in the strict sense, no predicate; there is only something affirmed as existing. And, further, *pugnabatur* might be drawn out in a detailed narrative, throughout which there should be no other than a formal distinction of subject and predicate. In very much of narrative and descriptive discourse, what belongs to the predicate in thought covers more than what is predicative in form.

In the members of a syllogism, that the signs of quantity, 'all,' 'some,' etc., are affixed normally only to the subject, is in accordance with the primary ground of the distinction in question; but the distinction itself is no way essential to the reasoning process (see Sir Wm. Hamilton's *New Analytic of Logical Forms*).

The primitive, the most simple, and a still much used means of indicating the distinction of (the real) predicate and subject, is emphasis, in the way of stress, tone, pause, etc. In the sign-language of deaf mutes, what corresponds to this in gesture and expression is the sole means of indication. Another means, equally primitive, is order of collocation. The natural order will, unquestionably, put the subject first, in the absence of other and countervailing reasons. In some rude languages, particles of a very indefinite significance serve the purpose, and do in some cases fulfil the office of copula—such, e. g., as the *wa*, a grouping and separating particle in Japanese, and the *ja* (*ya*) of the Alifurese in North Celebes. In the Dakota, there is an article, postpositive, annexed to words and groups of words, and often distinguishing subject from predicate; e. g., "And swine what eat *the*, even-that with fill-himself desired;" "Thy-kingdom *the*, come shall" (the *shall* a mere particle). (Riggs's *Grammar*, etc.) In Hebrew, the use of the personal pronoun and of certain particles, to a large extent, in place of the substantive verb, is well known to scholars. Pronominals serve naturally for emphasis and for grouping. It was thus that they became the signs of predication as affixed to the Indo-European verb.

As to the significance of the so-called copula, we have in it something more than a mere link, a simple *nexus*, joining attribute to subject. For, in what grammarians call "the attributive relation," there is this connection, but no copula. A copula involves the affirmation or assertion which is essential to predication. Affirmation involves and expresses belief in objective actuality, and thus in a sense signifies this actuality, that is, the existence (or non-existence, as the case may be) of that which is predicated. In this view, the term "verb of existence" seems not inappropriate, but not to be understood as at all implying that the use as copula was derived from the signification of simple existence. The real copula is, however, the personal termination in the finite verb, and in every finite verb; for the inherence-relation indicated by the participle or infinitive, even of the verb 'to be,' involves no affirmation, and where this is not there is no copula.

The paper concluded with remarking upon the importance that grammar, as a disciplinary study, should be kept free from fictions and false theories, and of observing a due discrimination between logical and grammatical relations.



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Professor Tracy Peck, of Cornell University, Ithaca, N. Y., read a paper on "Certain Differences among the Ancient Romans in the Pronunciation of their Language."

The object of the paper was to show that at the period of the greatest purity of the Latin language there were recognized and definable differences in the pronunciation of educated Romans.

Attention was first called to some of the positive characteristics of the standard utterance—that of a cultivated circle at the capital. The quality of this pronunciation was further illustrated by its contrast with some features in the speech of several classes.

Several peculiarities of the boorish pronunciation—the *vox rustica*—were noticed, such as the excessive use of the aspirate, the assimilation or complete identification of different vowel-sounds, the indifference to hiatus and general disregard of euphonic and rhythmical effects, the over-treatment of words as enclitics and proclitics, the heedless enunciation of final letters and syllables, a tendency to give the language an accentual character, lallation, and a flat, drawling utterance.

There was a large class that tenaciously clung to the archaic speech. The old-fashioned pronunciation was in many respects like the rustic. Among its other peculiarities were mentioned the frequent discarding of final letters and syllables, the syncopation of consonants, the treatment of the first vowel in reduplicated perfects, and the production of emphasis by aspirating vowels and consonants.

The presence of many Greeks and the great use of the Greek language in Rome must have exercised an important influence on the Latin language, and it was shown that native critics warned very earnestly against excessive modification and adulteration of their pronunciation from this source.

Educated Romans differed in regard to many points in the quantitative feature of their language, and pronounced accordingly. Reference was made to discussions among the native grammarians about the quantity of many vowels, and the usage of the most painstaking folk was cited as proof that in this particular there was great practical latitude and variety.

Those who spoke in a sing-song tone were next characterized and shown to have formed a numerous body, and then some of the exactions were given of the pedants, or sticklers for absolute correctness in pronunciation.

After this sketch of several classes of speakers, mention was made of some individual differences and defects in pronunciation, as those of Cicero, Julius Caesar, Augustus, and Maecenas.

In conclusion, the speaker urged that, as the best native speakers of Latin differed among themselves in theory and practice, but labored harmoniously and to the end to come nearer to the ideal pronunciation, so there is no ground for discouragement or for detraction if those who would restore the ancient pronunciation still differ in many points, and perhaps in all points fall below the true standard.

Professor W. D. Whitney, of Yale College, New Haven, Conn., next read a paper on "The Question of Indo-European and Semitic Relationship."

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The author explained that he had received a public letter* from Professor von Raumer, of Erlangen, taking exception to the views expressed by him adverse to the relationship of Semitic and Indo-European language, and setting forth in reply a succinct statement and array of evidence in favor of that relationship. He desired to make a brief rejoinder before the Association and in its Transactions, rather than in the form of a separate publication. He had really nothing of importance to say which he had not before said. The question involved is the general one of the value of individual linguistic correspondences as evidences of relationship, a question already more than once, in different forms, brought up and discussed before the Association. That there exist a certain considerable number of such correspondences between these two families, is not to be denied. In the old time, no one would have thought of doubting their competence as proof. But the progress of linguistic science has given a wholly new aspect to such matters, showing that every case of apparent correspondence must be judged in the light of the entire relation of the languages compared. As between languages proved to belong to different families, the highest degree of improbability belongs to resemblances which between nearly related tongues would pass without question; and especially when the families are so discordant in their whole structure as are these two. Semitic language is more unique and anomalous than any other, so far as known, in existence. If it really started from the same beginnings as the Indo-European, it has undergone such a peculiar and disguising development as could hardly by any possibility allow traces of the fact to remain. The main substance of von Raumer's argument consists in a series of thirty verbal correspondences, between weak or geminate Hebrew roots and Indo-European words; and he claims respecting them that, both in number and in degree, they are altogether beyond what could be explained as the result of chance, or of anything but genetic connection. This claim is by no means to be admitted. On the contrary, the resemblances wear the ordinary aspect of those on which, in all ages, false etymologies have been wont to be founded. Some of them would not pass for good even between two Indo-European branches. Many of them are (if we are allowed to cast out, as desired, the third Semitic radical) of that kind which are called "too good to be true"—that is (like ὅλος and *whole*), beyond the measure of what is to be expected except between closely related dialects. Such are to be found, as the obvious result of accident, between any two languages in the world, in numbers of which no investigation has yet determined the possible limits.

In one point, and only one of consequence, Professor von Raumer misunderstands the views which he opposes: he regards it, namely, as held by his opponent that the problem of Semitic structure must be completely solved within the circle of the Semitic languages themselves, before they can be compared with other languages. That is an error. Here, as elsewhere, the sphere of legitimate and fruitful comparison is unlimited, provided the legitimate method be followed. First, a thorough and exhaustive comparative investigation of the Semitic dialects among themselves must be made; and, till this is done, far better than at present, all outside comparison is premature. Then the other families may be brought in, with a distinct aim at solving together the question of their relationship to the Semitic and the problem of Semitic structure: the two being so connected

**Sendschreiben an Herrn Professor Whitney über die Urverwandtschaft der semitischen und indogermanischen Sprachen, von Rudolf von Raumer.* Frankfurt, 1876. 8vo. pp. 20.

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that the former cannot be solved without the latter. Apart from this method, no solid and abiding result, it is believed, can possibly be reached; and all the surface comparisons and discussions now carried on are unscientific, and a lamentable waste of time and energy.

Professor Whitney said in conclusion that he was far from maintaining that Semitic and Indo-European are not and cannot be related; no one has the right to say that; he only asserts that their relationship is by no means demonstrated, and that the time for demonstrating it has not yet come: as a matter of private opinion, he has no confidence that it will ever arrive.

Dr. J. Hammond Trumbull, of Hartford, Conn., read the last paper of the afternoon, on "The Algonkin Verb."

Professor H. Steinthal, selecting as types of American speech the Mexican and the Greenlandish (Eskimo), decides that the former has nouns, but no true verbs, and that in the latter "the sentence is not founded on Subject and Predicate," but centres in the Object; "the Verb is without a copula and the Noun without subjective character" (*Charakteristik*, 218, 226). Dr. Friedrich Müller, of Vienna, in his *Der grammatische Bau der Algonkin-Sprachen* (1867) and *Allgemeine Ethnographie* (1873), though he concedes true verb-forms to the Mexican and Dakota languages, denies them to the Iroquois and the Algonkin. "The distinction between the predicative and the dependent relation is," he says, "utterly unknown to the Algonkin languages." They do not distinguish the noun from the verb, and, "from the etymological point of view, they know *only a noun*, which, when it is furnished with *possessive* suffixes corresponds to our expression by a verb." Thus, the Algonkin verb becomes a 'nomen actionis,' with or without possessive affixes: e. g. Alg. *pimose* ('he walks') resolves itself into 'his walking'; *ni-sakih-ig-o* ('I am loved') is 'my being-loved by him—somebody, somewhere.' In his latest work (*Grundriss d. Sprachwissenschaft*, I. Abth. 1, s. 15) Dr. Müller again names the Algonkin among languages 'welche gar kein Verbum besitzen.'

Very different is the conclusion at which Bishop Baraga arrived after twenty years' study of the Chippeway, an Algonkin dialect. "This," he wrote, "is a language of verbs. . . All depends on the verb." Father Lacombe, after passing as many years among the Crees, and compiling a copious dictionary of their language, says: "It is nothing but a language of verbs:" and Mr. Joseph Howse, in his Cree Grammar, pronounces the Algonkin verb, "as respects its nature and essence, strictly analogous to the part of speech in European languages, bearing the same name, viz. as *predicating* being, or manner of being, or acting." In fact, no one who has learned to speak or write an Algonkin language, without having first determined its 'inner form' by the light of ethno-psychology, seems to have suspected that the verb on which "all depends," and from which every noun is formed or derived, is really *no* verb, but itself only a noun in disguise, that its pronominal affixes are merely possessive, not subjective, or that "the idea of time and place is altogether absent" from it.

It was proposed, in this paper, to examine the grounds on which the genuineness of verb-forms in Algonkin languages has been denied. Before entering on the examination some peculiarities of grammatical structure of these languages were noticed; particularly, the inflection of nouns, the formation of the possessive,

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'obviative,' 'sur-obviative,' and locative cases, the law of euphony which regulates the vocal connection of the formative with the principal root; and the "vowel change" which takes place in the root, in the subjunctive mood, in participles and gerundives, when the action of the verb is (as Mr. Howse expresses it,) "generalized, or rendered indefinite in respect of time," or "implies custom or habit in the subject." This change—which Eliot (*Indian Grammar*, 26) described as "a *flattening* of the first vowel of the root, in the suppositive mood"—is one of the most strongly marked peculiarities of Algonkin grammar. "It occupies us in all our conjugations," says Baraga, who devotes more than twenty pages of his Otchipwé Grammar to its illustration. No view of Algonkin languages in which this feature is unnoticed can be even approximately complete. Yet it seems to have escaped the observation of Professor Steinthal and Dr. Müller.

The relation of the Algonkin noun to the verb was next noticed. It was shown that names which must have been among the earliest formed of those now in the vocabulary, are formed directly from verbs or on predicative roots. The names for 'river,' 'rain,' 'path,' 'father,' are verbals. The 'nomen actionis' is formed from the verb in the indicative, but is distinguished, by its affix, from the conjugation (predicative) form; while the 'nomen agentis' is formed from the subjunctive or indefinite mood, with a change of the vowel of the root: e. g. *pimosé* 'he walks' (literally, 'passes'), *pimose-win* 'walking,' a walk, but *pEmoset* 'one who walks,' a walker.

From a discussion of the forms of primary verbs, active-intransitive and subjective, it was argued, that the characteristic function of the verb is as unmistakably indicated in Algonkin as in Indo-European speech, by special forms devoted to the expression of the predicative relation; that verbs are formed on predicative roots, and not from nouns by the help of affixes denoting possession; that the formative elements, whatever may have been their original character, have lost all traces of independence; that the distinction between the predicative and the dependent relation is well marked, in every Algonkin dialect; that the verbal noun with a possessive pronoun is never confounded with the predicative verb; and that the relation of the subject to the activity is as clearly apprehended, and as formally expressed, as that of the subject to the object.

The Association thereupon stood adjourned till to-morrow morning.

## Third Day—Thursday, July 20.

### Morning Session.

The Secretary announced the election of new members:

Professor O. M. Fernald, Williams College, Williamstown, Mass.; Mr. S. P. Andrews, 75 West Fifty-fourth Street, New York.

The Rev. J. Colver Wightman, of Taunton, Mass., read the first paper of the session, on "The Notation of the Palatal Sibilant in English."

Written language admits of two dissimilar systems of representation. One is ideographic, the other phonographic. The former employs as the vehicle of

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communication *thought-symbols;* the latter *voice-signs.* The one attempts to make the mental movement legible, the other to make articulate speech visible. Pictorial writing represents the former; alphabetic writing, the latter. The essential superiority of the alphabetic system is universally conceded. Its adoption by all the most enlightened nations is a verdict from which there is no appeal. Hence, the essential feature which pervades and characterizes the system ought to be tenaciously maintained.

All alphabetic writing is theoretically phonographic. The ideal perfection of the system demands as many symbols of uniform application as there are elementary sounds in the language. The English language, notwithstanding the obvious deficiencies of its alphabet and the consequent irregularities of its orthography, nevertheless belongs to the cosmopolitan sisterhood of tongues whose system of writing is alphabetic. Its original character, though seriously impaired, is not altogether lost. The language has outgrown its old alphabetic dress. The result is a loss of symmetry, which threatens loss of original generic character. The omission of the Phoenician letter *shin* from the Roman alphabet was a misfortune. The lack of it mars the orthography of many languages; English suffers most. English articulation recognizes four sibilants: two are surd and two sonant. The most common symbol of the tongue-tip surd sibilant is *s*; of its sonant mate, *z*. To denote the palatal or tongue-face surd sibilant, the digraph *sh* is often used; to denote its sonant mate, *zh* is used in dictionaries. The deformity caused in English orthography by the lack of a symbol to represent the surd palatal sibilant illustrates our imperative need of an enlarged alphabet.

This sound occurs in about three thousand four hundred and forty-seven words which are in common use. This estimate includes only such words as are inserted in Worcester's *Comprehensive Dictionary.* In some of them the same sound occurs twice. It is represented by twenty-two different letters and combinations of letters, as follow: in

| | | | | | *No. Words.* | | | | | | *No. Words.* |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Class | 1, by | *c*, | as in | 'associate,'..... | 52 | Class | 2, by | *ce*, | as in | 'ocean,'..... | 60 |
| " | 3, " | *ch*, | " | 'machine,'..... | 56 | " | 4, " | *che*, | " | 'truncheon,'.... | 6 |
| " | 5, " | *chi*, | " | 'falchion,'..... | 3 | " | 6, " | *chs*, | " | 'fuchsia,'..... | 1 |
| " | 7, " | *ci*, | " | 'ancient,'..... | 203 | " | 8, " | *cy*, | " | 'halcyon,'..... | 1 |
| " | 9, " | *s*, | " | 'nauseate,'..... | 34 | " | 10, " | *sc*, | " | 'prescience,'... | 5 |
| " | 11, " | *sch*, | " | 'schist,'..... | 4 | " | 12, " | *sci*, | " | 'conscience,'... | 11 |
| " | 13, " | *se*, | " | 'nauseous,'..... | 3 | " | 14, " | *sh*, | " | 'ship,'..... | 936 |
| " | 15, " | *shi*, | " | 'cushion,'..... | 2 | " | 16, " | *si*, | " | 'tension,'..... | 73 |
| " | 17, " | *ss*, | " | 'fissure,'..... | 13 | " | 18, " | *ssi*, | " | 'passion,'..... | 82 |
| " | 19, " | *t*, | " | 'negotiate,'..... | 40 | " | 20, " | *ti*, | " | 'action,'..... | 1836 |
| " | 21, " | *x*, | " | 'sexual,'..... | 5 | " | 22, " | *xi*, | " | 'fluxion,'..... | 21 |

The figures after the sample words represent the number of words in the several classes. Two additional facts need to be noted in order to show how far the present English orthography has wandered from the primary law on which alphabetic writing is founded. The first is, that all the above letters and combinations of letters are besides overworked in representing other sounds. The second fact to be kept in mind is, that lexicographers, in order to indicate the pronunciation of this class of words, usually spell them with other letters, because the historic orthography is beyond the help of diacritic marks. This deformity in the method of writing English arises from its scant supply of voice-signs.

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The language sorely needs an enlarged alphabetic wardrobe. In a cosmopolitan aspect the present dress of English speech is as ridiculous as we should appear in the costume of our ancestors.

If we compare the heterogeneous methods of writing this sibilant in English with the modes of writing analogous sounds in other tongues, the deficiency of our alphabetic resources becomes equally manifest. In the earliest attempts to indicate sound by symbols, it is not probable that the sibilants were distinguished from each other in representation. In the ancient Hebrew alphabet there is no character for *zayin* or *samekh*. But in the time of the Hebrew judges there was a palpable distinction between שִׁבֹּלֶת and סִבֹּלֶת, which is registered in the national literature by different characters. In Syriac, *semkath* always takes the place of the Hebrew *sin* in cognate words; and the Chaldaic generally uses *samekh*. In Arabic and in those languages which use Arabic letters, as in Turkish, modern Persian, Afghan Vîrdû, the two surd sibilants are distinguished from each other by diacritic points. In Sanskrit, three sibilants are represented by appropriate characters. The laws of euphonic change prove that each represents an elementary surd sound. In old Bactrian, not only are two surd sibilants represented by characters, but their sonant mates also, which are wanting in Sanskrit, have their symbols. There is, moreover, an Avestan character which Spiegel calls *überzählig*, or "supernumerary." The modern languages of India which use some modification of the Devanâgarî characters, as Assamî and Hindî, follow the custom of ancient Sanskrit. Gużurâti, the mercantile language of western India, however, recognizes but two sibilants which are expressed by separate symbols.

The Tamil, which belongs to the agglutinative class of languages, and is spoken by about fifteen millions of Dravidian ancestry, recognizes but one sibilant; but the Telûgû, the Canarese, the Malayâlam, and the Tûlû imitate Sanskrit usage and have symbols for three sibilants. The notation of Barmîs (or Burmese) is strictly phonetic, but the symbols for the surd palatal sibilant are digraphic.

In ancient Latin, the palatal sibilant was not recognized and hence had no symbol. In Spanish, the process of assibilation has never created a demand for another sibilant character to denote it. In Italian, the old Latin *s* before soft vowels became palatal, and the digraph *sc* is used to express this sound. In Portuguese, this sound is represented by *x*; in Wallachian, also, by a special character. The old Latin *c* (hard) became sibilant in French and Provençal and the digraph *ch* was appropriated to represent it.

Neither ancient nor modern Greek contains this sound. Fortunately, a character to represent it was borrowed by the inventor of the Glagolitza for the use of the people then inhabiting Illyria, Dalmatia, and Bulgaria. Thence it was incorporated into the Kyrilitza, the Russian alphabet, and into the alphabets of all those nations which are affiliated with the Greek Church.

There is no reason to believe that the sound occurred in either ancient Gothic or early Anglo-Saxon. In German it is frequent, and its proper symbol is *sch*, which is never ambiguous. In classic Welch and Danish the sound does not occur. The Irish tongue is singularly deficient in alphabetic symbols. The language may be said to be clothed in worn-out shreds of letters. Two surd sibilants occur and they divide one letter between them. In Swedish the surd palatal sibilant is indicated by *stj*, *sj*, and *skj* in all positions, and by *sk* before soft vowels: as, *skjorta* 'a shirt,' *skepp* 'a ship.' This is the largest number of

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symbols I have noticed in any European language to denote a simple sound; but there is here no ambiguity; the notation is simple in comparison with the protean variations of English writing. A simple symbol to denote this sound in English is extremely desirable. Its use would save in the aggregate a vast amount of mechanical labor in writing. It would simplify orthography. It would tend to unify pronunciation. It would diminish the labor of teachers and pupils and at the same time increase knowledge. It would reduce the cost of printing at least one per cent., saving the cost of one volume in every one hundred, and the cost of one entire edition of a daily paper in every one hundred days. Accuracy of alphabetic notation measures the progress of civilization. The recovery of a symbol equivalent in value to that which was lost from the Phoenician alphabet would wipe away the deserved reproach of English and American scholars, and register the advent of a new era.

Accuracy in the use of other instruments portends the possibility of improvement in linguistic implements. But it must not be forgotten that great principles of acknowledged worth often mature but slowly.

The restoration of the old long *s* to our alphabet to represent this sound would answer the necessities of English speech. But most of the Romanic languages need another letter for the same purpose, though not so imperatively as English. Perhaps therefore ç would be preferable to *ſ*. The French now employ *ch*, the Italians *sc*, and the Germans *sch*. There is room in all for economical improvement. The letter *c* either alone or in combination stands in a majority of the classes of English words which contain this sound. If ç were adopted in English the above sample words would take the following forms: (1) 'assoçiate,' (2) 'oçan,' (3) 'maçine,' (4) 'trunçon,' (5) 'falçon,' etc.

Possibly a more critical analysis of sound and improved articulation may find profitable use in the future for both these symbols in English.

In that case the *c* with cedilla would be the proper symbol for the cerebral or tongue-face sibilant, and the old long *s*, modified a little in its printed form to distinguish it from *ſ*, would denote with admirable precision the more strictly palatal sound: as in 'seſſon,' 'miſſon,' 'equaſon.'

Professor D. S. Martin, of Rutgers Female College, New York, read a paper on "The Relation of Philological Accuracy to Scientific Nomenclature."

The paper began by pointing out the fact that the language of science, in its nomenclature alone, is already a great department of human speech, deserving of much attention. The field, too, is widening from day to day; and this elaborate system of naming must go on until it has comprehended every living organism of the lands and the seas, and all that have left their remains in the rocks of every preceding age. The scientific requirements in the formation of these thousands of names are two: faithfulness and accuracy in descriptive character. But has not philology also a claim to be heard? Must not some corresponding regard be paid to linguistic accuracy? This question is a grave one, and should receive attention from students both of language and of science. There are tendencies at work, which, if allowed to operate unchecked, will ruin the character of scientific nomenclature.

The author then proceeded to sketch the general principles of nomenclature,

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explaining the theoretical mode of forming the "monomial" names of chemistry and mineralogy, and the "binomial" names of natural history, illustrating the method by numerous examples. The fact was then pointed out, that no modern and spoken language could serve the purposes of science, for two great reasons, viz.: that the scientific names demand a fixity and precision in the use of words, which cannot be obtained in the changing usages of a living tongue; and also, that the names need to be cosmopolitan, and, like science and nature, to know no bounds of nation or race. By common consent, therefore, the great classical languages have been chosen as the basis of nomenclature.

Turning from the ideal system, and the partial modifications imposed upon it by necessity and by legitimate usage, there remain to be considered, as the particular point in view, the improper and perverted applications that have intruded themselves into the language of science.

These may be classed under three main heads, viz.:—

I. Hybrid and barbarous compounds.

II. Awkward and ambiguous compounds.

III. Names regularly formed, but incorrectly spelled, so as to obscure the real sense or derivation.

I. The consideration of hybrid and barbarous compounds resolves itself at once into two parts: (A) as to *voces hybridae* between Latin and Greek; and (B) as to compounding either of these with other languages.

A. Words formed partly of Latin and partly of Greek roots should be ruled out as inadmissible. In some rare instances, such words may be apt and even euphonious; but, if once allowed, there is no limit to their introduction, and science would ere long be overrun with them. Several cases were referred to, among recently described genera of fossil quadrupeds from the Tertiary beds of Wyoming Territory. In these instances, the usual skill and judgment of the eminent and accomplished describer had been overborne by the immense amount of material requiring to be treated at his hands. In this *embarras de richesse*, most of the designations were singularly apt and accurate; but exception must be taken to *Trogosus*, *Palaeosyops*, and *Microsus*, as hybrid names* requiring modification.

B. In respect to the union of classical roots with those of other languages, it is perhaps unwise to lay down a rigid rule; though all such names are hazardous, and rarely to be allowed. Among admissible instances of this kind, were cited the genera *Sivatherium* and *Bramatherium* for great quadrupeds of the fossil fauna of India, and the genus *Indigofera* among plants, where the familiarity of the name 'indigo' may render this word legitimate for the plant that produces it.

But, on the other hand, there is a tendency to mingle classical roots with all sorts of modern words in a manner simply barbarous. Such an example is found in *Agassizocrinus*, a most dubious honor to the great naturalist, and only fit to be banished from our lists. But the worst outrage on propriety has lately appeared in one of our geological reports, in naming a fossil shell of the genus *Spirifer*, *Spirifer Rocky-montani!!* If such barbarisms as this are to be tolerated, the great vocabulary of science will soon become a hideous jargon, no more entitled to respect than "pigeon English," or the trade-language of the Chinook Indians.

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**Sus*, with τρώγων, παλαιός-ὤψ, and μικρός.



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Nor can such monstrosities be prevented, save by so emphatic a protest on the part of scholars, that their retention shall be made and felt to be disgraceful. A sentiment must be developed among scientists that shall consign such names to just oblivion, and forfeit, in every instance, the eagerly-sought claim of priority. Thus only can care be enforced, and ignorance rebuked.*

Proper names from other than classical languages cannot with reason be objected to, for either genera or species. They are very largely employed; and so long as they are not united with Latin or Greek roots in the same word, they serve a convenient purpose with no impropriety. Thus when Dr. Harris named species of New England butterflies, of the genus *Hesperia*, after celebrated chiefs of the Massachusetts Indians—as *Hesperia Sassacus*, *H. Metacomet*, etc.—or when native names are Latinized as designations for genera or species of plants—as *Vanilla* and *Cocos*, *Cedrus Deodara* and *Mirabilis Jalapa*—no serious objection can arise.

II. The second main head, of awkward and ambiguous names, is of less importance and frequency. Two or three examples may serve to illustrate this evil, which can generally be remedied with little difficulty, by the exercise of a moderate amount of taste or skill. One case may be cited from chemistry, where the prefix *per* is used before 'iodic acid,' the result being apparently 'periodic acid'! Here all that is requisite is a hyphen. The other instances are both genera of echinoderms, one the absurd and misleading name *Eugaster*, from the attempted union of εὖγε (exclamation) and ἀστήρ; the other from δίς (numeral) and ἀστήρ, in which *Disaster* was the unavoidable result! To mend matters, however, some unwise counsellor altered the *i* to *y*, and the changeling appeared as *Dysaster*, with its etymology apparently quite remote. Either of these names could easily be modified into a respectable form, as any Greek scholar will readily see.

III. The third class is that of names neither barbarous nor defective in construction, but spelled in a manner which obscures their true origin and meaning. The difficulty here arises mainly from two sources: (1) a mistaken idea of simplifying the words and abridging them; and (2) especially from the fallacious "English pronunciation" of Latin and Greek, whereby our vowels are made to do duty for sounds that are wholly different in the original.

The most marked instances of this defective spelling are found in the representation of classical diphthongs by English long vowels. Thus in geology, the adjective δεινός enters into several generic and family names, as *Deinotherium*, *Deinosauria*, etc.; and these words, in many good works, are spelled with our long *i*, which never had that force in the original language, and should not be used to represent it. A like case is that of the genus of fossil reptiles termed *Liodon* (λεῖος-ὀδούς), and also the periods named *Miocene* and *Pliocene;* which

* Professor Haldeman remarked, at the close of the paper, upon the extraordinary lawlessness that some naturalists had indulged in as to forming names. He referred especially to the fact that the so-called "tom-cod" had been actually described as *Gadus tomcodus*—a name which some had supposed to be of Indian origin. Perhaps a yet more extreme case, if possible, was that of Mr. Say's genus of beetles, *Oblesus*—a name that arose from the fact that, when the first specimen of the new genus was brought in to him, Mr. Say, in his enthusiasm, greeted it with the exclamation, "Oh, bless us"!!

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are apt to be mispronounced 'Meeocene,' etc., by students who come to study geology after being trained in a better system of classical pronunciation.

It is a matter of surprise and regret that one of the ablest and most cultured of American scientists should have made the printed assertion that the *αι* of the Greeks always becomes *e* in English! This has occurred in a few thoroughly naturalized words, as 'Egypt,' 'ether,' etc.; but the working of the rule thus stated can best be judged by its application a few pages away in the same work of the same author. The great division comprising the Tertiary and recent rocks, is known as the *Caenozoic* Age (*καινός-ζωή*); but in this volume, according to the above dictum, it is spelled *Cenozoic*, totally obscuring the whole derivation, and referring the word apparently to *κενός*! One such example is surely enough: "*ex pede Herculem.*"

Various other points might be dwelt upon, but in such a paper only the most prominent ones could be even referred to, and the discussion must be of necessity very defective. The subject has received very little consideration, while it certainly claims a great deal. Professor Dana, in his "Mineralogy" (1868), lays down an excellent series of rules for nomenclature in that department, and justly rejects all hybrid and blundering names from the claim of priority. Only let such principles be extended into the other fields of science, and let men of culture unite to uphold and enforce them, and we may hope for better things. Meanwhile, it may be well that less should be said about the uselessness of "dead languages" in comparison with science, and that those who desire permanent fame in the latter, should inform themselves a little more in relation to the former.

Professor W. D. Whitney, of Yale College, New Haven, Conn., Chairman of the Committee appointed in 1875 to take into consideration the whole matter of the Reform of English Spelling, presented the Report of the Committee as follows:

1. The true and sole office of alphabetic writing is, faithfully and intelligibly to represent spoken speech. So-called "historical" orthography is only a concession to the weakness of prejudice.

2. The ideal of an alphabet is that every sound should have its own unvarying sign, and every sign its own unvarying sound.

3. An alphabet intended for use by a vast community need not attempt an exhaustive analysis of the elements of utterance, and a representation of the nicest varieties of articulation; it may well leave room for the unavoidable play of individual and local pronunciation.

4. An ideal alphabet would seek to adopt for its characters forms which should suggest the sounds signified, and of which the resemblances should in some measure represent the similarities of the sounds. But for general practical use, there is no advantage in a system which aims to depict in detail the physical processes of utterance.

5. No language has ever had, or is likely to have, a perfect alphabet; and, in changing and amending the mode of writing of a language already long written, regard must necessarily be had to what is practically possible quite as much as to what is inherently desirable.

6. To prepare the way for such a change, the first step is to break down, by the combined influence of enlightened scholars and of practical educators, the

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immense and stubborn prejudice which regards the established modes of spelling almost as constituting *the* language, as having a sacred character, as in themselves preferable to others. All agitation and all definite proposals of reform are to be welcomed so far as they work in this direction.

7. An altered orthography will be unavoidably offensive to those who are first called upon to use it; but any sensible and consistent new system will rapidly win the hearty preference of the mass of writers.

8. The Roman alphabet is so widely and firmly established in use among the leading civilized nations that it cannot be displaced: in adapting it to improved use for English, the efforts of scholars should be directed toward its use with uniformity and in conformity with other nations.

On motion, the report of the Committee was accepted.

At the request of the Executive Committee, Mr. E. Jones, of Liverpool, England, addressed the Association on the same subject.

On motion, it was

*Resolved*, That the Committee on the Reform of English Spelling be continued for one year, with Professor F. A. March as Chairman.

Professor W. C. Sawyer, of Lawrence University, Appleton, Wis., read a paper on "The Alphabet, as a Means to an End."

The paper discussed the origin, character, and importance of the alphabet, and placed its importance very high on account of its effect upon literature and education, but ranked its character very low on account of, first, its being inadequate to represent the sounds of several languages, and the consequent difficulty of teaching the orthoepy of foreign languages; second, its inadequacy to the representation of all the English sounds; third, its consequent but excessive multiplication of the uses of the same letter, completely unsettling the powers of the alphabet; fourth, the great variety of ways of expressing the same sound, amounting in one case to twenty-two; and fifth, its entire want of system as well as significance. The alphabet might be creditable to Shem, but it does not answer our purposes, and it is a disgrace to our civilization to continue our patronage of it. It is the gift of barbarism to civilization, and in adapting it to our wants we have made it ten-fold more barbarous than before.

Mr. James W. Shearer, of Liberty Corner, N. J., read the last paper of the morning, on "Phonetic Reform."

Isaac Pitman's system of phonography, published first in 1837, showed the evident advantages of phonetic representation in printing our language. Since that time, the sentiment favorable to reform in English orthography has gradually made progress in both England and America. The phonotypic proposal of Mr. Pitman failed to meet with the public favor which was anticipated, not because of lack of beauty or perfection, but because the public mind was not yet prepared for it. More rapid progress has been made of late. Simultaneous action tending in this direction has been taken in different quarters by teachers, philanthropists, publishers, and philologists. The stereotyped objection, that spelling reform would seriously interfere with the philological study of our language, has been

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removed by the concurrent testimony to the contrary of leading philologists here and in England. Recently the name of Max Müller has been added to those of Whitney, March, Hadley, and Trumbull declaring such reform *desirable*, and the National Teachers' Association, as well as this body, is already seriously considering the question of phonetic reform.

Is such a reform *practicable?* A writer in "The Galaxy" of July declares that it is not *possible*, even if desirable and tolerable. He forgets that language is a growth, not governed by fixed laws like those of the Medes and Persians, but influenced in its growth and development by various causes. Public sentiment and usage are omnipotent in determining the fluctuations of a language, and whatever is generally adopted becomes part of a language. However *improbable* certain changes may seem, it assumes the appearance of arrogance to declare them *impossible*.

Nor must we fall into the opposite error that *authority*, however eminent, is omnipotent in language. It is one of the causes operating on its growth, and as such its influence must neither be overlooked nor overestimated. The great factor in spelling reform is *public sentiment*, and the true method of gaining it is that which moulds and guides this factor. Phonetic representation furnishes the key to the reform, and such a conformity of printed with spoken language as unites fully the advantages of both ear and eye in learning the language, is the end to be attained. The anomalies of our language are so great that any sudden change to a purely phonetic print would so materially change the appearance of the printed page as to seriously interfere with the ability to read the new print. A preparation is needed before such advance can be made. This preparation is the instilling of phonetic principles in such a way as to overcome prejudices which are deep-rooted and ofttimes more firmly fixed in proportion to the weakness or ignorance of the mind. To prepare the way for this reform, extremes are to be avoided, prejudices soothed, settled ideas recognized, and public sentiment satisfied by such an application of phonetics to the language, as teaching its principles without seriously interfering with the present appearance of words. Language appeals primarily to the ear, but since only five words in our language are spelled as they are pronounced, the eye is the organ now chiefly used in learning to read. Phonetic principles bring the ear to the aid of the eye, and make both prominent organs in instruction. By such an application of phonetics to the alphabet and to the language, its beauty and importance will soon be perceived, and the phonetic idea imbibed as a part of mental conviction will soon show itself in modes of thought, and *work out* in language itself the solution of the problem under the guidance of authority and associations such as this.

The difficulties in the way of this reform are partly theoretical and partly practical. The theoretical difficulties lie chiefly in the disagreement even among leading scholars as to the proper pronunciation of words. Most words have now a fixed orthography, whilst differences exist in their pronunciation in different sections. Should a purely phonetic print be at once introduced, there is great danger that these differences would soon lead to dialects differing more than those of the ancient Greek. The true plan seems to be to accept the fact that our language is anomalous, and that the assistance needed to guide to a correct pronunciation must be simple and phonetic without interfering with present orthography.

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The coöperation of the National Teachers' Association furnishes the field for the practical application of these principles in the school-room. Encourage all teachers to teach phonetically by the use of phonetic notation, or adaptation which preserves present orthography. Let this be supported by dictionaries and pronouncing-books printed in the same way. Thus in a generation or two prejudice will be overcome, and the public mind will learn to love phonetics, favor spelling reform, and under suitable guidance work out for itself the *minutiae* of the problem.

A recess was then taken till 3 o'clock.

### Afternoon Session.

Mr. T. C. Murray, Secretary *pro tempore*, announced the election of new members:

Mr. C. Osborne Ward, 486 Adelphi St., Brooklyn, N. Y.; Professor Howard Osgood, Rochester Theological Seminary, Rochester, N. Y.

The Auditors of the Treasurer's Report reported that they found it correct; and it was, on motion, accepted.

The Committee on the place and the time of the next meeting recommended that the next meeting be held at Baltimore, Md., on Tuesday, July 10th, 1877.

On motion, the report of the Committee was accepted, and the recommendation therein contained was adopted.

An invitation from the Mayor and Aldermen of Greenville, S. C., asking the Association to hold its next annual session at that place, having been referred to that Committee, the Committee recommended the passage of the following resolutions:

*Resolved*, That we appreciate highly the kindness and courtesy of the Mayor and Aldermen of the city of Greenville, S. C., in inviting the Association to hold its next annual meeting in that place, and regret that the distance of Greenville from the residences of the large majority of our members, presents a serious obstacle to our acceptance of the invitation.

*Resolved*, That we express to the Mayor and Aldermen of Greenville, S. C., our sincere thanks for their attention, and our gratification at the interest thus manifested in the progress of philological science.

On motion, the resolutions were adopted.

The Committee to nominate officers for the next year presented nominations as follow:

For *President*—Professor S. S. Haldeman (University of Pennsylvania), Chickies, Penn.

For *Vice-Presidents*—Professor B. L. Gildersleeve, Johns Hopkins University, Baltimore, Md.; Professor J. B. Sewall, Bowdoin College, Brunswick, Me.

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For *Secretary and Curator*—Professor Samuel Hart, Trinity College, Hartford, Conn.

For *Treasurer*—Mr. Charles J. Buckingham, Poughkeepsie, N. Y.

For additional members of the *Executive Committee*—

Professor Fisk P. Brewer, University of South Carolina, Columbia, S. C.

Professor Albert Harkness, Brown University, Providence, R. I.

Professor W. G. Richardson, Central University, Richmond, Ky.

Dr. J. Hammond Trumbull, Hartford, Conn.

Professor W. D. Whitney, Yale College, New Haven, Conn.

The report was accepted, and the persons therein named were declared elected to the offices to which they were respectively nominated.

Dr. J. B. Bittinger, of Sewickly, Pa., read the first paper of the afternoon, on "What Shakespeare Knew of Horsemanship—a New Reading of Macbeth I. 7."

"I have no spur
To prick the sides of my intent, but only
Vaulting ambition, which o'erleaps itself
And falls on the other."

This is the current text—it can hardly be called "the received." It has been amended from Hanmer's time to the present, each succeeding commentator or critic discrediting his predecessor's suggestions. The text as amended presents the following aspect:

"I have no spur
To prick the sides of my intent, but only
Vaulting ambition, which [oft] o'erleaps itself [its sell, its seat],
And falls on the other [side, horse, the rider, the earth, theory, the bank]."

These diverse and conflicting readings show the stress under which the commentators have lain. Of course it is the sense of the passage that is sought after. If Macbeth stopt with 'other' because he was done, or if he stopt because of the change of scene, what did he mean? or, what would he have said, if Lady Macbeth had not broken off his sentence? The amended text shows what, according to the critics, he would have said but for the interruption. However, accepting any one of the readings, the question still remains: What did Shakespeare mean?

In getting at this, the comments have been as diverse as the readings. All the commentators agree that the underlying figure is one of equestrianism; and, with the exception of Steevens, Elwin, and Staunton, all agree that the point, from which the figure is viewed, is that of a person in the act of mounting, and in his eagerness, overleaping the saddle. This is their first mistake and it is fatal.

What is the idea of the soliloquy? It is the conflict between will and desire, as it sways to and fro in the mind of the ambitious but irresolute Macbeth—ambitious to be king, but without the courage to enact his ambition;

"Letting 'I dare not' wait upon 'I would';"

courageous in thought, but a coward in act. Ambition made all seem easy, so long as he was under the spell of its first suggestion—nay, even practical, when

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smarting under the chastisement of his wife's tongue; but no sooner is he alone, or confronted with the deed, than he whispers: "If we should fail?" Irresolution was his distinguishing characteristic. It was the essence of his will, so that no sooner does his wife take off her hand than he falls from his resolution. "Infirm of purpose!" is her fierce and final verdict of his character.

Now by what aspect of the figure of equitation shall this vacillation be set before us? Is it by the image of one getting *on* his horse, as the commentators explain; or of one mounted, and urging his steed towards the goal? The former might symbolize a single act of mind, but the case demands a figure that shall set forth a double, nay, a complex and conflicting state of motives. Moreover a person mounting clearly would not need any spurs either as instruments of or incitements to mounting. But granted that spurs were used by knights for stirrups or stimulants, Macbeth could not have mourned the lack of a spur, seeing that, as it was, his "vaulting ambition" carried him beyond the saddle.

No; Shakespeare's hero is already mounted, and eager for the goal; but he and his steed are not of one mind, and, for want of a spur, cannot be brought into harmony of action. To set forth the conflict between Macbeth's ambition and his irresolution—whether this irresolution sprang from weakness, cowardice, or conscience—what so apt as the two-fold image of an eager horseman on a balking horse, the spurless rider leaning forward (vaulting) on his laggard steed? This was the poet's metaphor. The age of chivalry was not yet past. Horsemanship was as common in Old England as husbandry, and Shakespeare knew it in all its details. His plays are filled with feats of equestrianism, and this figure, besides being most apposite, was right at hand.

> "Pity like a new-born babe
> Striding the blast, or heaven's cherubin horsed
> Upon the sightless couriers of the air,"

had just ridden across his pictured page, and in continuation of and consonance with this imagery, Macbeth sighs forth his repining:

> "I have no spur
> To prick the sides of my intent, but only
> Vaulting ambition."

Manifestly we have here a figure of equitation. 'Intent' is the steed, 'Ambition' is the rider. The rider has no spur to prick on his halting steed, while he himself is urged forward by strong desires. Rising in his stirrups, and [*Anglice*] bending over ('vaulting'), he overleaps himself.

The goal of Macbeth's 'intent' is the assassination; the goal of his 'ambition' is the throne. Macbeth must be a murderer before he can be a monarch. If the intent to murder halts, the desire to mount the throne will be futile. All this Macbeth knows and feels. He does not repine at any lack of ambition; that is in full force and action; it is o'erwrought: but over his purpose he mourns—that is infirm; over his courage—that needs "screwing to the sticking-place"; and so, like an eager rider on a sluggish steed, he o'erleaps himself, and '*falls on the withers*,' and so Shakespeare wrote. The consequence is not a catastrophe, it is even beneath the dignity of a failure—it is a *fiasco*, and this shade of thought is brought out by substituting 'withers' for 'other'. So long as he had not the courage to commit murder, "the hope wherein he dressed himself was drunk" and nothing but a maudlin ambition.

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This reading seems to me to meet all the demands of the passage. 'Withers' calls for no explanation, it explains itself. Whether copied by eye or ear, it was easy to mistake in sound or appearance 'other' for 'withers.' This reading dispenses with the many and conflicting readings suggested. It calls for no subsidiary adjustments of the text. It charges Shakespeare with no mixed, double, confused, or imperfect metaphors. It leaves his rhetoric and imagination unsuspect, brings the whole passage into harmony with itself, with the rest of the soliloquy, and with the character of Macbeth—too ambitious to be innocent in thought, too cowardly to be guilty in deed. His imagination sicklied o'er with the pale cast of conscience, he is vacillating in purpose, irresolute in action, and querulous in speech. Thus he spake to himself, and so would I read:

"I have no spur
To prick the sides of my intent, but only
Vaulting ambition, which o'erleaps itself
And falls on the withers."

Professor Fisk P. Brewer, of the University of South Carolina, Columbia, S. C., read a paper on "Section 262 of Demosthenes' *De Corona.*"

The author proposed the following translation for the passage under consideration:

"Having hired thyself to those actors, Simylos and Sokrates, nicknamed the 'heavy groaners,' thou didst exhibit as a third-part actor, [thus] collecting figs and grapes and olives, just as a fruit-peddler, from other people's fields, receiving from these [employers of thine] more than [was received] from the exhibitions in which ye exhibited at the peril of your lives."

Professor C. H. Toy, of the Southern Baptist Theological Seminary, Greenville, S. C., read a paper on "Hebrew Etymology."

The object of this paper is to state the present position of Hebrew verbal etymology, especially the etymology of firm stems. The first step is to reduce triliterals to biliterals. It is commonly assumed, but is not absolutely proved, that the reduplicated and concave stems may be brought to the biliteral form; the special considerations in their case are here left out of view, and the treatment of stems in general is discussed. The search for primitive stems must be guided by certain general principles: (1) the original Shemitic form and meaning of a stem must be determined; (2) the laws of letter-interchange between Hebrew and other Shemitic dialects and within the bounds of Hebrew itself must be fixed; (3) there must be wide and careful comparison of the triliteral stems, in connection with the two processes above mentioned.

Three methods of stem analysis have been attempted: (1) that based on the hypothesis of stems formed by composition of biliteral roots; this method is so cumbrous and violent, and its results so arbitrary, that it is not entitled to be called scientific; (2) the method of reduplication adopted by E. Meier, which also is quite arbitrary, and has been generally rejected by scholars; (3) the method by affixes, which has been resorted to by the more careful modern investigators. But even in these last attempts there is wide diversity and great



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uncertainty in the results reached; there is little more than feeling after right methods. Much has been accomplished in the way of discarding unscientific processes, but from the almost complete formal identity of the various Shemitic dialects, a science of Shemitic or Hebrew etymology cannot exist till much more thorough etymological investigations have been made in each of these dialects.

Professor F. A. March, of Lafayette College, Easton, Penn., read a paper entitled "Remarks on No. CCCXXVIII. of the Codex Diplomaticus Anglo-Saxonum."

This document is a letter to King Edward the elder, giving the history of a title to five hides of land at Fonthill. Its difficulty and importance to the student of Anglo-Saxon law are such that Professor Henry Adams, of Harvard University, who is printing some studies on the Anglo-Saxon laws, thought it worth while to obtain a photograph of the original manuscript, which is in the archives of the church at Canterbury. The photograph was shown to the Association, and an account of its history and contents was given, with some remarks on the handwriting, the spelling, and the grammatical forms. The words found in it, which do not appear in the dictionaries, were also mentioned and explained.

Professor Frederick Stengel, of the School of Mines of Columbia College, New York, read a paper on "The Historical Formation of the French Language."

When Greeks from Phocis founded Massilia about 600 B. C., the south coast of Gaul was occupied by Iberians west from the Rhone to the Pyrenees, by Ligurians in the present Piedmont, Provence, and Dauphiny, and near the mouth of the Rhone, from Montpellier to Marseilles, by a mixed Iberian-Ligurian population. We find Ligurians also in the north-west of Gaul near the river Liger (Loire). Béarn, at the extreme north-east of the Pyrenees, has preserved the original Iberian language. In the isle of Corsica there are Iberian descendants, and the Sard may boast to be of the old Ligurian race, though without any trace of its language.

The Celts invaded France from the north-east in the sixth century before Christ. At the end of the fourth century B. C., the Celtic was generally spoken in Gaul.

When Caesar finally subjugated Gaul, in 51 B. C., he found three distinct peoples: the Aquitanians in the south, mostly old Iberians; the Belgians in the north, a Celtic population mixed with Germans; and the Celts or Gauls in the centre. Latin was soon introduced and studied zealously. The Germanic invasion in the fifth century caused many changes in the already corrupted Latin, chiefly in the way of contraction, shortening, and aspirating. From this time to the tenth century, one Romanic language was spoken in France; Charlemagne recommended that the Gospel should be preached in that language. The people of the south called themselves Romans-Provençaux, while those of the north took the name of Romans Wallons; the language of the former was called the "langue d'oc," and the latter the "langue d'oïl" or "langue de oui." This "langue d' oïl" became so highly cultivated that it decided the common literary language of France, but not without taking

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numerous forms from the surrounding dialects—the Norman, the Picard, and the Burgundian.

In the sixteenth century, the French language was fixed by corrections and rules, and the spelling was decided by etymology. In 1660, the "Grammaire de Port Royal" struck the basis of agreement between writing and pronunciation. In 1694 appeared the first dictionary of the Academy. In its third edition (1740) thousands of parasitic letters were suppressed without fear of effacing the iretymological origin. Of eighteen thousand words, about five thousand were modified. In 1856 the profound linguist, Francisque Michel, said with bitter mockery: "By the progress the 'argot' makes in the mouth of the people and even among the fashionable classes, I do not despair that one day it will replace the French, which we are forgetting more and more."

Professor W. D. Whitney, of Yale College, New Haven, Conn., read a paper on "A Botanico-Philological Problem."

This was a discussion of the singular theory put forward by Mr. M. Müller (in the second volume of his Lectures on Language) as to the "names for fir, oak, and beech;" namely, that the (postulated) change of meaning in Latin *fāgus* and our *beech* from 'oak' (Greek φηγός), and that in Latin *quercus* from 'fir' (O. G. *foraha*), are somehow a consequence of the supplanting of firs by oaks, and of oaks by beeches, which the peat-bogs of Denmark show to have taken place in that part of Europe. In contravention of this theory, the writer sought to show, in the first place, that such a botanical change could not by any possibility lead to such a linguistic one; in the second place, that the kind and degree of accordance between the botanical and the linguistic facts was not what the theory demands, and that there is no practicable method of reconciling their discrepancies; and, in the third place, that in the regions where the changes of meaning had taken place there has been no succession of firs, oaks, and beeches, crowding out and supplanting one another: this last fact, if Mr. Müller continues inaccessible to ocular proof, is demonstrable to him as linguist by the presence in all the languages concerned of words for 'fir' and for 'oak' beside those for 'beech.' The theory proposed lacks even the tolerable semblance of a foundation. The doubtful and apologetic way in which it was originally put forward does not relieve its author of responsibility for it—especially, as he has repeated it in edition after edition of his work, though its fallacies were exposed a dozen years ago; and as he now, in the fourth volume of his *Chips*, returns to and insists on it, and makes a vain show of answering the objections with which it has been refuted.

Professor F. A. March, of Lafayette College, Easton, Penn., read a paper on "What Acts are to be Attributed to the 'Faculty of Speech,' and how far are they Instinctive?"

The acts of man in speech are not instinctive as being performed without movement of the will. Few acts of the lower orders of animals appear to be so. Birds build by instinct; but each straw is picked and placed after the manner of choice. Acts of man are called instinctive when they are prompted by an impulse prior to reasoning, definite and powerful enough to produce the acts in

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all normal beings under normal conditions, and to modify the acts of normal beings in abnormal conditions. Man is fitted up with controllable impulses which guide his acts when reason is inactive, as it is in most men most of the time.

A particular conception is taken to be the sign of a complex conception or of an external object, so as to take its place in thinking. These primary signs are necessary to reasoning. The impulse to make them does not depend on experience of their value, but is instinctive. It acts, however, on a free agent, who chooses his primary sign on grounds of reason, or the laws of association, or the suggestion of language.

Hardly any free act is earlier than the use of the natural cries as vocal gestures to obtain satisfaction of wants. A little later, at the jabber age, children are instinctively moved to exercise and train the organs of articulation. They exhibit a constitutional predisposition to the complicated muscular habits used in articulation, so that the use of the vocal organs is learned with ease and certainty like the use of the eye.

A connection is established between the conceptions of the sounds and the nerves of the speech organs such that we will to produce the sounds, and not to move the separate muscles. Persons who have no conceptions of sound can not speak in the same way as other men. They are guided by the feeling of the muscles, which is a very imperfect and often painful guide. Deaf-mutes are thus prevented from exercising the complete acts of the faculty of speech.

In acquiring speech from tradition the vocal sound is learned first, for the most part, and artificially associated with a primary sign, or the object for which it stands. As to originating words it is agreed that an instinctive connection is established between certain strong emotions and the vocal organs, producing laughter, groans, and the like; but it is usual to distinguish these from other states of mind, and especially to deny any connection between the intellect and the voice. It may however be said that conceptions of these emotions act instinctively on the vocal organs; and, further, that the distinction is based on no real separation: the intellect, the sensibilities, and the will, act together, especially in the young and unreflective mind. Perception of food, the joy of appetite, the desire of food, and the demand for it, are all mingled in such a mind, and the vocal gesture which expresses this state belongs to one as much as the other, and may be the root of a name for the food, or the desire, or the act. Comparatively unnoticed emotions naturally prompting utterance accompany many intellectual states.

The desire to communicate seems to be instinctive. Man hears himself talk, and is society to himself. He muses, and accompanies his silent thought with conceptions of words, or even audible utterances, musical or other, talking for himself.

When the faculty of speech has selected a primary sign of an object and a secondary vocal sign, and has so associated them that the sound goes with the thought without effort, and has established the muscular habits necessary to unreflective articulation of the sound, a word is born. Children often make and use words for a long time without any one else ever using them, or even understanding them; and so do philosophers.

No additional action of the faculty of speech is necessary to give the word standing as a member of a national language: only the same action repeated by a number of persons.

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Does the repetition of the same acts in the production of each of the words in a language account for the language? The laws of thought and voice lead to the arrangement of words into sentences, and into compounds. There is instinctive guidance of the faculty of speech, which produces harmonies and unities never invented or intended among the words; a guidance to be explained from the laws of reason, and from the relations of the objects and choices of the faculty of speech.

Dr. J. Hammond Trumbull, of Hartford, Conn., read a paper on "Names of the Sun and of Water, in some American Languages."

In the mythology of northern nations the Sun is the giver of light and warmth, the producer and vivifier: with the Moon are associated darkness, cold, and the destructive agencies of water. The Algonkin name for the sun is related to verbs signifying to warm, to ripen, to complete or perfect, to produce. The Indians of Canada, the Lake region, and the Atlantic seaboard, seldom had occasion to pray for rain. They believed—like the Chaldeans—that Water was "the producing mother," but the Water God appeared oftener as a destroyer than as a preserver of men. On the sandy plains of the southwest and within the tropics, the Giver of Rain was held in higher regard—as the giver and sustainer of life. His office and attributes were given to the eldest and greatest and most beneficent of the gods. Even when Sun-worship had become the established religion of the empire of the Incas, it was the water-born Viracocha, "world animating," the cloud gatherer, the possessor of the "thunder vase,"— and not the Sun—who was invoked as "lord and master of the whole world."

In some Central American languages we find resemblances or coincidences that cannot be regarded as accidental, between names of the Sun and Water. For example, in two dialects of Southern Costa Rica, the Bri-bri and Cabecar (in Dr. W. M. Gabb's vocabularies), we have:

| | | |
|---|---|---|
| Bri-bri, *di*, water, | *di-wo*, sun, | (*si-wo*, moon) |
| Cabecar, *di-kru*, | *di*, *di-wo*, | (*toru* " ) |

*Wo*, in composition, denotes a round or roundish mass, or lump; *di-wo* is 'round water' or 'lump of water.' The Chorotegan ("Dirian," Squier,) of Nicaragua has *nimbu* 'water' and *nimbuyumbu* 'sea,' *numbu* 'sun,' and *nimbumbi* "devil." In the Lenca (Honduras): *quash* and *uash* 'water,' *gasi*, *gashi*, 'sun.' In the Otomi, of Mexico, *dè*, *he*, 'water,' *hia-di* 'sun.' In the Huastecan, *ija* 'water,' *aquicha* (= *aqui-ija*?) 'sun.' Further north, similar coincidences are observed in some of the Pueblo dialects of Arizona and New Mexico: e. g. Acoma, *tsits*, *zitz*, 'water,' *ozutz* 'sun'; Isleta, *p'a* 'water,' *p'a-ida* 'moon,' but *tlor-ida* 'rain,' *tor-ida* 'sun.' And again in the Witchita (Tawaihash) of the Pawnee group: *kitche*, *kitsah*, 'water,' *kishaw* 'sun.'

Professor M. W. Humphreys, of Vanderbilt University, Nashville, Tenn., read the last paper of the session, on "Certain Influences of Accent in Latin Iambic Trimeters."

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On motion, it was

*Resolved*, That the thanks of the American Philological Association are hereby tendered to Chancellor Howard Crosby, D.D., LL.D., of the University of the City of New York, and to those families who have so hospitably entertained us during the session; to the proprietors of the Ashland House for their special reduction of prices; also to the Trustees of the University of the City of New York, and those of the Fourth Avenue Presbyterian Church, for the use of their buildings on the present occasion.

On motion, the Association adjourned.

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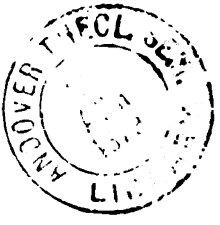


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# CONTENTS.

I. Notes on Certain Passages in the Phaedo and the Gorgias of Plato.
By Professor L. R. PACKARD. . . . . 5

II. On the Nominal Basis of the Hebrew Verb.
By Professor C. H. TOY. . . . . 18

III. On a Certain Apparently Pleonastic Use of ὡς.
By Professor F. D. ALLEN. . . . . 38

IV. On the Relation of Surd and Sonant.
By Professor W. D. WHITNEY. . . . . 41

V. On the Vocabularies of Children under Two Years of Age.
By Professor E. S. HOLDEN. . . . . 58

VI. On the Text and Interpretation of Certain Passages in the Agamemnon of Aeschylus.
By Professor W. W. GOODWIN. . . . . 69

VII. On the Single Case-Form in Italian.
By Professor AUSTIN STICKNEY. . . . . 87

VIII. On Willmanns' Theory of the Authorship of the Nibelungenlied.
By Professor FRANKLIN CARTER. . . . . 94

IX. On Herodotus's and Aeschylus's Accounts of the Battle of Salamis.
By Mr. ERNEST G. SIHLER. . . . . 109

X. On the Principle of Economy as a Phonetic Force.
By Professor W. D. WHITNEY. . . . . 123

XI. On the Kürenberg Hypothesis.
By Professor FRANKLIN CARTER. . . . . 134

XII. On Dissimilated Gemination.
By Professor F. A. MARCH. . . . . 145

PROCEEDINGS :—Ninth Annual Session, Baltimore, 1877.

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TRANSACTIONS

OF THE

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1877.

# I.—*Notes on certain Passages in the Phaedo and the Gorgias of Plato.*

BY LEWIS R. PACKARD,

HILLHOUSE PROFESSOR OF THE GREEK LANGUAGE AND LITERATURE IN YALE COLLEGE.

## PHAEDO.

61 C. All the editions within my reach, except Heindorf's, have an interrogation point at the word Εὐηνῷ. But the phrase contains no interrogative word, and there should be, therefore, if we do not use our exclamation point in a Greek text, a period or colon here. The sentence would then be translated "What a piece of advice this is, Sokrates, that you give to Euenus!" A similar case is found in 117 D: Οἷα, ἔφη, ποιεῖτε, ὦ θαυμάσιοι, where my editions have a period.

71 D. οὐκ ἐναντίον μὲν φὴς τῷ ζῆν τὸ τεθνάναι εἶναι; Ἔγωγε. Γίγνεσθαι δὲ ἐξ ἀλλήλων; Ναί. Ἐξ οὖν τοῦ ζῶντος τί τὸ γιγνόμενον; Τὸ τεθνηκός, ἔφη. Τί δέ, ἦ δ' ὅς, ἐκ τοῦ τεθνεῶτος; Ἀναγκαῖον, ἔφη, ὁμολογεῖν ὅτι τὸ ζῶν.

In this there is an instance, apparently, of the use of the neuter participle with the article in the sense of an abstract noun, in which sense the infinitive is frequent. The participle in this use is sometimes thought to be found almost solely in Thucydides, among prose writers. It certainly is more common with him. Böhme in his note on THUC. I. 36 cites two instances



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from Antiphon and one from Plato. In this passage we see it in *τοῦ ζῶντος* and in *τὸ τεθνηκός*, which seem to be substitutes for *τῷ ζῆν* and *τὸ τεθνάναι* of the first line quoted. In the next sentence after the quotation we have *τῶν τεθνεώτων, τὰ ζῶντα*, and *οἱ ζῶντες*, where the plural marks the change to the concrete sense. A still clearer example is in 72 B, where *τὸ ἀνεγείρεσθαι* occurs in correlation with *τοῦ καθεύδοντος*, which latter therefore plainly amounts to the same with *τοῦ καθεύδειν*. This is the case quoted by Böhme. Another case is to be found in Rep. 462 E. *ἑνὸς δὴ πάσχοντος τῶν πολιτῶν ὁτιοῦν, ἡ τοιαύτη πόλις φήσει ἑαυτῆς εἶναι τὸ πάσχον*, where the context shows that *τὸ πάσχον* does not mean "the suffering member," but "the fact of suffering."

78 A. *ζητεῖν δὲ χρὴ καὶ αὐτοὺς μετ' ἀλλήλων.*

It may be worth noting that *αὐτούς* here is in apposition with the object of *ζητεῖν*, so that the sense would be: "You must not only seek among others for such a man, but you must examine yourselves (*ὑμᾶς αὐτούς*) with one another's help to see if you have not the power." This depends upon the sense given to *τοῦτο ποιεῖν* in the next clause, *ἴσως γὰρ ἂν οὐδὲ ῥᾳδίως εὕροιτε μᾶλλον ὑμῶν δυναμένους τοῦτο ποιεῖν.* Here *τοῦτο ποιεῖν* cannot refer to searching, for it does not appear that one could not do that as well as another, and there is no suggestion in the passage of getting any one else to search for them. But *τοῦτο ποιεῖν* means *ἐπᾴδειν τῷ ἐν ὑμῖν παιδί*—and thus Sokrates suggests to them that the banishment of the fear of death is what no one else can do for them, but they must do it for themselves, as he had done it for himself, by the cultivation of their higher reason. Stallbaum seems to regard *αὐτούς* as in apposition with the subject of *ζητεῖν*, by speaking of the latter verb as meaning *investigare rem.* But he gives no reason for this. Jowett's translation agrees nearly with the view above suggested: "And you must not forget to seek for him among yourselves too; for he is nowhere more likely to be found."

79 D. *καὶ περὶ ἐκεῖνα.*

Ast proposed to read here *ὥσπερ* instead of *περί*. No edition I believe has ventured to follow him, yet the change would

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be a very great improvement, and it may be confirmed by a similar confusion in Rep. 510 B, where the text of several MSS. followed by Stallbaum is *ἄνευ ὧν περὶ ἐκεῖνο*—but the best MS. (Par. A) has *ἄνευ ὧνπερ ἐκεῖνο*, which the Zürich editors and Hermann adopt.

86 E. *ἢ ξυγχωρεῖν αὐτοῖς ἐάν τι δοκῶσι προσᾴδειν, ἐὰν δὲ μή, οὕτως ἤδη ὑπερδικεῖν τοῦ λόγου.*

The singularity of this phrase is not, as Wagner seems to think, the omission of *ἤ* before the second *ἐάν*, but the use of *δέ*, instead of *ἤ*, as correlative to the *ἤ* expressed. It may be appreciated by an illustration in English exactly like it, only in negative form. In Bunyan's "Pilgrim's Progress," Hopeful says, at sight of the pillar of salt that was once Lot's wife, "What a mercy that *neither* thou, *but* especially I am not made myself this example!"

89 C. *ἀλλὰ καὶ ἐμέ, ἔφη, τὸν Ἰόλεων παρακάλει, ἕως ἔτι φῶς ἐστίν.*

The commentators, so far as I see, all refer this last phrase, *ἕως ἔτι φῶς ἐστίν*, to the fact that Sokrates was to drink the poison at sunset, and understand it therefore as meaning "While my life lasts." But is not that an unnecessary forcing of special meaning upon a general phrase? Such a phrase, "So long as it is day," would of course be in common use, and would be applied to any day even without such a special event to mark its close. In this place it comes in as part of a playful reference to a supposed battle. Every sentence contains an allusion or metaphor. "Let us cut off our hair in mourning for the argument, if it is slain and we cannot rescue or revive it. Rather let us vow, like the Argives, never to cut it off until we vanquish the opposing argument of Kebes and Simmias." "But," says Phaedo, "two are too many for even Herakles, as the proverb says." "Call on me then," answers Sokrates, "as your Iolaus, as long as it is light." The figure of a battle and the references to traditional battles suggest clearly the familiar idea of fighting until it is dark. That occurs repeatedly in the Iliad, and notably in the prayer of Aias (Il. xvii. 646 f.) for light

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upon the conflict, even if they are doomed to death. Other cases where nearly this exact phrase occurs with reference to a contest are Xen. Cyr. 4. 2. 26; Plut. Philop. ch. 14. This general idea seems more in harmony with the context than a special reference to the fact that Sokrates must die at sunset. This opinion seems confirmed by comparison of 107 a, where Kebes says: *οὐκ οἶδα εἰς ὅντινά τις ἄλλον καιρὸν ἀναβάλλοιτο ἢ τὸν νῦν παρόντα, περὶ τῶν τοιούτων βουλόμενος ἤ τι εἰπεῖν ἢ ἀκοῦσαι.* Here Stallbaum says, *quippe quod tempus est extremum, quo de talibus rebus disserere nobis liceat*, and Wagner, "As discussion on this point cannot be deferred to any later opportunity than the last day of our life." One cannot help wondering at the words *de talibus rebus* and *on this point*, and asking on what other points discussion can be deferred beyond the last day of one's life. In this passage such absurdity is unavoidable, as soon as the phrase *τὸν νῦν παρόντα* is referred to that day as the last of Sokrates's life. The true view of the passage is that it is an entirely general remark. "Such a subject as this of the immortality of the soul is one which is too important to be deferred and ought to take precedence of any other. The best time for discussing it is always *the present* time, whenever that may be, until it is settled." Thus the clause *ἢ τὸν νῦν παρόντα* is simply the needed completion of *ἄλλον*, as may be seen by changing the order in translating or paraphrasing. "I have nothing further to say, but if Simmias here or any other person present has, he had better say it, for I hardly know to what time other than the present moment one should defer discussion on such matters as these." There is no time like the present, in other words. If the sentence referred to Sokrates's death we should have, instead of *περὶ τῶν τοιούτων*, some phrase referring to him, as for instance *σὺν τούτῳ τῷ φίλῳ*. Then it would all hold together, but, as it is, the emphasis is on the character of the subject under discussion (*περὶ τῶν τοιούτων*) and no reference is made to the death of Sokrates.

In these two passages then, 89 c and 107 a, it seems better not to find any special reference to the circumstances of the conversation. In maintaining this, I must not be thought to

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suppose that the dialogue is not full of such references to the impending death of Sokrates (as for instance 61 E), which more than any other so slight element give it its singular charm.

97 D. *ἐκ δὲ δὴ τοῦ λόγου τούτου οὐδὲν ἄλλο σκοπεῖν προσήκειν ἀνθρώπῳ καὶ περὶ αὐτοῦ ἐκείνου καὶ περὶ τῶν ἄλλων.*

Wagner quotes from K. F. Hermann the statement that *ἐκεῖνος* sometimes stands for the reflexive pronoun—that is, where ordinary usage would require the latter. The remark does not seem to apply to this passage, for we cannot with any propriety put *ἑαυτοῦ* here after *αὐτοῦ*, in the place of *ἐκείνου*. Plato might have used *ἑαυτοῦ* alone here, instead of *αὐτοῦ ἐκείνου*, but the latter expression sets the man off rather more as a distinct object of contemplation, whereas *ἑαυτοῦ* would express only the identity of subject and object. In the phrase used, *ἐκείνου* is to be regarded as depending directly on *περί*, and *αὐτοῦ* as the intensive pronoun in apposition with it. The same phrase occurs in Rep. 600 B: *Λέγεται γάρ, ὡς πολλή τις ἀμέλεια περὶ αὐτὸν ἦν ἐπ' αὐτοῦ ἐκείνου, ὅτε ἔζη.* Here the phrase means "in the time of him himself," and there can be no thought of any reflexive idea about it.

99 C. *τὴν δὲ τοῦ ὡς οἷόν τε βέλτιστα αὐτὰ τεθῆναι δύναμιν οὕτω νῦν κεῖσθαι, ταύτην οὔτε ζητοῦσιν οὔτε τινὰ οἴονται δαιμονίαν ἰσχὺν ἔχειν.*

Plato has been arguing that if one explains the universe as organized by a rational principle or element in it, the *νοῦς* of Anaxagoras, he ought to hold himself bound to show in every instance that things are as they are because they are best so; his idea seeming to be that this best possible condition is the only conceivable aim and end of rational action. Here at the close of his argument he speaks of those who have other explanations of the condition of things and ignore this one. The question of grammar in the sentence is whether *τοῦ* belongs to *τεθῆναι* or to *κεῖσθαι*. The interpreters generally say to *κεῖσθαι*. Stallbaum, for instance, following Heindorf, puts the words in this order (in his note), *τὴν δὲ δύναμιν τοῦ οὕτω νῦν αὐτὰ κεῖσθαι, ὡς οἷόν τε βέλτιστα τεθῆναι*, where of course

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he understands ἐστί with οἷόν τε and makes τεθῆναι depend on οἷόν τέ ἐστιν. In that case we should regard the whole as an expanded form (possibly the original form, Hadley's Grammar, 664 a.) of what would ordinarily be expressed by τὴν δύναμιν τοῦ ὡς βέλτιστα νῦν αὐτὰ κεῖσθαι. For such an expansion of the familiar phrase ὡς βέλτιστα, he might find a parallel in Rep. 530 A. The only one, apparently, who differs from this view is Ast, who joins τοῦ with τεθῆναι, and suggests that the sentence would seem more natural to us if it were put thus, τὴν δὲ τοῦ...τεθῆναι δύναμιν δι' ἣν οὕτω νῦν κεῖται. This seems the simpler and more natural explanation, for which a few reasons may be given. The order of the words is plainly in favor of it. Any one at first sight would suppose that the whole genitive lay between τήν and δύναμιν, and there seems to be no reason on the other theory for separating δύναμιν so far from τήν and interrupting by it the genitive phrase τοῦ... κεῖσθαι. It may be added that αὐτά would better go with the principal verb of the genitive phrase. Again, the common explanation leaves δύναμιν without anything to determine it, any expression of the effect produced by it, and obliges us to supply such an idea. Ast's explanation finds this in οὕτω νῦν κεῖσθαι, for the whole would be translated, "the power of the fact that things were once put in the best possible position (to secure) that they now remain so, this they neither look for nor," etc. This explanation regards οὕτω νῦν κεῖσθαι as depending on the active force of δύναμιν, for which the following examples may be adduced: Rep. 433 B. ὃ πᾶσιν ἐκείνοις τὴν δύναμιν παρέσχεν ὥστε ἐγγενέσθαι. Theaet. 156 A. τῆς κινήσεως δύο εἴδη, πλήθει μὲν ἄπειρον ἑκάτερον, δύναμιν δὲ τὸ μὲν ποιεῖν ἔχον, τὸ δὲ πάσχειν. Soph. 236 B. δύναμιν δὲ εἴ τις λάβοι τὰ τηλικαῦτα ἱκανῶς ὁρᾶν. But so familiar a fact as the dependence of an infinitive on a substantive needs no proof. These are only given as instances from Plato of such a use with the same noun as in the passage in Phaedo.

101 E. ἱκανοὶ γὰρ ὑπὸ σοφίας ὁμοῦ πάντα κυκῶντες ὅμως δύνασθαι αὐτοὶ αὐτοῖς ἀρέσκειν.

The commentators remark upon the strangeness of the expression ἱκανοὶ δύνασθαι, "able to be able." Hirschig, as

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might be anticipated, brackets δύνασθαι. Ast and Stallbaum regard it as intended for irony. The latter defends the text by a quotation from Philo *de Agricultura* (ἱκανοὶ εἶναι δύνασθαι), and by others from writers as late or later, where οἷός τε is followed by δύνασθαι. A somewhat similar redundancy occurs repeatedly in the Republic, e. g. 613 A. ὃς ἂν προθυμεῖσθαι ἐθέλῃ δίκαιος γίγνεσθαι, where προθυμεῖσθαι is a stronger expression of the idea of ἐθέλῃ. But it seems remarkable that no one, so far as I can learn, has ever supported the phrase in Phaedo by the almost identical one in an earlier author, THUC. II. 48: λεγέτω οὖν....τὰς αἰτίας ἅστινας νομίζει τυσαύτης μεταβολῆς ἱκανὰς εἶναι δύναμιν ἐς τὸ μεταστῆσαι σχεῖν. Here δύναμιν σχεῖν is put for δύνασθαι, but in other respects the expression is the same as that in the Phaedo, and there is perhaps some reason for regarding this passage too as ironical. It seems strange that neither passage has been used to illustrate the other.

GORGIAS.

453 C. ἆρ' οὐκ ἂν δικαίως σε ἠρόμην ὁ τὰ ποῖα τῶν ζώων γράφων καὶ ποῦ;

The last two words of this sentence have given great trouble to all editors of the Gorgias. Some, as for instance Ast, defend the above reading by some unusual explanation of ποῦ. Thompson, the last English editor, brackets the two words as hopeless. Others propose various changes—πῶς, πῇ, and πόσου, and even τοῦ (*i. e.* τίνος, supplying υἱός ἐστιν). Stallbaum in his third edition mentions with decided approval, though he does not put it into his text, the conjecture ποῖ 'οὔ, which came to him from two independent sources. Then the question reads: "Should I not have been justified in asking you, when you had said Zeuxis was a painter of living objects. what living objects he painted, and what ones he did not?" This certainly is probable and falls in with the line of the argument better than the common text or any of the other conjectures. No good explanation can be given of his asking what living objects Zeuxis painted "and *where*," or "and *how*," or "and *for how much*," or, least of all, "and *whose son he was*." But there is a suggestion made in President Woolsey's edition (1842) of the Gorgias which seems to deserve more

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attention than it has yet received. It was evidently original with him, as appears from his language. "I conjecture (that I likewise may contribute my mite) that the sentence originally ended at γράφων. * * Next to γράφων came ἢ οὔ, ΗΟΥ, which was corrupted into ΠΟΥ, and then καί was added to bring ποῦ into grammatical connection with the (rest of the) sentence. * * A similar corruption of ἢ οὔ into που, in Rep. 437 D, is removed in modern editions." The neatness of this suggestion and its complete success in meeting the difficulties will strike every one at first sight. It seems strange that it should have occurred to but one other scholar, Deuschle, who suggested it in his first edition (1859). In Cron's Bearbeitung (1867) of Deuschle's edition, the suggestion is omitted and the old text restored. Deuschle's conception of the sentence and of the way in which the mistake in the reading arose is almost precisely like Woolsey's, but he neither refers to any authority, nor speaks as if it was an idea of his own. The passages which he compares, 454 A, 479 E, resemble this, but not closely enough to justify the reading ἢ οὔ; In a review by Keck in Jahn's Jahrbücher, 1861, the objection is made that after a question expecting an affirmative answer, ἆρ' οὐκ ἂν κτλ., "is it not?" it is at the least superfluous to ask "or is it not so?" Deuschle had the same difficulty in mind and was moved by it to suggest whether οὐκ in the opening of the question should not be οὖν. Still the objection seems to be entirely unfounded, as may be seen by translating the whole sentence here, with the reading ἢ οὔ; "Should I not have been justified in asking what living objects Zeuxis painted? Or should I *not?*" Such a succession of questions in contrasted form occurs repeatedly in Plato. An instance just like this is found in Rep. 468 B, τὸν δὲ ἀριστεύσαντά τε καὶ εὐδοκιμήσαντα οὐ . . . δοκεῖ σοι χρῆναι στεφανωθῆναι; ἢ οὔ; "Don't you think the brave man ought to be crowned? or don't you?"

461 B. ἢ οἴει, ὅτι Γοργίας ᾐσχύνθη σοι μὴ προσομολογῆσαι τὸν ῥητορικὸν ἄνδρα μὴ οὐχὶ καὶ τὰ δίκαια εἰδέναι, . . καὶ ἐὰν μὴ ἔλθῃ ταῦτα εἰδὼς παρ' αὐτόν, αὐτὸς διδάξειν—.

Much of the difficulty of this passage arises from the heaping up of negatives in it. A strict translation will show

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this: "Or do you think, because Gorgias was prevented by shame from (not) admitting that the rhetor does not know what justice is, and that, if a pupil came to him ignorant of this, he would not teach him —." What Gorgias had said was that the rhetor knows justice, and, if any one came ignorant of it to him to be taught rhetoric, he would teach him justice first. What Polus here maintains is that this was just the opposite of the real opinion of Gorgias, but that he was prevented by shame from expressing his real opinion. Now the double negative *μὴ οὐχί* is used here with the infinitives *εἰδέναι* and *διδάξειν*, because they depend upon a verb of negative meaning, *μὴ προσομολογῆσαι*, meaning *to deny*, to which verb a negative is added by the use of *ᾐσχύνθη* before it (Hadley's Grammar, 847 a., Goodwin's Modes and Tenses, 95, 2, N. 1, b.). In other words, the sentence is equivalent to *οὐκ ἀπηρνήθη μὴ οὐχὶ εἰδέναι καὶ διδάξειν*, in which *οὐκ* represents the effect of *ᾐσχύνθη*, and *ἀπηρνήθη* is substituted for *μὴ προσομολογῆσαι*. In the next sentence we have *τίνα οἴει* giving the same negative effect which *ᾐσχύνθη* here gives, *ἀπαρνήσεσθαι* used in place of *μὴ προσομολογήσειν*, and then *μὴ οὐχί* and the rest substantially as here. The only editor, so far as I see, who clearly recognizes the negative influence of *ᾐσχύνθη* is Kratz (Stuttgart, 1864). It will appear plainly by supposing that word changed into an affirmative, for instance, *ἐδυνήθη* or *ἐτόλμησε*. No one, I think, will doubt that the sentence then would be *ὅτι Γοργίας ἐτόλμησε μή προσομολογῆσαι τὸν ῥητορικὸν ἄνδρα μὴ καὶ τὰ δίκαια εἰδέναι κτλ.*, "because Gorgias had the courage to deny that the rhetor knew," having *μή* with *εἰδέναι* instead of *μὴ οὐχί*. This seems overlooked by Stallbaum, who also erroneously supposes that *αὐτὸς διδάξειν* depends on a verb of affirmative meaning to be supplied from *μὴ προσομολογῆσαι*. Now if this is done it will be necessary also to supply *μή*, from *μὴ οὐχί*, with *διδάξειν*, as will appear from translating again with such an affirmative verb: "Because Gorgias was prevented by shame from admitting that the rhetor does not know justice, and (from asserting) that he himself would not teach it to his ignorant pupil." It is plain that the "not" before "teach" is necessary, for Gorgias said he would teach,



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being, as here alleged, ashamed to say that he would not. Now it is much better to make *αὐτὸς διδάξειν* depend directly upon *μὴ προσομολογῆσαι*, as *εἰδέναι* does, and then it gets its negative from *μὴ οὐχί*, which belongs to both infinitives in the same way and for the same reason. What Gorgias said was, in effect, *οἶδε, καὶ, ἐὰν ἔλθῃ μὴ εἰδώς, αὐτὸς διδάξω.* Here that whole statement is put under the influence of *μὴ οὐχί*, because it all depends upon *ᾐσχύνθη μὴ προσομολογῆσαι.* If we translate into English and represent *μὴ προσομολογῆσαι* by *deny*, then of course the negative, as translation of *μὴ οὐχί*, disappears entirely from what follows, as the English idiom does not admit it. "He was ashamed to deny that the rhetor knew what justice was, and that, if a pupil came to him without such knowledge, he himself would impart it."

471 D. *κατ' ἀρχὰς τῶν λόγων, ὦ Πῶλε, ἔγωγέ σε ἐπῄνεσα, ὅτι μοι δοκεῖς εὖ πρὸς τὴν ῥητορικὴν πεπαιδεῦσθαι, τοῦ δὲ διαλέγεσθαι ἠμεληκέναι.*

The editors generally remark upon *δέ* here as meaning "although," translating: "At the opening of our talk I complimented you, Polus, because you seem to me to have been well trained in rhetoric, *although* you have neglected logic." The reference is to 448 D, where Sokrates says at first, *Καλῶς γε, ὦ Γοργία, φαίνεται Πῶλος παρεσκευάσθαι εἰς λόγους*, and then a little later, *δῆλος γάρ μοι Πῶλος . . ὅτι τὴν καλουμένην ῥητορικὴν μᾶλλον μεμελέτηκεν ἢ διαλέγεσθαι.* Now the compliment is contained in the word *καλῶς*, which is separated by several remarks from the reference to his neglect of dialectics. The need of translating *δέ* 'although' arises from the bringing the two together here, as being both included under *ἐπῄνεσα.* How could Sokrates praise any one for neglecting that which was the business of his own life? But it is worthy of question whether Thompson, the latest English editor, is not right in regarding this remark as purely ironical. After the description which Sokrates has been giving of the character of rhetoric in his view, as a mere tricky art, not deserving the name of a science, corresponding to cookery as a pander to the whims and weaknesses of the mind, it is hardly to be supposed that he would praise any one for any degree of proficiency in it. In this view *ἐπῄνεσα* does not mean serious

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praise for skill in rhetoric, but ironical praise as for skill in cheating, and then he goes on to the next clause and in the same way *praises* the neglect of logic. Thus we may translate δέ as usual by 'and.' "I paid you a left-handed compliment, Polus, for your skill in a base pursuit and ignorance of a noble science."

A similar irony may have misled the editors in 486 C, where they generally make περισυλᾶσθαι and ζῆν depend upon ἔθηκε, thus letting the same verb govern first a predicate adjective (χείρονα or δυνάμενον) and then an infinitive. Is it not better to put these infinitives in the same construction with the preceding βοηθεῖν and ἐκσῶσαι, depending on δυνάμενον? Then Kallikles ironically describes the man of thought, in contrast with the man of affairs, as "*having power* to be plundered of all his property and to live as an outcast in his town."

506 D—507 A.

It is perhaps worthy of remark, as no edition that I have refers to it, how Plato makes Sokrates in this brief recapitulation of the previous dialogue represent the questions as coming, not from himself, but from another person, and the answers as in a sense forced from him. This appears in the first answer, to the question, "Are pleasure and good the same thing?" "Not the same, as I and Kallikles have agreed." Here the questioner would seem to be some third party, but just below, the answer, "It seems to me to be necessary, Kallikles," implies that he, Kallikles, is asking the questions; and so at the end Sokrates pretends to concede the conclusion unwillingly as forced from him by stress of logic. Thus he is made to seem to abandon all effort to guide the opinion of Kallikles, but at the same time he foists his own belief upon him and represents him as the one who led others to the conclusion, so opposed to the character of the man, that the conquest of one's self is the chief good.

511 D. *τὴν κυβερνητικήν, ἣ οὐ μόνον τὰς ψυχὰς σώζει, ἀλλὰ καὶ τὰ σώματα καὶ τὰ χρήματα.*

Here ψυχή is not contrasted with σῶμα, "soul and body," as it is in 512 A, but means simply 'life.' Then, as Kratz point

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out, the idea of life need not have been repeated in the next clause; it would have been enough to have gone on, "Not only lives, but also possessions." But the more emphatic phrase is chosen, especially as *σώματα καὶ χρήματα* is a frequent combination in this sense. And all difficulty is removed from the passage by translating the *καί* after *ἀλλά*, not by 'also,' as usual in such a place after *οὐ μόνον*, but by 'both.' Then the whole reads, "The art of navigation, which saves not only lives, but both lives and property." There would be no ambiguity in the Greek sentence as spoken or read, for a slight emphasis on that first *καί*, with no pause at all after it, would show the above sense perfectly.

512 A. *λογίζεται οὖν ὅτι οὐκ εἰ μέν τις μεγάλοις καὶ ἀνιάτοις νοσήμασι κατὰ τὸ σῶμα συνεχόμενος μὴ ἀπεπνίγη, οὗτος μὲν ἄθλιός ἐστιν ὅτι οὐκ ἀπέθανε, . . εἰ δέ τις ἄρα . . ἐν τῇ ψυχῇ πολλὰ νοσήματα ἔχει καὶ ἀνίατα, τούτῳ δὲ βιωτέον ἐστίν.*

Most of my editions (Ast, Stallbaum, Woolsey, Deuschle's first, Thompson) say that *οὐκ* in this sentence belongs to the latter part only, to the clause *τούτῳ δὲ βιωτέον ἐστί*, and that the prior clause is put in only for the contrast and in coördinate form where other languages would make it subordinate. But others (Heindorf, Kratz, Cron's Deuschle) say that *οὐκ* belongs to the whole, denying the truth of such a contrasted statement as is conveyed by the two clauses together. It seems clear that this latter is the true view. The position of the *οὐ* suggests such a connection of it with all that follows, though it does not of course require it. But the idea to be expressed seems to require it. Plato does not mean to ascribe to his "meditative skipper" (as Thompson calls him) the belief that the man whose body is incurably diseased would be happier if drowned at once. He sometimes hints at such an opinion himself (e. g. Rep. 410 A), but he is not so sanguine as to imagine such philosophy in every shipmaster in the Peiraeeus. He means that such a man with only ordinary common sense can see the contradiction and absurdity in supposing that that would be true of the man incurably sick in body but not of him incurably sick in soul. The thing denied by *οὐκ* is the coëxistence of the two following thoughts, which may be well

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represented by translating οὐκ thus: "*It is not the case* that if," etc. This may be seen more clearly in the similar sentence in 516 E: *οὔκουν οἵ γε ἀγαθοὶ ἡνίοχοι κατ' ἀρχὰς μὲν οὐκ ἐκπίπτουσιν ἐκ τῶν ζευγῶν, ἐπειδὰν δὲ θεραπεύσωσι τοὺς ἵππους καὶ αὐτοὶ ἀμείνους γένωνται ἡνίοχοι, τότ' ἐκπίπτουσιν.* Here it is plainly impossible to take the first clause out from under the negative influence of οὐκ in οὔκουν. We cannot make the assertion, "Good drivers are not thrown at the beginning," and then go on, "but when they are more skilful, then are not thrown." Plainly the negative goes with the whole: "*It is not true* that at the beginning they are not thrown, but after becoming more skilful are thrown." The thing denied in both these passages is the coëxistence of two conflicting things, not either of these things separately. This may be seen more clearly, perhaps, if in the second part of the sentence in 512 A, we substitute for the words Plato uses a simple repetition of the statement in the first part with the necessary negative. The whole would then read: *λογίζεται οὖν ὅτι οὐκ εἰ μέν τις ἀνιάτοις νοσήμασι κατὰ τὸ σῶμα συνεχόμενος μὴ ἀπεπνίγη, οὗτος μὲν ἄθλιός ἐστιν ὅτι οὐκ ἀπέθανε, εἰ δέ τις ἄρα ἀνιάτοις νοσήμασι κατὰ τὴν ψυχὴν συνεχόμενος μὴ ἀπεπνίγη, οὗτος δὲ οὐκ ἄθλιός ἐστιν ὅτι οὐκ ἀπέθανεν.* Here it would be impossible to join the οὐκ before εἰ μέν with this last clause, for there we have a negative already and the two would cancel, leaving the second conclusion the same with the first, *ἄθλιός ἐστιν.* But *τούτῳ βιωτέον ἐστίν* is only an equivalent in sense for *οὐκ ἄθλιός ἐστίν ὅτι οὐκ ἀπέθανεν*, and therefore to join the οὐκ before εἰ μέν with it alone would have the same fatal effect upon the meaning. The difficulty or confusion has arisen from Plato's using this affirmative form here (*βιωτέον ἐστί*) instead of the negative form (*οὐκ ἄθλιός ἐστι*) suggested above. If he had used the latter, no one probably would have thought of joining the οὐ with it alone. The use of this affirmative form is also the reason for the opposition to it expressed in the next following words: *ἀλλ' οἶδεν ὅτι οὐκ ἄμεινόν ἐστι ζῆν τῷ μοχθηρῷ.* The sense of the whole is this: "He reflects that it is not the case that the man with diseased body would wish to die, while the man with diseased soul would wish to live, but he knows that the latter is not profited by living."

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## II.—*On the Nominal Basis of the Hebrew Verb.*

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The object of this paper is to state what may be regarded as established concerning the Nominal Basis of the Hebrew Verb, to contribute something towards the solution of questions still undecided, and to inquire into the traces of nominal origin visible in the Syntax of the language.

There are certain quasi-verbal forms in Hebrew and the related languages, that are manifestly nominal. Such are Hebrew *yesh* or *ish* (יֵשׁ or אִשׁ), Aramaic *ithai*, *ith*, used as substantive verb, and the negatives Hebrew *ayin* (אַיִן), Arabic *laisa*, *lâta*, Aramaic *laith*, *lêth*. *Yesh* is found once (Prov. viii. 21) as a substantive in the sense of 'substance,' οὐσία, and *yeshka* (יֶשְׁךָ) may mean 'thy being or existence' = 'thou art': so '*od* (עוֹד) is a substantive, used adverbially, and acting as a verb when suffixes are attached to it; '*odi* '*omed* (עוֹדִי עוֹמֵד) 'my continuance is standing' = 'I am still standing.' Along with these may be probably put the so-called Arabic "Verbs of wonder." Of these the form that takes an accusative after it is usually explained as the causative (IV.) of the verb, e. g. *ma ah.sanahu* 'how goodly he is!' literally, 'what has made him goodly?' But the diminutive form of this expression, *ma uhaisinahu* 'how very goodly he is!' clearly points to a substantival conception of it, the sense being: 'What a beautifying of him!' The other form also, *ahsin bi.hi*, commonly regarded as an imperative = 'make him goodly!' (*i. e.* 'he is goodly'), is simply the noun, 'goodliness in him!' It is not surprising that the accusative suffixes are attached to the first form, since the same construction is found with the Nomen Verbi; we may regard this form as an old Nomen Verbi, identical with the present third singular masculine perfect of the causal, but dating from a time when the distinction between noun and verb did not exist. There are a few other Arabic forms which seem to point to a similar explanation.

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The obviously Nominal Infinitive and Imperative may be passed by with a word. The former is in all respects treated as a simple substantive, taking suffixes freely, and also admitting variation of gender. The latter is peculiar only in that it has petrified certain affixes of gender and number, and these the same that appear in the verb, whence we may infer that at an early period the noun-forms divided themselves into two classes, of which one moved towards the fully developed form of the noun proper, the other towards that of the verb proper.

Coming, now, to the Perfect of the Simple Stem, the ground-form *katab* (*kataba*) is identical with the noun, and the inflection is purely nominal. The third person is without pronominal addition, probably because this was not needed for distinctness of reference after the other persons had been distinguished by such additions. The variations of gender and number are purely those of the noun: singular masculine *kataba* (Hebrew *kātab*), feminine *katabat* (Hebrew *kātebā*), plural masculine *katabū* (Hebrew *kātebū*), feminine *katab.na* (= *katabāna*, Aramaic *ketabēn*, *ketabā*, comp. the imperfect ending *ān*), dual masculine *katabā*, feminine *katabtā* (= *katabatā*). The second person has the pronominal affix, singular *ta*, *ti*, dual *tumā*, plural *tum*, *tunna* (Aramaic *tūn*, *tēn*, Hebrew *tem*, *ten*). The first person has singular *tu* (*ti*), plural *na* (*nu*). Here the general agreement between these affixes and the pronouns is too close to permit doubt of their identity, but a difficulty arises from the divergence of the initial letters of the two sets of forms, particularly when the Assyrian, Aethiopic, and Mehri dialects are taken into consideration. While the separate pronoun has *ta* in the second person, the noun has *ka*, and the Aethiopic has *ka* in the verb. On the other hand, while the separate first personal pronoun sounds *anoki* (Assyrian *anaku*), Arabic, Hebrew, Aramaic have *tu* or *ti* in the verb, and Assyrian and Aethiopic *ku*. What is the explanation of this seemingly arbitrary appearance of the *t* and *k* letters? The simplest way of escaping the difficulty would be to suppose a phonetic interchange of the two, the existence of which, however, in Shemitic is unproved, and its

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laws here unexplained. Or it might be supposed that the original full pronouns contained both the letters (as, for example, second person *an.ta.ka*), from which the various dialects selected such parts as they preferred. This somewhat cumbrous hypothesis might derive a seeming support from the Egyptian second person pronoun singular masculine *ntek*, Coptic *entak*, but this combination of *t* and *k* is elsewhere lacking in Egyptian, and there is no indication of its existence in Shemitic. There is proof, however, that the demonstrative stems *ta*, *ha*, *sa*, *ka*, *na* are freely employed in Shemitic in a very general way for defining objects, and it is in accordance with what we know of the history of language to suppose that they were originally employed without distinction of person, the language gradually settling down on certain forms for certain personal distinctions.* Before the breaking up of the primitive Shemitic people, and before or along with the origination of the compound forms (*an.ta*, etc.), the *k*-stem (used also in some particles) was appropriated to the nominal suffixes of the second person, and by some languages to the verb also, while other languages (governed by considerations not yet known) chose the *t*-form for the verbal subject-affix. So in the first person, while *ni* or *i* was generally taken for the suffix, by some *tu* (*ti*), by others *ku* was chosen for the subject-affix of the verb. This view is not without its difficulties, since the reasons for the selection of one or another pronominal form are not given, but it seems open to fewer objections than any other. Not only in the simple stem but also in the derived stems the ground-forms are nouns, and most of them have their parallel forms in the substantives in common use. The principal varieties of stems are made by prefixing letters (*'a*, *ta*, *sa*, *na*) or by doubling consonants or broadening vowels within the stem, and these modes of formation are all found among substantives, though the verb has developed a greater freedom in this respect than the noun, by reason of the greater variety of aspects exhibited by it. With the intensive or Pael we may compare the large class of nouns (usually denoting occupations) that double the middle stem-

* Comp. Merx, *Grammatica Syriaca*, § 50.

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letter; with the affective or Poel, the form of the active participle Qal; with the causal or Hiphil (Aphal) the intensive adjective used for comparative and superlative in Arabic; with Paiel, the Arabic diminutive; and the prefixes *ta*, *sa*, *na* occur in the noun as well as in the verb. True, we cannot in all cases discover any close resemblance between the effects of these literal changes on the significations in noun and verb; but it is to be regarded as probable that these effects were at first very general and undefined, and only after a considerable time passed into the strict limits in which we now find them; moreover the verb-stems themselves show a somewhat wide range of meaning.

The Nominal origin of the base of the Imperfect is indicated in general by the modal terminations *u*, *a*, *an*, *i* (of which there remain in Hebrew only *an* in the cohortative and some suffix-forms, and traces of *i* in suffixes; as in the other Shemitic languages also, *i* has been generally dropped), which are identical with the case-endings of the noun. This identity is too exact to be explained as accidental, and the verb and the noun in their developed state differ from each other too much to allow the supposition of an imitation of one by the other. This remains true whether we can satisfactorily explain the modal significations of these terminations or not. On this point there is very general agreement. But there is great difference of opinion as to the origin of the existing forms. To begin with the third person. The old view that the prefix *ya* comes from the substantive verb *hawa* is now abandoned; though this might explain the meaning of the imperfect, *ya* could not be gotten from *wa*, and moreover the Syriac prefix *n* would be left unaccounted for. It is held by many that *ya* is from the third singular personal pronoun *hu.wa*. But similar objections lie against this view. There is no such masculine pronoun as *ya*, nor can this form be supposed to be a variation of *wa*, for, though Hebrew almost always changes initial *w* to *y*, this would not account for the appearance of *y* in Arabic, which finds no difficulty in an initial *w*; and, as above, we have here no explanation of the Syriac *n*. We are thus led to the supposition that the *y* (and so also the *n* of



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Syriac) is a nominal formative, and the third person singular masculine of the imperfect a mere noun, precisely analogous in this respect to the corresponding person in the perfect. The nominal formation by prefixed *y* is not uncommon in the Shemitic languages. In Hebrew the majority of the examples are proper names, but there are not wanting common nouns, as *yiṣ.har* 'oil,' *i. e.*, 'the shining (liquid).' The proper names were originally appellatives, and are in many cases obviously not imperfects of a verb, but simple nouns. So, many names of places, as *Yogbehah* (יָגְבְּהָה) 'height,' or 'high,' *Yâgur* (יָגוּר) 'sojourning-place,' the river *Yabboq* (יַבֹּק) 'the gushing stream,' *Yiphtah* (יִפְתָּח) 'the open place.' And, finding names that belong both to persons and to places, as *Yiphtah* and *Ya'beṣ* (יַעְבֵּץ), we are justified in regarding the personal names also as true nouns, as *Ya'aqob* (יַעֲקֹב) 'trickster or supplanter,' *Yuda* (*Yehuda*) 'praised,' *Yeroham* 'loved,' *Yezreel* 'God's sower,' *Yisrael* 'God's victory or prince.' It is also noteworthy that these nouns agree both in form and meaning with the imperfects of derived stems (chiefly Hiphil and Hophal) as well as of the simple stem (Qal): *Yoseph* and *Yabneh* are identical with Hiphil of *yasaph* and *bana*, *Yubal* and *Yudah* with Hophal of *yabal* and *yada;* the natural and sufficient explanation of which is that the derived stems were nouns and received the prefix *ya* (*yu*) just as the simple stem did. Alongside of *wasaph* (Hebrew *yasaph*) 'increaser' (*crescens*) was *awsaph* or *hawsaph* (Hebrew *hosiph* = *hosaph*, *hoseph*) 'causing to increase,' whence *ye.hoseph* = *yoseph* 'increaser'; from *hudah* (Hophal of *wadah*, Hebrew *yadah*) came *ye.hudah* = *yudah* (*laudatus*). So concrete nouns with prefix *m* (participles) follow the form of the derived stems, *maktib*, *muktāb*, etc.; and if the language had developed a verbal form from this noun (a *makteb*, *maktabta*, etc., 'he is causing to write, thou art causing to write,' etc.), we should have the true verb and the true noun standing alongside of one another, with the possibility of a free formation and use of both, long after the original signification and force of the prefix had been forgotten. The same thing may be true of the prefix *ya*, and we may therefore hold ourselves

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justified in regarding the base of the Hebrew imperfect as a noun, along with the proper nouns* and common nouns above cited.

But besides this *y*, we find another preformative of the third singular masculine imperfect, the Syriac *n* (*nektub* = Hebrew *yiktob* = Arabic *yaktubu*), which cannot be from the substantive verb *hawa*, or from the personal pronoun *huwa*, but may be the element *n* (*an*) found in all the personal pronouns (Aramaic *in.hu*), which is a demonstrative, used also in nominal forms (taking the term demonstrative in a very general sense, without expressing an opinion as to the original form and meaning of the elements so designated); *nektub* (for *naktub*) would then be just such a noun as *yiktob* (for *yaktub*). No more satisfactory account of this prefix has been proposed, though it may not be possible to define the original force of the *na*. The most obvious point of comparison is with the Shemitic reflexive with the same prefix (Niphal), of which the meaning is: 'he acted on himself,' while the imperfect signifies: 'he set himself to act, entered on acting,' or, if we go back to the nominal form (in which the essential signification was doubtless already fixed), the Niphal *naqṭal* = 'him killing,' 'one killing

* Among these is to be put the Hebrew quadriliteral divine name יהוה, which, like all other Shemitic names of the Deity, is to be regarded as an appellative. It is not easy, however, to fix its meaning. It stands almost alone; outside of Hebrew there is, as far as is now known, no divine name that resembles it. The Hebrew forms that come nearest to it are *Yēshu'* (יֵשׁוּעַ) and *Yehu'* (יֵהוּא), made from the stems שׁוּעַ and הוּא by the prefix *yē* = *yā*, and identical in form with the imperfect Qal (as יָבוֹשׁ from בּוֹשׁ); with יֵהוּא compare יְהוֹ for יְהוֹ (*Yeho* for *Yaho*) found as a component in proper names. The only vocalization that explains the forms of the quadriliteral given in the Old Testament is יַהְוֶה *Yahwe* (from which *yahu*, *yah*, *yeho* easily came), made from הוה by prefix יַ and signifying 'being' (Qal) or 'causing being' (Hiphil); the former was apparently the ancient Hebrew understanding of the name (Ex. iii. 14). As the stem *hawa*, though common in Aramaic and Arabic, is rare, almost obsolete in classic Hebrew (re-introduced at a later time by Aramaic influence), the name takes us back to an early time, perhaps before the separation of the Hebrew (Phenician-Canaanitish) from the parent stock. It was possibly an old Shemitic name, appropriated by the Hebrews in accordance with that higher conception of the Deity that seems to have belonged to them from the beginning, fixed as the national name by Moses (Ex. vi. 3), yielding at a later period (Ezra, Ecclesiastes) to the more generic term Elohim.

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himself, the imperfect *naqṭul* = 'he killing,' 'one proceeding to kill.' This makes the prefixes simple pronouns (not, however, signs of person), and is perhaps too general to be satisfactory; yet it is probable that these formatives were originally indefinite in meaning, and gradually settled into distinct shape, and it is possibly not accidental that the only Shemitic family that has no reflexive with prefix *n* is that which uses *n* as prefix of the imperfect. The two may, however, have arisen independently of each other, and the decision of this question does not affect the supposition of a nominal formative *n*, of which there are examples in Hebrew, as *ne.sibba* (II. Chr. x. 15) and perhaps *Neballâṭ* (Neh. xi. 34), and in Aramaic, as *nebizba* (Dan. ii. 6).

To this account of the third person masculine imperfect it is objected that the other persons must then be formed on it as base, and we should have *ta.yaktulu*, etc., from which *taktulu* could come only either by the falling out of *y* and the coalescence of the two vowels, which ought to produce *â*, or by the falling away of the syllable *ya*, which, it is said, is improbable. But the coalescence of two vowels does not always produce a long vowel, as is clear from such forms as *kan.ta* for *kawanta* (from כּוּן); and the falling out of a syllable *yi* = *ya* seems actually to take place in the Biblical-Aramaic *leheweh* (לְהֵוֵה), if, as is probable, that is for *leyeheweh*. In the participle also (Hebrew *maktib* = *me.haktib*, Arabic *muktib* = *mu.aktib*) there is a disappearance in the Arabic of an *a* without trace. However, it is not necessary to suppose that the following forms are all based on the third person masculine; this is certainly not true of the third person feminine, and of the others it may be supposed that they are formed independently on the same base with the third person masculine. We come, then, to the other forms.

The prefix *ta* of the third singular feminine *ta.ktubu* (*tiktob*) is not a personal pronoun, for there is no feminine personal pronoun of this form. It may be the sign of the feminine, as in the noun and the perfect of the verb, or an independent nominal prefix. In spite of the plausibility of the former view, the analogy of the other persons seems to decide against

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it. We could not say that it is impossible for a Shemitic noun to have the feminine sign prefixed (though there is no established case of such prefix), but it is obviously the method of the imperfect forms to affix the sign of gender, as in the second singular and third and second plural. If it be said that the feminine sign was prefixed because, if it had been affixed, there would have been no difference between perfect and imperfect, the answer is, that the analogy of the imperfect plural third feminine would lead us to expect not *katab.at* (it would, indeed, in any case be rather *ketubat* or *kutb.at*), but *yaktubat* (as *yaktubna*), without possibility of confusion with the perfect; and it is equally unsatisfactory to say that the feminine sign has been prefixed by imitation of the masculine third person, since the *ya* is not a sign of gender. Rather we have here an independent noun, made by prefix *ta* from the base *ktubu*. It is no doubt a serious difficulty in the way of this view that the nouns in common use made by this prefix are all masculine, except those that have the feminine *at* affixed, and it may be reasonably objected that we should at least expect the *i* at the end, as in the second person. But this difficulty seems less formidable than those that attach to the other views above mentioned. It is possible to suppose that such a noun as *taktubu*, originating at a time when genders were not yet definitely marked, came to be used for a feminine in accordance with the same general movement that later fixed *t* as a feminine termination.

The preformatives of the first and second person singular agree in form and meaning with the corresponding personal pronouns: *a* appears in Arabic *an.ā* (often *an.a* in poetry), Aethiopic *an.a*, Assyrian *an.a.ku*, Hebrew *ānoʿki*; it is hardly from *an*, since there is no trace of an assimilation of *n*; *ta* in *an.ta* was a personal pronoun in the primitive Shemitic. It may be that these demonstrative elements were employed as prefixes before the personal significations were fixed, as in the perfect, and that they gradually acquired personal meaning as the separate pronouns were fixed. This would more easily accord with the fact that they are prefixed immediately to the base *ktubu* and not to *yaktubu*. As the two second persons

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were not distinguished by the prefix, the feminine received the affix *i* (with an added demonstrative *n* in Arabic *īna*, Aramaic *īn*), the origin of which is not to be sought in Hebrew *hi'*, Arabic *hi.ya* or *an.ti*, since it equally needs explanation there; but rather we must suppose that, after usage had selected such a form as *hi* or *ti* from the coexisting *hu*, *ha*, *hi* or *tu*, *ta*, *ti* as feminine, the letter *i* came to be identified with this gender (though not exclusively), and was employed to indicate it in the imperfect, where the prefix failed to make the necessary distinction. And since the prefix is *ta* and not *ti*, this indicates that the form *ta.ktubu* served at first equally for both genders, as in the Pentateuch the pronoun הוּא (*hū'*) is both masculine and feminine. The mode of formation of the plural third person is somewhat different from that of the singular. The masculine, indeed, is simply the plural of the masculine singular (after the nominal formation), but the feminine, instead of being based on the singular feminine, is derived (except in Hebrew and Mehri) from a feminine of the masculine singular; for, from a comparison of the Assyrian (*iktuba*), the Aramaic (*nektebon*), and the Aethiopic (*yektebā*) it appears that the Arabic *yaktub.na* is for *yaktubāna*, the plural of a feminine singular *yaktubat*, of the existence of which, however, there is no other trace. Yet the possibility of some such vanished form we must admit, in order to account for the existing plural feminine. Hebrew and Mehri are peculiar in having initial *t* instead of *y*, (Hebrew *tiktob.nā*, Mehri *tenḥaên**), that is, in forming the plural feminine directly from the singular feminine, which is apparently a more regular process than that of the other dialects. It could be considered a doubling of the feminine sign only in case the singular *taktubu* were proved to be essentially feminine, the doubtfulness of which has been referred to above. The second person plural is made regularly by forming masculine and feminine plurals of the singular *taktubu*, on the prefix of which see above. The *na* of the first plural *naktubu* is the last part of the pronoun *an.ah.na* (Aethiopic *nahna*, Arabic *nahnu*, Hebrew *anahnu*), though it

* H. von Maltzan, ZDMG, xxv. p. 201.

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may have been employed at first in a merely general demonstrative sense. The dual presents nothing peculiar; its forms are the regular duals of the singulars *yaktubu* and *taktubu*.

The base of the imperfect is monosyllabic (in contrast with the dissyllabic perfect base), the vowel standing usually under the second radical (as *ktub*), but sometimes in certain dialects (Aethiopic, Mehri) under the first (*katb*, where a helping vowel *e* or *i* is commonly introduced under the second radical). In this case Aethiopic has differentiated the base into two uses. The monosyllabic stem seems (from comparison of infinitive and imperative) to have been connected with an abstract signification, as the dissyllabic with the concrete, and from the union of this abstract base with the prefix results a concrete noun. We naturally inquire the origin of this base and prefix. As to the base, it is better taken as an original substantive (as *ktab* = *katb*), rather than as a phonetic diminution of the longer form (*katab* or *kataba*) for easier pronunciation on the addition of the prefix. As far as we know, the forms *katb* (=*ktab*) and *katab* existed side by side from the earliest times, and whether one came from the other or the two were parallel derivatives from some simpler form, cannot now be determined (the same remark is to be made of the Aphel *aktab* and similar perfects). More exactly, the imperfect base in the simple stem (Qal) appears under the forms *ktub*, *ktib*, *ktab*, in the differences of whose vowels it is not possible to make out any clear scheme of symbolism. Of our three forms the first is (in the imperfect) commonly transitive but sometimes intransitive, the second and third commonly intransitive, but sometimes transitive; on the other hand, in the perfect the vowel-usage is nearly the reverse of this, *katab* being transitive and *katib* and *katub* intransitive. The attempt to discover symbolic meaning in the *u*, *i*, *a* is rendered still more difficult by a reference to other verbal forms: the passive perfects are marked by a persistent *u* in the first syllable (followed in Arabic by *i*), but the passive participle in the simple stem has the same sequence of vowels as the imperfect active (*maktūb*, *yaktub*), and in the derived stems is based on an *a*-form (*mu.kattab*, etc.); a similar

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apparently arbitrary use of the vowels exists in the nominal forms. It is not only difficult to fix any symbolism in the vowels, but also to discover any fixed force assigned them by usage. If it be said, for example, that the simplest vowel *a* was naturally employed in a transitive sense, and then as a matter of course in the perfect the intransitive sense assigned to the remaining vowels *i*, *u*, which naturally appear also in the allied passive, it must be added that in the imperfect the use was nearly reversed, and we must conclude that the vowels occupied an indifferent position in respect to the idea of action. It would be rash to say that there was never any distinction in meaning between the three vowels, but certainly the data for its determination, if it did exist, are not now at hand; we can only hold that from various forms originally standing side by side the different dialects have made various selections, and into them introduced certain euphonic changes, according to laws not yet discovered.

The uncertainty of the origin of the prefix *ya* is seen in the diversity of the opinions held in relation to it. It appears under the forms *ya*, *yu*, *ye* (Hebrew Shewa), which are parallel with those of the participial preformative *ma*, *mu*, *me*; of these the third, however (*ye*), is simply a weakening of one of the other two. The second (*yu*) is found in Arabic throughout the passive and in the active of three of the derived stems (II., III., IV.), and in Assyrian apparently in nearly the same set of stems. Of a difference of meaning between *ya* and *yu* there is no clear trace. The supposition that these vowels mark different personal relations (*u* the person speaking, *a* the person spoken of), in itself precarious, is useless here. The explanation of the vowel-difference may more reasonably be sought in dissimilation (*ya* being found in some *u*-bases, *yu* in some *a*-bases, but not regularly), or in analogy (the passive *yu* may be suggested by the passive perfect *kutiba*). The force of the prefix *ya* was originally that of a simple demonstrative. On the supposition that its Assyrian form *i* is a weakening of an original *a*, it has been surmised that it was merely euphonic, introduced to facilitate the pronunciation of a word beginning with two consonants (*ktub*, and so Aphel

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*aktab* for *ktab*), according to a not infrequent usage in the Shemitic languages; but, while the form of the Aphel might be thus explained, it would not be easy to account for the *ya* of the imperfect; rather, a comparison of the modern Arabic *iktub* with the ancient *yaktubu* indicates that the reverse is the case, the *i* is a weakening of *ya*. All the phonetic phenomena point to an original *ya*, as in the numerous existing nouns made by this preformative. Some light may be thrown on its meaning by reference to the analogous preformative of the participle, *ma*. This preformative is found in numerous nouns designating the place or time or instrument (*mi*) of an action, in all which the meaning may be accounted for by regarding the *ma* (= *mi*) as indicating 'place' (and so also perhaps the nominal mimation); it is natural, then, to look for the same meaning in the participial prefix, the 'place' being in this case an agent or object. Compare, for the general turn of thought, the Hebrew instrumental use of בְּ, and the expression: "In Isaac will a seed be called to thee," that is, Isaac, as the locus of the calling, is 'the person calling,' the name-giver, the true Abrahamidae will be the Isaacidae (in distinction from the Ishmaelites). This view of the force of the *ma* does not prevent our carrying it farther back to a demonstrative identical with the interrogative-indefinite pronoun. If, now, we look for the uses of the form *ya* in Shemitic, we find it employed to denote the genitive plural (the only plural form found in Hebrew, as *sus.a.yi.k* for *susayak*, Arabic dual *rajulai.ni* = *rajula.ya.ni*), and to form relative adjectives (as *'ibri* 'a Hebrew,' for *'ibri.y.ya*), in both which cases the force of the affix is 'pertaining to'. It is not improbable, then, that the noun *yiṣ.har* ('oil') signifies 'that which pertains to, belongs in the category of, is defined by shining,' *yiṣ haq* (Isaac) 'he who pertains to laughing,' and the imperfect *yaktubu* would mean, 'he who pertains to writing.' Perhaps in this patronymic force of the *ya* lies the reason for its frequent use in proper names. The difference in meaning between participle with *ma* and noun with *ya* would then be that the former represents a thing as agent or object of an action, while the



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latter represents it as belonging to the category indicated by the base, a distinction which (as is often true of original etymological differences) amounts to little or nothing in actual use. Whether this account of *ya* be correct or not, it is plain that the imperfect form *yaktubu* belongs to the class of derivative nouns made by prefixes, and that no significance pertains to the position of the formative syllable that does not equally pertain to other prefix-forms, as, for example, in the participle. It is bare conjecture to say that, while in the perfect the postposition of the pronoun, keeping the substantive base prominent, emphasizes the action as complete, the preposition of the pronoun in the imperfect, giving it the prominent and the action the subordinate place, indicates the incomplete character of the latter. The preceding discussion of the *ya* leads us to deny its position a recognizable symbolic significance, and to seek the distinction in meaning between perfect and imperfect in differences imposed on them by usage. It is also impossible to determine the relative priority of perfect and imperfect from the form of their bases. Though the simple *kataba* may be older than the derivative *yaktubu*, it does not follow that a similar difference of age exists in the verb-forms that sprang from these. The attempt to fix the relation of age between these forms by calling the imperfect an 'aorist,' that is, an indefinite, must equally fail. It is really no more an aorist than the perfect; both are indefinite in their indication of time, their difference of signification and use has at bottom nothing to do with time, and the distinction between them must be sought elsewhere.

The base of the perfect of the Shemitic verb is not an abstract, but a concrete noun. The analogy of many agglutinizing languages has been urged in proof of its abstract character, but such an argument avails little against facts of the Shemitic dialects themselves. On Shemitic ground ingenious arguments for the abstract character have been drawn from the difference in form between the pronominal affixes and the separate pronouns, and between the ordinary verb and the periphrastic Aramaean form, and from an Aethiopic infinitive construction. It is urged that the verbal affixes are

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in the oblique case (the separate pronouns being nominatives), whence it is inferred that *katab.ti* = '(act of) killing of me,' *katab.ka* (the Aethiopic being taken as the original) = 'killing of thee,' etc. But this division of Shemitic personal-pronominal forms into nominative and oblique cases is without foundation. There is no evidence that such forms as *ku*, *ki*, *ka* with case-distinctions ever existed; it is evident that in the third person *hu'*, *hem* acted for all case-relations. In Hebrew, when the oblique pronoun is to be repeated emphatically, it is the separate form (as *'ani* in the first person) that is used. The suffix-pronouns are fragments or components of the separate pronouns. The relation between the difficult *t* and *k*-forms has been discussed above, and, whatever conclusion we come to concerning their origin, we must hold that they are interchangeable and equivalent. So in the Aramaic periphrastic form, which is supposed to establish a case-difference in the pronoun. The late *kotel.no* (= *kātil.na*) 'killing (am) I' is indeed very different in appearance from the ordinary first person singular *kitleth* (= *katal.ti*), not because the *no* (for *ēno* = *ani*) is nominative and the *th* (= *t* = *ti*) is genitive, but because the latter (ordinary perfect first singular) originated at a time when a first person pronoun *ti* (= *tu*) was a living word in the language, while the former (the so-called periphrastic form) came into existence when *ti* had become petrified as verbal ending and *ēno* was the only word that was felt to mean 'I.' The Aethiopic infinitive absolute (*gabir*) is adduced as an instance of the formation of a verb by the addition of suffixes to an abstract noun: thus *gabir.o* (literally 'making of him') in connected discourse signifies 'when he makes.' To make the statement complete, it should be added that this combination of abstract noun and pronoun produces not only a verb, but at the same time a conjunction —an extension of the argument that would probably be felt to be undesirable for the end proposed, but would point to the real nature of this construction: namely, this peculiar use of the Aethiopic infinitive differs very slightly from the ordinary use of the Hebrew infinitive absolute and infinitive construct, from the former in taking a suffix, from the

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latter in not taking a preposition. It is attached to a verb to express some circumstance connected with the main action, and in the language of Indo-European Grammar would be termed an 'abstract accusative absolute' or 'accusative of general reference': *samî'o dangaḍa*, literally 'as to his hearing, he feared,' = 'when he heard, he feared.' The infinitive does not step outside its legitimate nominal character, and there is no question here of the formation of a verb. The form of the perfect rather leads to the conclusion that it is a concrete noun. In the simple stem (Qal) it resembles the participle rather than the infinitive, and in Hebrew verbs middle E and O is identical with the participle. If the above statement of the relation of the various forms of the personal pronouns be correct (*ani* and *ti*, *ta* and *ka*) the Aramaic periphrastic verb would point in the same direction. But apart from this, as the imperfect agrees in the form of its base with the abstract infinitive, so the perfect agrees with the concrete participle. Though there may have been at first such indefiniteness or freedom in the forms *katab* and *ktab* or *katb* that both might equally be concrete or abstract, in the present stage of the verb they have been somewhat distinctly differentiated, and have settled into their respective concrete and abstract significations. With the imperfect the case is different. It is not like the perfect a simple noun, but a derivative made by a prefix to an abstract base, whence there results a concrete noun. As has been above pointed out, each person of the imperfect is a separate compound, which is inflected as a simple noun, and each has the same concrete character as has been established for the third masculine singular, *yaktubu*. The form *katabta*, then, means literally 'writer (art) thou' (='thou writest'), and *kataba*, in which no pronoun is expressed, came to be equivalent to 'writer (is) he' ('he writes'). The plurals *katabu*, *katabān* = 'writers' masculine and feminine (where, as in third singular, it would be difficult to understand the abstract 'acts of writing,' or in third singular feminine *katabat* a 'feminine act of writing'); in the first and second persons the pronoun is attached to the singular form, as *katab.na*, *katab.tum*, not, as in the Aramaic periphrastic forms

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*kotebi.nan*, *kotebi.tun*, to the plural, and = 'writer (are) we, ye,' the simpler singular being preferred in the compound, unless the final vowels have fallen out, as in the second singular masculine, and the present forms stand for original *katabū.na*, *katabū.tum*, *katabā.tunna*, where the length of the vowel makes a difficulty. In the imperfect the plural third and second are made from the singular by the nominal plural terminations, but the first person has a separate prefix, since 'we' is not the plural of 'I,' but = 'I and others.'

The traces of the nominal origin of the Shemitic verb are less obvious in the syntax than in the etymology, yet here also quite recognizable. The syntactical emphasizing of the noun-element in a language is an indication of the extent of its syntactical development, that is, of how far it has carried the differentiation of the idea of the verb, and how far the original nominal conception has lingered in the consciousness of the people. The verb—to define it according to the character it has assumed or aims at in the most highly developed languages—is a complete fusion of action and subject into a conceptional unity, in which the merely relational modifications of the action (temporal, modal and others) are represented by corresponding formal modifications; and the degree of verbal development will be felt in all those constructions in which these modifications come into play. The difference between different languages in this respect is one of degree rather than of essence; those that have the best elaborated verb yet show traces of its primitive nominal character, and it may be doubted whether there are any that fail entirely to distinguish between noun and verb. The Shemitic languages occupy an intermediate position in this respect: they distinguish clearly between verb and noun, but they sometimes emphasize the nominal element of the verb, and a clear apprehension of such cases will facilitate our understanding of the syntax and therefore of the thought of these languages.

First may be mentioned the frequent employment of the abstract noun (infinitive) where we should rather use the finite verb, a usage found in Indo-European languages, but more frequent in Hebrew and its cognate dialects. In curt

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and intense discourse the infinitive absolute is employed instead of the finite verb where the subject of the affirmation is evident from the context (2 Kin. iv. 43), and in the ordinary intensive use of this infinitive, its close union with the verb is shown, for example, by the fact that in a negative assertion the negative stands before infinitive and verb. So the infinitive construct is widely used with prepositions to express relations of purpose, contemporaneousness, etc., where the construction with conjunction and finite verb or with participle is more usual in the Indo-European family of languages. It is also noticeable that this infinitive often alternates with the finite verb in discourse, as in Isa. xxxviii. 9: "In his being sick and he recovered from his sickness" = "When he had been sick and had recovered." With those constructions compare that of the Aethiopic absolute infinitive referred to above. This striking approach of the infinitive and verb has led to the discussion of the question whether the Shemitic infinitive ever transcends the nominal character,* and some writers have preferred to designate the two forms of infinitive as nominal and verbal respectively.† But this usage rather indicates that the Shemitic idea of the verb lingered in the plane of the noun; it is not that the infinitive advances towards the verb, but that Hebrew and its sister tongues sometimes prefer to treat an action as merely substantive, where we prefer to consider it as blended with its subject into a unit of thought. The bearing of this usage on the determination of the character (whether abstract or concrete) of the nominal base of the perfect has been touched on above in connection with the Aethiopic infinitive. The syntactical interchange of finite verb and infinitive might seem to point to an abstract form for the base of the perfect; it only points, however, to the possibility of such a form, and the facts above adduced show that historically the perfect has come from a participle and not from an infinitive. Add to this use of the infinitive the alternation of imperative and finite verb in discourse, the former appearing in sequence where we should

* Koch's *Der Semitische Infinitiv.*

† Dillman's *Grammatik der Aethiopischen Sprache*, p. 209.

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use the latter: "I will bless thee and make thy name great, and be thou a blessing" (Gen. xii. 2). In these constructions the true sense of the language can be gotten only by treating infinitive and imperative in their proper character, not regarding them as improper substitutes for true verbs.

The next fact to be noted is the comparative abundance of neuter verbs and the corresponding paucity of adjectives in Hebrew and the related dialects. A neuter verb is the attribution of a quality to a subject, and by the prominence given to the substantive base stands nearest to the noun and farthest from the fully developed verbal idea. The language has thus thrown its adjectival conceptions into a verbal shape, and has less need to isolate them. In the Indo-European languages even the verbs called neuter set forth an act inhering in the subject ('he sleeps' means 'he performs the act of sleeping,' not 'he is asleep'), and to express simple attribution of qualities they prefer to isolate adjective and substantive verb. But the Shemitic tongues make comparatively small use of the substantive verb, which is the highest generalization of the idea of relation; wherever it is possible they set subject and predicate side by side and leave the relation to be inferred from the juxtaposition. This is the ground of their frequent use of the personal pronoun to mark or rather to call attention to relations, where in most grammars it is loosely and incorrectly said that the pronoun is used for the substantive verb. Rather, the substantive verb is ignored, and the nominal elements of the phrase placed side by side, the reader or hearer being left to discover the relation from the general connection. The phrase: *Dawid hu' ham.melek* (דוד הוא המלך) is literally: 'David, he the king,' where the pronoun is anything but the copula. The paucity of adjectives and the infrequency of the substantive verb are thus correlative phenomena, and are both connected with the relative prominence given to the nominal idea.

In a considerable number of cases the verb in Hebrew is treated *in regimine* as a noun, standing in a distinctly substantival way after the construct state and after a preposition. A good instance of this occurs in the opening expression

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of the Book of Genesis: *be.rēshith bārā' Elohim* (ברשית ברא אלהים), where, the noun *reshith* being in the construct state, the precise form is: ἐν ἀρχῇ τοῦ ἐποίησεν ὁ θεός, 'in the beginning of God created,' (='in the beginning of the time when God created') with the verb *bara'* 'created' in the syntactical position of a noun (the same construction in Hos. i. 2). So in Isa. lxv. 1: *nirdashti le.lo' shāālu* (נרדשתי ללוא שאלו) 'I am sought by they did not ask after me,' where the phrase 'they did not ask' is governed by a preposition. In these sentences we cannot well understand merely the omission of a relative pronoun, as no doubt occurs in some relational sentences; that is, the consciousness of the language did not here supply a relative pronoun, as we often do in English in such phrases as 'the man I saw' for 'the man whom I saw.' The Hebrew distinctly defines the word 'beginning' by the word 'created'; the *bara'* still retained so much of its nominal character in the feeling of the people that it could stand where we should think a noun absolutely necessary. There is a syntactical gradation in these Hebrew relational phrases from the form with relative pronoun identical with our usage through the simpler omission of the relative to the sharply defined constructions above cited. But to regard these latter as merely consciously abridged phrases (they occur in prose as well as in poetry) is to measure Hebrew by our own standards, and to miss the statuesque nominal conception of these constructions. In the English sentence: "Letting 'I dare not' wait upon 'I would'," there is a clear consciousness that the verbs are used out of their natural position, but there is no sign of this in the Hebrew phrases in question.

Finally, the small development of temporal and modal forms in Hebrew may perhaps receive a partial explanation from the prominence of the nominal base of the verb. The Aramaic is the only one of the Shemitic dialects that has produced a tolerably clear distinction of time-forms; this came in part from its flexible and practical character (the Aramaeans were in ancient times the intermediaries between Shemitic and Indo-European culture), and partly, perhaps, from the influence of foreign modes of thought; the Shemitic languages

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generally have almost no time-forms proper. The form commonly called the imperfect (by Böttcher the *Fiens*) is in the present use of Hebrew more exactly an inchoative or ingressive, marking the entering on an action; but it probably goes back finally to the general notion of incompleteness. We must assume the existence in primitive Shemitic of at least germinal modal forms; but these partly died out in Hebrew (as similar forms have disappeared from English), partly from peculiar circumstances were specially developed only in Arabic and slightly in Aethiopic. Moreover, this modal growth took place only in the imperfect, because there alone the idea of incompleteness naturally connected itself with ideas of dependence, uncertainty, etc. May it, then, not be that the distinct consciousness of the nominal base of the verb, in Shemitic generally and particularly in Hebrew, led to the development of substantival rather than modal modifications of the verbal form—to the derived stems rather than to conjunctive and optative, to forms expressing degrees of completeness rather than to tenses? The derived stems all express modes of the performance of the action itself, intensive, causal, reflexive, but not modes of existence of the action; they pertain more naturally to the contemplation of the action as a substantive thing, they express actual modifications in it, while the mood proper indicates changes, not in the act itself, but in the mode of conception of the speaker or writer. So also the distinction of completeness and its opposite is an objective quality of the action, while time is merely the sphere in which the action takes place. If such were the development of the Hebrew verb, it does not follow that all languages must have followed the same course. It is possible that some languages, emphasizing the nominal verb-base in some points, may in other points have wrought out the full idea of the verb more or less completely; each language will have its own direction of growth, as for example, Sanskrit falls far behind Greek and Latin in its modal forms, and the Vedic subjunctive was not retained in the later period of the language. So among the Shemitic languages there are various differences in the lines of growth. Hebrew has worked out a system of sequence in



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verb-forms that is as fixed and often as mechanical as its time-system is defective. The phenomena of sequence (which it is not our purpose here to discuss) grow out of the signification of the perfect and imperfect, and thus, if the above view be correct, out of the distinct consciousness that the Hebrew retained of the nominal origin of its verb.

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## III.—*On a Certain Apparently Pleonastic Use of* ὡς.

By FREDERIC D. ALLEN,

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In many expressions ὡς seems to be used superfluously, but this is notably so in ὡς ἄλλως and ὡς ἑτέρως, 'otherwise.' Grammarians explain the origin of these phrases as best they can, generally regarding them as abbreviations of longer phrases containing a comparison. So Kühner (Ausführliche Grammatik, vol. ii., p. 921) says that ὡς ἄλλως stands for οὕτως ὡς ἄλλο ἐστίν, 'thus as something else is'; and similarly ὡς ἑτέρως. Krüger speaks to much the same effect, and so does Klotz in his Devarius. Liddell and Scott consider the ὡς as used to "strengthen" the positive; others have thought of the exclamatory ὡς in this connection ('oh, how differently!'). In short, the whole matter is highly nebulous. Yet it is not hard to see what was probably the origin of these expressions, if only one is willing to look a little below the surface of things.

The adverbs in -ως are, as I suppose every one now knows, the old ablative singular. This case ended in Sanskrit in *-at* or *-āt*, in old Latin in *ēd* or *ōd* (feminine *ād*), and in pre-Hellenic Greek in -ωτ. This final *t*-sound, as it could not maintain itself in Greek, was either dropped (as in οὕτω, ὦ-δε), or changed to σ (as in οὕτως, σοφῶς, δικαίως). Now these phrases ὡς ἄλλως, ὡς ἑτέρως, are, as I take it, nothing more or less than the ablatives of ὁ ἄλλος, ὁ ἕτερος, petrified—if one may so express

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it—into adverbs. This ὡς is not the ordinary ὡς, adverb of the relative ὅς, but is here the adverb of the article ὁ, and the two adverbial ablatives ὡς and ἄλλως agree together just as the genitives τοῦ ἄλλου, or the accusatives τὸν ἄλλον. Ὡς ἄλλως, ὡς ἑτέρως meant originally 'in the other manner,' and so find a simple and rational explanation. For example, Dem. Cor. § 85; ἐάν τε καλῶς ἔχῃ, χάριτος τυγχάνει, ἐάν τε ὡς ἑτέρως, τιμωρίας, "If matters are in a good condition they meet with approval, if *in the opposite condition*, punishment."

Just the same use of ὡς lies even more clearly before us in ὡς αὔτως (written ὡσαύτως or separately ὡς δ' αὔτως). This is the ablative of ὁ αὐτός, and means 'in the same manner,' 'just so.' The recognition of this simple fact ought to give the form αὔτως its final quietus. This spelling was defended by Elmsley and others on the ground that the adverb came from οὗτος, or rather from the feminine αὕτη; and it still finds some adherents, even among such men as August Nauck. Supposing the adverb to come from οὗτος, then what can ὡς αὔτως mean? Just as little as ὡς οὕτως, and that would be as bad as ὁ οὗτος.* But even aside from this, the idea of an αὔτως from οὗτος is untenable. In the first place, οὗτος has already its properly formed adverb in οὕτως, and a separate adverb in -ως formed from the feminine would be a thing unheard of. Secondly, the forms of οὗτος all require their first syllable to conform to the last in color, and an αὔτως (even if formed from the feminine) would be as impossible as a genitive plural ταύτων. The right derivation is certainly from αὐτός; and the right spelling αὔτως. The accent is thrown back; this is an irregularity, of course,

* Professor T. D. Seymour, since this paper was presented, has kindly called my attention to the following paragraph of Buttmann's Lexil. vol. i., 13, 7: "Nach dieser Schreibart [ὡς δ' αὔτως] wäre also diese Form eine Häufung der Demonstrativa ὡς und αὔτως, welches kaum denkbar ist, so wenig als im adjectiven Sinn ὁ οὗτος oder ὃς οὗτος jemals den Begriff *derselbe* hatte. Aber eben aus dieser Erwägung ergibt sich von selbst, dass da im adjectiven Sinn gesagt wird ὁ αὐτός, das natürliche Adverb davon ὡς αὐτῶς sein müsste. Ich glaube es bedarf nur dieser Darlegung um gewiss zu machen dass dies der wahre Ursprung jener zusammengesetzten Partikel ist, da denn die übliche Betonung und Behauchung in ὡσαύτως, ὡς δ' αὔτως durch eine Verwirrung in die Form αὔτως entstanden sein muss." I had entirely overlooked this passage, and did not remember that any one had ever suggested the (to me very obvious) connection between ὁ αὐτός and ὡσαύτως.

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but not an unparalleled one. Bekker, it may be observed in passing, writes αὐτῶς in Homer, in the phrase ὡς δ' αὐτῶς, on his own authority.

It seems then clear that in these three formulae, ὡς αὔτως, ὡς ἄλλως, ὡς ἑτέρως, ὡς is the ablative of the article ὁ, and so differs in origin from the ordinary ὡς from ὅς. Whether this could be carried still further, namely so as to cover expressions like ὡς ἀληθῶς, ὡς ἑτητύμως, ὡς ἠπίως, I do not venture to say.

Only one point more. One might object that the ablative of the article ought, like the other oblique cases, to come from the *t*-stem (το-), not the *s*-stem (ὁ- = *sa*-), so that τώς (or rather τῶς) would be the normal form, corresponding to τῶν, τῷ, τόν. Now as τώς (Bekker τῶς) already exists in Epic, a second adverb from the *s*-stem might be thought improbable. But the employment of the same *s*-stem for the ablative adverb is certain in other pronouns. Ὅδε is a compound of the article and δε; and the adverb of ὅδε is ὧ-δε, not τῶ-δε. So οὗτος, which has a compound stem, made up of the article-stem along with another (υτο-), has the adverb οὕτως, not τούτως. Just so the simple ὁ makes ὡς, alongside, it is true, of τώς. Ὡς and τώς exist side by side, like the nominatives plural οἱ and τοί (both Homeric), and the Sanskrit locatives *sa-smin* and *ta-smin*.

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## IV.—*On the Relation of Surd and Sonant.*

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What is the real, the essential, difference between the members of such pairs of related mutes as *p* and *b*, or of such pairs of related fricatives as *f* and *v*, or as *s* and *z*—this is one of the vexed questions of the modern science of phonetics. One may fairly say, of the *modern* science; for in the only ancient school of phonology that has any claim to be called scientific, the Hindu, the question would seem never to have arisen: the old grammarians of India were as accordant respecting it as their successors have been at variance.* For some time past there has seemed to be a decided tendency toward agreement, in the prevailing acceptance of the view that *b* and *v* and *z*, and their relatives in other like pairs, get their distinctive character from the circumstance that their utterance is accompanied by sonant vibrations of the vocal chords—that they are sonant, or intoned, or voiced, or phthongal, or by whatever other name we may choose to denote that property; while the others are surd, or toneless, or breathed, or aphthongal, or anything else equivalent to this. But very recently there are signs of a reaction, of a cessation or reversal of the unitary movement; and this seems to render desirable a brief further discussion of the matter.

One satisfactory result of the investigations made has been to establish the fact of an actual difference between different communities as regards the mode of utterance of sounds written with the same signs and hence generally assumed to be identical, and to show that this difference is accountable for at least a part of the misunderstanding between phonetists of diverse nationality. It has come to be generally acknowledged, even by German students of phonology, that the English (and French, and other: it is not needful for the

*The Sanskrit names for 'surd' and 'sonant' are *aghosha* and *ghoshavant*, literally 'toneless' and 'possessing tone,' respectively.

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purposes of this inquiry that we determine how widely the mode of utterance referred to prevails) *b* and *v* are made to differ from their corresponding surds* by the accompaniment of tone or sonant vibration.† And, on the other hand, phonetists to whom English is native have been made to see that in a large part of Germany, at least, there is no such distinction of sonancy and non-sonancy, but, instead of it, a difference in respect to force of utterance: the *b* and *v* and their like being weaker, and the *p* and *f* and their like being stronger. To those who do not admit these two premises, the present exposition is not directly addressed. We will take their truth as demonstrated, and proceed to occupy ourselves with the mutual relation and the comparative value of the two kinds of distinction thus defined: are they independent, or are they necessarily combined with one another? and, in the one case as in the other, does either of them furnish the superior and more characteristic element of discrimination?

We to whom the distinction truthfully expressed by the words *surd* and *sonant* is a native and familiar one need make no difficulty about conceding to the other party—skilled phonetic investigators as they are, and armed with instruments

* The name *surd* is used for convenience's sake, without intended prejudice to any other word that shall mean practically the same thing; only those terms need to be objected to which either distinctly imply and suggest a wrong theory, or are, like *flat* and *sharp*, absurdly fanciful and meaningless.

† Not that there are not still some dissidents. Among such may be mentioned with respect and regret the recently deceased Professor Grassmann, who, in an article (*Annalen der Physik und Chemie*, N. S. i. 626) which must have been almost or quite the last that came from his pen, expresses himself thus: 'The soft explosives have been erroneously viewed as sonant utterances; an actual tone is never formed in them; for, if that were the case, one would necessarily be able to produce a melody with *b*, for example, without adding a vowel: which is impossible.' To this argument it is a full and sufficient reply that the asserted impossibility does not exist; the sonant accompaniment by which *b* is distinguished from *p* is perfectly susceptible of variation of pitch, and hence a tune can be sung to *b* alone—not, of course, without a degree of awkwardness and difficulty determined by the circumstances of the case, and exceeding that of performing the same task to a *v* as this exceeds that to an *l* or *n*, or this that to a full vowel; the sonant element of the *b* can be maintained only till the cheeks are distended by the breath forced through the vibrating chords into the closed cavity of the mouth; and to finish the tune the breath must be returned to the lungs, or 'swallowed,' and used again over and over.

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which help to furnish the means of exact determination—that in the central and southern German *b* and *v* there is an utter absence of sonant vibration of the vocal chords. And we may do this the more readily, since (as will be more particularly noted further on) the German distinction is to our ears so different from our own that we hardly perceive it to exist at all.

But there is also no reason why we, on our part, should not be acknowledged fully competent to determine whether into our distinction of *p* and *b*, of *f* and *v*, and the rest, there enters any element of a difference in respect to force of utterance. Not a few points in phonetics, indeed, are so obscure and subtle that we may question the right of any one to settle them authoritatively. But this is not one of them. There is no manner of difficulty in applying here a sufficient test. If we are able to utter each of the two sounds in any of these pairs with every desired degree of energy, without a shadow of detriment to its identity in the one case (for instance) as *b* and in the other as *p*—if we may in the same word, in two successive syllables or in the same syllable, utter a *p* that is less forcible than a *b*, or *vice versâ*, without exchanging or assimilating the two sounds, or at all defacing their distinction: then there is no good ground on which any one can claim that our *p* and *b* involve, along with their difference of sonancy, a difference also of force of utterance—so that the sonant mute is also properly to be called a "soft" or "weak" one.

And that such is the case may be asserted in the most positive manner, and without any fear of successful contradiction. To those who after the manner of English speakers make their *b* sonant and their *p* surd, it is of not the smallest consequence, as bearing on their full distinction and the unimpaired identity of both, whether either of them be strong or weak absolutely, or strong or soft relatively, as compared with the other. We may set our lips and drive out our intoned breath as violently as we will, and the product, if the contact be broken during the emission of the intonation, will be an unmistakable *b*, with no suspicion of a *p*-character.

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And, on the other hand, we may bring the lips into the gentlest possible contact, and part them so softly that the explosion is next to inaudible; and, if the intonation do not begin until the parting, the result will be a *p*, with no glimmer of a claim to be called anything else. We may intensify the accent of *beetle*, strengthening the accented syllable and weakening the unaccented, to any extent, and it will not sound to us one particle more like *peedle* than in its ordinary utterance. Or (if the telling character of the illustration may be allowed to excuse its want of dignity), the Englishman may utter his (asserted) pet exclamation of *god-dam* with all the explosive energy that he can put into it, in his mood of highest indignation and wrath, and it will not verge one perceptible particle toward *cot-tam;* the least change in that direction would be an infallible *shibboleth*, convicting the utterer of being no Englishman, but a masquerading "Dutchman." I may safely appeal to every native English speaker whether this is not so. And if it be so, it is not less futile to ascribe to the English surds and sonants a distinction of force than to ascribe to the German strong and weak sounds a distinction of sonancy.

Of course, the question whether *b* essentially, or even at all, differs from *p* by an inferior degree of force of articulation is independent of the question whether *b* frequently, or usually, or regularly, comes in the history of phonetic changes of language from *p* by one of those processes to which we are accustomed to give the name of "weakening." Let us for the moment admit that it is so; that the *b* is historically a product of the "weakening" of *p* (the point will be further considered later). But that is quite a different thing from saying that *b* is phonetically a weak *p;* that adding or subtracting energy of articulation will turn either of the two sounds into the other. Just so the *f* is a weakened successor of *p* in the history of sounds; growing, in a manner much more evident and undeniable, out of a relaxation of the contact which makes the *p*; but, once developed and taken cognizance of in the consciousness of the language-users as another sound than *p*, and not to be confounded with the latter, it acquires

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an independent value, as an element having its own articulating position, just as the *b* has its characteristic resonance; and it becomes, in its turn, capable of utterance with every degree of force, like the *p:* energy of expulsion does not remand it to the *p*-condition. And so, among the vowels, an *i* is on a large scale product of the "weakening" of an *a;* but, having won its own place and mode of articulation, it is not less tenacious of its identity than is the *a:* both are utterable with the same variety of force, and of pitch, while still remaining to all intents and purposes the same sound. The same thing is widely true among the constituents of our alphabet. Only the minority of our sounds are original, even within the cognizance of our imperfect science; the rest did not exist in the early Indo-European language, and have been developed out of the more original ones, in the phonetic history of its various branches. But in their present condition all are alike stable; there is not an element in our spoken system, vowel or consonant, which does not admit all the degrees of stress or emphasis, and all the degrees of pitch or modulation, that the most varied expression demands, without any detriment to its identity.

This characteristic of our English spoken alphabet, however, need not belong equally to the alphabets of all languages. It is not impossible, in the first place, that differences of pitch and tone-inflection should be raised to the value of alphabetic distinctions, aids to the discrimination of meaning: they are, in fact, so raised in the Chinese and other monosyllabic tongues, whose scanty resources of expression are notably increased by them. In like manner it is possible that differences of stress should be turned to account: and they appear to be so employed in a part of the Germanic dialects—just how widely, and whether at all outside of the Germanic branch, is not at present determined; nor is the question a practical one to us. But it would seem to be just as reasonable that a Chinese phonetist should attempt to trace his significant modulations of tone as inhering in our vowel-utterance and constituting an essential element in our connection of sound with sense, as that a German phonetist should claim to discover



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his native distinctions of force in our surds and sonants, as co-existing with the distinction of sonancy and being an element of a higher and more essential order. The two modes of distinction may be combined, but they are not mutually dependent; there is even no limit to the variety of their combination: either extreme of the one kind may go along with either extreme of the other; there is nothing to forbid the surd letter being habitually made weaker than its sonant correlative—whether there be actually in existence a language of such habit is a question of another kind.

In truth, the difficulty of correlating the two kinds of distinction so as to make sonancy depend on a diminution of stress is to me insuperable. Sonancy is emphatically a *plus* element. It is the effect of a definite muscular action which is not negative, but positive. It does not come in insidiously as the result of a relaxation, a letting-go of anything anywhere. It is only brought about by setting in action a separate piece of muscular apparatus, which in surd utterance lies idle: the vocal chords have to be so adjusted and stretched that the air forced through them shall set them in vibration. A diminution of muscular effort, so far as this apparatus is concerned, tends to the reduction of the glottal aperture to the position of breathing, and so to the substitution of surd utterance for sonant. And that a falling-off of articulating energy in the oral organs should have any tendency to awaken a compensating activity in the larynx is what has yet to be demonstrated, or even made probable. It is indeed true that, the chords once narrowed to the sonant position, their closeness makes the column of expelled air a thinner one. Precisely how in the case of the mutes this is to be balanced against and combined with the complete closure at the mouth might not be easy to determine (even if its determination were a matter of consequence); but in the fricative pairs, as *f* and *v*, it may be allowed that the glottal closure thins the current that is seeking exit at the labial closure, and perhaps makes the expenditure of breath less in the *v* than in the *f*. And, so far as I can see, this fact and its analogue in the other pairs is in no small measure at the bottom of the whole misunderstanding as to the "strong"

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and "weak" character of the two classes of sounds. To raise it, however, from its subordinate position as a consequence to the rank of an independent element in phonetic history is certainly a very grave error. For, in the first place, it is out of harmony with all that we know respecting the mode of action of the organs of utterance to regard them as ever finding out by experience that a waste of breath at the mouth can be diminished by a tension of the vocal chords, and so resorting to sonancy as an economical device. And, in the second place, the object does not appear to be one that is made any account of, or that is sought after. In the economy of speech, nothing is so cheap as breath. The mutes, for example, turn to fricatives, regardless of the breath that runs to waste through the opened organs. The *s*, *kh*, and so on, turn to *h*, which, as singers well know, squanders breath faster than any other audible sound.

So far as appears, then, there is no connection between a weakening process and the addition of tone to an expiration of breath made with a given position of the mouth-organs. There is no homology between a sound which differs from *p* by a weaker articulating effort, and a sound that differs from *p* by sonant vibrations of the vocal chords during the labial closure. One of these two may, in the course of the phonetic history of a language, become a substitute for the other, but it is in no sense the same thing with the other. The English *b* and the South-German *b* are so unlike that they have no good right to be called by the same name and written by the same sign; if they are so called and written, it is only by one of those concessions to popular convenience of which the history of writing is full. That the popular ear does not apprehend the two distinctions as equivalent is abundantly shown by the fact that, to a native English speaker, a German in general is distinctively a man who cannot keep his *p*'s and *b*'s apart: what the latter gives as a *b* does not seem to the former to be any such sound. English caricature is full of illustrations of this: it forms, for example, no small part of the fun of the Breitmann ballads.

If, now, it can be shown that the Indo-European and the

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Sanskrit-Greek-Latin *b* or *d* or *g* was not a sonant counterpart of the *p* or *t* or *k*, but rather a weakened counterpart, then the South-German *b*-sound has the real right to the name *media* and the sign *b*. But if the sonant utterance is the older and more original one, and the weaker utterance without sonancy a later and locally restricted substitute for it, then the name and sign belong properly to the English *b* alone, and not to the German, which ought rather to be called a weak *p*.

There is perhaps little in the above exposition which has not been said plainly enough before, and which would have any need to be repeated if contrary or dissenting views were not still widely prevalent and demanding to be opposed. And it may be found useful, as bearing on the general question in controversy, that we take up some statement and defense of a dissenting view, and examine whether there be that in it which shall prove able to drive us off the ground we have taken. I choose for this purpose the manual of phonology recently published by Professor Sievers of Jena as introduction to a series of Indo-European grammars,* because the high value of the work as a whole makes it particularly worthy of attention, and also renders dangerous any error on its part in reference to this particular point. Professor Sievers is far from holding the long-rooted and still too common error that sonancy and weakness are virtually convertible terms, that the sonant letter of a pair is a weakened surd; on the contrary, he fully acknowledges the separateness, if not the entire independence, of the two modes of distinction. But he embroils the whole question, as it seems to me, by setting up a peculiar character of superiority as belonging to the distinction of force; and so conducts his argument as to give not a little comfort and seeming support to the adherents of the old false doctrine. Indeed, if I did not feel that his exposition amounted to a partial revival and rehabilitation of that doctrine, I should hardly have undertaken a new discussion of the subject.

After describing the distinction of surd and sonant utterance, as shown both in mutes and in fricatives, Professor Sievers

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**Grundzüge der Lautphysiologie, zur Einführung in das Studium der Lautlehre der indo-germanischen Sprachen, von Eduard Sievers.* Leipzig, 1876.

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proceeds to point out (p. 64) that the progress of phonetic observation has shown many languages and dialects—for example, those of Central and Southern Germany—to possess a distinction of *tenuis* and *media* (mutes), without ever giving to the latter a sonant character; the same thing being true also of the *s* and *z*. Further, the fact cannot possibly escape recognition that an entirely similar (*ganz ähnlicher*) distinction prevails among the liquids and nasals, which are, of course, always sonant. As examples of this last distinction, he cites the German words *alle* and *Ahle*, *Amme* and *ahme*, *Amt* and *ahmt*, in each of which pairs we are to understand that the second word exemplifies the weaker sound. Fortunately for us English speakers, who might otherwise be left in doubt whether our want of native familiarity with the German does not disqualify us for apprehending the true nature and value of this distinction, he goes on to bring up English examples of a wholly analogous (*ganz analoger*) difference: it is seen, he tells us, between the *z*-sounds of *puzzle* and *measles*, or between the *zh*-sounds of *measure* and *glazier*. All this, he concludes, lays upon us the necessity of setting up a superior principle of division other than and above that of the presence or absence of sonant tone.

Now we may well enough admit that there is a difference in force of articulation between the *z*-sound of *puzzle* and that of *measles*—although there are very few, even of practised phonetists, who have ever become aware of it, and not one in a hundred or a thousand of ordinary speakers can be made to perceive it, even when carefully pointed out to them. And there is no difficulty in noting the cause of it. The alleged weaker sound in the English words quoted (as also in the German ones) is preceded by a long vowel; and a long vowel is by the nature of its utterance a *diminuendo* sound:* it begins stronger than it ends. The *z* of *puzzle* shares in the more energetic utterance of the short accented vowel which it follows; the *z*-sound of *measles* begins already to show the weaker utterance of the unaccented syllable to which it

* This was pointed out, for example, by Mr. H. Sweet, in a criticism in the London Academy for Sept. 30, 1876.

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belongs. After all, however, the particular way in which the difference, such as it is, is brought about, is a matter of comparative insignificance; the important points are: 1, that it is not a difference which attracts the notice of its speakers; not one that has ever served, so far as we know, or that ever would serve, so far as we can judge, as the foundation of a distinction in writing; and 2, that it is liable to be exhibited by the surd sounds as well as by the sonant. There is precisely the same difference between the *s*-sounds of *bluster* and *oyster*, or the *sh*-sounds of *usher* and *nation*. Or we may go into the mutes, and find it again between the sonants of *babble* and *fable;* and equally between the surds of *apple* and *maple*. It is, in short, nothing more than one of the modes of exhibition of the capacity pointed out above as belonging to every phonetic constituent of a language like ours—the capacity of being pronounced with every grade of force, as with every variety of pitch, without detriment to its identity. There still remains the unassailable fact that the strengthened sonant of *puzzle* or *babble* is in no measure assimilated to a surd, and made liable to confusion with even the weakened surd of *oyster* or *maple;* the contrast of *puzzle* and *oyster* is not less absolute and unmistakable than that of *measles* and *bluster*. The presence or absence of sonancy is all that makes the difference to our apprehension; increase or diminution of force has nothing to do with it. Possibly we, in our turn, may be called upon to acknowledge that to the South-German the difference of stress, being that which his ear is accustomed to note, is more conspicuous and important than that of sonancy; whether this is actually the case, I do not know; if true, it would only be the other part of the demonstration that the two forms of distinction are wholly independent of one another, and combinable and appreciable according to the habit of speaker and hearer. How the circumstance that sonant mutes and fricatives, like sonant nasals and semi-vowels, are pronounceable with varying force should compel us to find between sonant and surd mutes or fricatives a higher distinction than their respective surdness and sonancy, is by no means apparent; on the contrary, the inference wears every aspect of being a

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*non-sequitur.* But let us go on to see how and with what result the matter is followed up in the next paragraph.

Our author proceeds there to mention an attempt made by Kräuter to establish as the desired higher principle of division a difference of quantity, the latter holding that the South-German so-called *mediæ* are briefer than the *tenues.* To this he objects that the length of a consonant is obviously nothing primary, but only a consequence of the degree of energy which is imparted to the expiration—'as then this latter is, taking everything together, the most primary conceivable factor (*der denkbar primärste Factor*) in the whole process of sound-formation.' I find it difficult to admit the justice of either part of this claim. In the first place, a more energetic consonant does not seem to be necessarily a longer one. But that is a point of only minor consequence. What is much more important is that I am wholly unable to see why strength of expiration is the factor of highest rank in utterance. I should have said, rather, that it is the factor of lowest rank; it is the one which may vary most with least effect upon the identity of any sound in the alphabet. If we are to establish a scale of rank among the elements that go to make up articulate sounds, we cannot well help giving the first place to the position of the articulating organs; here is (notwithstanding a certain degree of variableness) where the smallest change tells most upon the characterization of the sound. Next to it I should put the differentiation of the expelled current of breath as sonant or surd; the alteration which converts mere breath into resonant tone, modifying the very nature of the raw material converted by the will to the uses of speech, seems decidedly to excel in 'primariness' the difference of a greater or less rush in the current of either sort; it is the difference which, in the closer or consonantal division of our alphabet, and of alphabets generally, produces results in the distinction of sounds which are next in importance to those coming from change of position of the organs. Then would follow the quantity of a given utterance; this (often along with other attending modifications) makes the distinction of long and short vowels, which is a conspicuous element in the

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phonetic structure of the majority of languages; among the consonants, it distinguishes the "double" from the single utterance. At the bottom of the list, and together, would be ranked variation of pitch and variation of force, because in most languages these are comparatively unessential elements, not standing upon the same plane with the rest, serving the uses of rhetorical effect rather than of significant distinction—doing so to no small extent even in those exceptional tongues which, like the South-German, have in certain cases given a significant value to force-distinctions, or which, like the Chinese, have given a like value to pitch-distinctions. I am at a loss to see on what possible grounds this order of importance can be reversed. Professor Sievers says nothing in explanation or defense of the preference he gives to the most external and arbitrarily variable of the elements of utterance; his expression 'conceivable' (*denkbar*) seems to have no other value than a purely subjective one, signifying 'according to his way of thinking'; and we have a perfect right to think otherwise, if we can give good reasons for so doing. If his conception is the true one, then, for all that I can see, an accented *a* differs from an unaccented by a higher and more essential difference than that which separates an *a* from an *e* or *o*, and so on. Perhaps Professor Sievers may be able to give his view an interpretation which will save it from this *reductio ad absurdum;* but, with the best will, I am unable to discover how this should be done. With just as good reason, it seems to me, might a Chinaman claim that pitch be acknowledged the most primary element in utterance, and that hence an *a* of high tone be regarded as more essentially different from an *a* of low tone than an *a* from an *e*.

Professor Sievers then proposes that the stronger and weaker utterances should be distinguished from one another as *fortes* and *lenes*. To this no reasonable person can object —provided, on the one hand, we are not called upon to hold that the distinction of *fortis* and *lenis* has something about it which is higher than that of surd and sonant; and provided, on the other hand, the names are not asserted to be applicable to the *p* and *b*, and the other similar pairs, of languages like the English.

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The question of nomenclature Professor Sievers goes on further to discuss. He refers first to the long contest as to the existence of surd *mediæ*, and expresses his opinion that no harmony of view can be reached unless we throw overboard the old definitions, generally won by an *à priori* method. 'Whoever (he says) starts off, as Brücke does, with the asser tion that a *media* is only a mute with accompanying resonance, must of course declare everything a *tenuis* that lacks the element of tone. As a matter of fact, however, there are in existence languages, like the Swiss and many South-German dialects, which set over against a wholly unaspirated *tenuis* a sound which is equivalent to a North-German *media* in all respects except in its lack of sonancy, and which, accordingly, justice and propriety require us to include likewise in the class of *mediæ*.' The question in dispute is, in fact, partly a verbal one. Shall we apply the same name, *media* (or any other), to these two different classes of sounds, which by different languages are set over against the normal *tenuis?* If we are to call the one kind of correlatives *fortis* and *lenis*, and the other kind surd and sonant (or something equivalent to this), it may be convenient to use for the two together some such terms as *tenuis* and *media*, which are in themselves tolerably insignificant, and may bear well enough this conventional application. Only we must do it with our eyes open, knowing and acknowledging that we are thus putting together under the name *media* two very different things, of which, if one is indeed what in old time had the appellation, the other certainly is not. If, on the other hand, Brücke, believing that the name did originally signify only one of the two things, refused to give it to the other also, it does not seem as if any one has the right to blame him. To one with an English speaker's appreciation of the distinction of surd and sonant, the imputation of a violation of justice and propriety against those who will not allow the South-German *lenis* to be a *media* also, when it differs *only* in lacking the element of tone, will not seem very damaging. It is as if one were to complain of the unreasonableness of those who refuse the name of *aspirata* to the later Greek spirants φ, θ, and χ,



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urging that these sounds still stand opposed to *tenuis* and *media*, like their historical predecessors, and *only* differ from the latter in lacking the element of mute closure. Everything depends on the alphabetic importance of the lacking element in each case; and to the element of sonancy it appears clearly that Professor Sievers is very far from doing justice.

He himself proceeds in the immediate sequel to touch upon the other, the historical aspect of the question, as follows: 'So much as this may perhaps be granted to Brücke, that these non-sonant *mediæ*, at least in the languages just named, have in only comparatively recent time been developed out of sonant ones, and that perhaps the less force of expiration or explosion of the *mediæ* in distinction from that of the *tenues* is, historically regarded, actually a secondary consequence of the narrowing of the glottal aperture.' But nevertheless, he is of opinion that as concerns the weak surd spirants the historical progress may, likely enough, be shown to have been in just the opposite direction. And, at any rate, he concludes that a mode of distinction which in many languages does not exist at all, and which therefore for others can be only accidental, must by no means be raised to the rank of highest principle of division. Now this last is just the consideration which we should turn against our author's undue exaltation of the value of force-distinctions. It is a poor rule, the saying goes, that does not work both ways. Professor Sievers uses it for his purpose thus: since there are languages in which the distinction of *media* from *tenuis* does not involve a difference in regard to sonancy, therefore this difference can only be regarded as one of secondary order. There is nothing to prevent our retorting thus: since in the great majority of known languages the distinction of *media* from *tenuis* does not involve a difference of force, therefore, *à fortiori*, this difference is not one of primary value in the relations of the two classes of sounds. Variation of force is, indeed, an element in the utterance of all languages; but so is also variation of pitch; so is the distinction of surd and sonant expiration; so are a great many other things which do not

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on that account rise to primary rank as characteristics of every sound in which they appear. Pitch and force, on the whole, serve only the rhetorical purposes of expression; they help to add the emotional element to the intellectual, the individual and personal to the general and conventional; their application to the other use is so rare as almost to deserve to be called sporadic, not merely "accidental"; and it seems very strange to find either of them set up as primary in the highest conceivable degree.

Nor, on the other hand, is the principle that factors of utterance are to be ranked according to their universality one that is in any way calculated to command our assent. What is gained for phonology by setting up an absolute scale of superiorities and inferiorities by which phonetic elements shall be judged? Different languages have their own idiosyncrasies, and different classes of sounds their own susceptibilities. Each element is to be estimated according to the effects it produces in the languages which make use of it and the sounds into which it enters. The French nasalization of a vowel-sound is a rather unusual element; but it is very sharply characteristic where it shows itself. The South-African clicks* are yet more exceptional; but in the alphabetic sounds constituted or accompanied by them they are the dominant constituent. We have only to note once more, as a fact past all reach of successful question, that the *media*, the sonant letter of every class, in English and the other languages that are like English, is not in the least affected by changes of force, but admits as many degrees as a vowel, in order to see that all attempts to subordinate sonancy to force must break down helplessly. And if, as Professor Sievers acknowledges, there is good reason to believe that the distinction of the original Indo-European tongue was, as that of most of its descendants has continued to the present time, one of sonancy and its absence, for which the other is a recent substitute, a local usage, limited to a

* With regard to these sounds, I would point out in passing, Professor Sievers commits the error (p. 17) of defining them as made by drawing in the breath. In fact, clicks, like kisses, have nothing to do with the current of breath; they are made by mouth-action only, by suction.

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part of one subdivision of one of the branches, then how useless the attempt to raise the latter to a first-rate position in the classification of sounds! If the historical aspect of the case can be reversed, let it be so, and we will modify our opinions accordingly as to the *historical* relation of the two modes of distinction. But even then their *phonetical* relation would remain what it is now: the sonant letter would be liable to the same varieties of force, as *fortis* or *lenis*, with the surd, having always its constant distinction from the surd in the vibrant tension of the vocal chords—along with whatever difference of expiration comes as a consequence of that tension, and of the accompanying reduction of the glottal aperture.

After this examination of Professor Sievers's view and of the arguments by which he supports it, I think we have the right to adhere to the position taken at the outset, that the distinction of sonancy is wholly independent of that of force, and to pronounce both useless and harmful any attempt to prove the superiority of the latter to the former. It is not only untrue that a sonant *b* is a weak *p*; it is also untrue that there is any element of weakness in the *b* which helps, and in a *de haut en bas* way, to make it different from the *p*.

There is hardly an error, if there be even a single one, in the whole domain of phonetics, which has done more harm than the current misapprehension of the relation between surd and sonant. It has shown itself most conspicuously in the discussions of Germanic word-history, appearing *ad nauseam*, for instance, in the innumerable attempts at explaining Grimm's Law: every experimenter at that so fascinating and baffling task is wont to be sure of one element, at least—that he can turn a surd *p* into a sonant *b* by the simple process of "weakening"; or *vice versâ*. To me, no theory which contains that element is worth a second serious look; it is wholly vitiated and worthless. But the same error runs more or less through the greater part of current Indo-European comparative philology; and damages it in just that measure.

That, in the history of phonetic changes, a surd more often becomes a sonant than a sonant a surd, and that a sonant is

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more easily dropped altogether than a surd, are both doubtless true. For the former fact a sufficient reason can be easily given. The leading method by which the alterative forces act in phonetic history is assimilation; and the great preponderance of the sonant elements over the surd in continuous speech (in Sanskrit, for example, more than three to one; in English nearly four to one) makes the process of conversion of surd to sonant the more frequent. In all the ordinary movements of utterance, a *p* becomes *b* when its position and the habits of its speakers favor the introduction into it of the element of sonancy; and the *b* is just as ready to become *p* in the opposite case; but the former conjuncture occurs oftener than the latter. It is no weakening process that turns the English inflectional sign *s* (of plural in nouns, or of third singular in verbs) into *z* in *heads* and *eggs* and *rubs;* and it is just as far from being a strengthening process that turns the English preterit-ending *d* into *t* in *dropped* and *flocked*, in *wished* and *puffed*, and their like.

As regards the other point, the inferior persistency of the sonant consonant, I will not at present pretend to offer an explanation of it. If it be true that in the general history of speech the sonant alone practically exhibits a tendency to sink, as in German, to the position of weaker utterance—that is, of a surd *lenis*—then the two facts may possibly hang together. But the actuality of the suggested relation is not to be assumed as a matter of course. Since the sonant is capable of energetic utterance, and the surd of weakened utterance, without damage to their identity, there appears to be no reason in the nature of things why, in the practice of another people than the South-Germans, the surd mute should not sink into the condition of a *lenis*. To points like these the attention of investigators may be directed with the hope of valuable result; no such result is within reach so long as it is held that changes in the force of utterance will have as their natural effect interchanges of surd and sonant.

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## V.—*On the Vocabularies of Children Under Two Years of Age.*

By EDWARD S. HOLDEN,

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Some time ago,* I made an examination of my own vocabulary, in order to see how many words were at one's command in ordinary speaking and writing.

I desired to do this, as I was sure that the ordinary statement that "the vocabulary of an intelligent adult is about ten thousand words" was quite inaccurate.

This I supposed to be the case because ten thousand *technical* words is by no means a large vocabulary for a specialist in many branches of science. A chemist, a geologist, a botanist, a zoölogist, has frequently command of many thousand mere *names*, which if added to the vocabulary which he possesses in common with unprofessional persons of his own rank in life, will bring the sum total of the words at his command up to a very high figure.

To avoid misunderstanding, I defined *a word* to be a symbol occurring in capital letters in WEBSTER'S Unabridged Dictionary, edition of 1852. By an approximate method I found that my own vocabulary was thirty-three thousand four hundred and fifty-six words, with a probable error of not more than *one per cent.* Allowing a probable error of even *two per cent.*, my own vocabulary would then be comprised between the limits of thirty-four thousand one hundred and twenty-five words and thirty-two thousand seven hundred and eighty-seven words. I am confident this is not too large, for the following reasons:

1. The method of obtaining this number, though necessarily approximate, is tolerably accurate, and the limits of *error to fear* are ample.

2. My own process was repeated, in part, by Mr. FARQUHAR of the Patent Office, independently of me, in the most careful manner, and with the result of finding a still larger vocabulary than my own.

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* Bulletin of the Philosophical Society of Washington, 1875, Appendix VI., and Proceedings American Philological Association, 1875, p. 4.

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3. The same work was done independently by Professor EASTMAN, of the Observatory, with a like result.

4. Dr. GRAY, of Harvard College, and Professor THEODORE GILL, of the Smithsonian Institution, have informed me that their own merely technical vocabularies comprise a very large fraction indeed of thirty thousand words, and hence it appears that the limit of ten thousand words is exceeded in some cases by technical words alone.

5. The Anglo-Saxon Chronicle, which was *completed* before A. D. 1100, actually contains no less than eleven thousand nine hundred and thirteen words, which were all in use before this date; and while some of these are now obsolete, it is little less than certain that the succeeding seven hundred years has at least doubled the number of words in daily use by intelligent men. The mere introduction of the telegraph has added scores of words to ordinary vocabularies and steam has added hundreds.

For these reasons, and because of other partial trials, I have great confidence in repeating my previous conclusion, viz.: that twenty-five thousand words and over is by no means an unusual vocabulary for persons of average intelligence and education.

While I am myself satisfied of the correctness of this result, and while I am also satisfied that an examination of the subject by any competent person will lead to the same results, I still desired to carry this research a little further, as I had opportunity. It is plainly a matter of great difficulty to form a concordance to the published works of any writer, and the purpose in hand is perhaps not sufficiently important to justify the expense of time and labor.

It is, however, possible to determine the actual vocabularies of children while they are yet young, with great precision. This I have done in two cases, which are herewith presented, and I am able by the courtesy of a friend in England to give the same data in another case. In all these cases only words are included which were actually *used* during the twenty-fourth month of the child's life. The most rigid system of exclusion of all doubtful words has been followed. For example, there

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are few children who are not familiar with many nursery rhymes, but in the cases presented, no word of such rhymes is included in the vocabulary unless it was independently and separately used, in the same way with the words of daily and common use. In the first two cases the words so excluded are above 500 in number. Again, the names of objects represented in pictures which were known and frequently repeated, are omitted unless they were often spontaneously used or applied to living objects as well. I desire to emphasize the rigor with which words at all doubtful were excluded. I may give still another example of it. In descending or ascending stairs their number was frequently and intelligently counted in order from *one* to *fourteen:* yet I have only included such numbers as were separately and independently employed.

I have presented the lists of words in the order of their initial letters, as I at first believed (and now am certain) that the ease or difficulty of pronouncing a word largely determines its early or late adoption, it being all the while entirely comprehended. Under each initial letter I have divided the words used into several classes: 1. nouns, etc.; 2. verbs, etc.; 3. adjectives, etc.; 4. adverbs, etc.; 5. all other words. This was done because I supposed that I should meet with more nouns than verbs, more adjectives than adverbs, and that this inequality would be great according to the varying complexity of the idea. Thus, the idea of a modified action, as "walking easily" is far more complex than that of a modified substance, as "red clay." I expected then to find great inequality: the comparative tables hardly bear this out, as may be seen by consulting them.

I am inclined to take it as a result of my inquiry that the ease of pronunciation, far more than the complexity of the idea, determines the adoption of a word. This is borne out by all the material under consideration. The children under examination were as nearly as possible exposed to similar surroundings. Of the three the first [M. H.] spoke very distinctly always, the acquisition of words was a pleasure, and the total number used up to two years of age was four hundred and eighty-three. By glancing over the summary of

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this case it will be seen that the words of easy initial letters predominate, and this is true in all cases. Thus for B there are fifty-three words used, while for L there are but sixteen, or a ratio of three and thirty-one one-hundredths. In the Dictionaries, which represent the vocabularies of adults, this ratio is one and seventy-three one-hundredths, and the difference I attribute to the easy pronunciation of words having initial B. In the second case [M. M. H.] the total number of words is three hundred and ninety-nine. In this case the pronunciation of many words was difficult, and they were therefore avoided and easy ones substituted, although the meaning of the ones not used was thoroughly comprehended. In the third case [B. K.] there was considerable backwardness in enunciation, although words were comprehended quite as fully as in the other cases, and the total number used is in this case but one hundred and seventy-two, and great preference is evident for the words of easy initials.

It may be interesting to exhibit the ratios of the various classes of words, nouns, verbs, etc., in the various cases, and I append the following table relating to this.

| | M. H. | M. M. H. | B. K. |
|---|---|---|---|
| Number of Nouns ÷ Number of Verbs = ..... | 2.66 | 2.56 | 3.86 |
| Number of Nouns ÷ Number of Adjectives = ..... | 8.38 | 6.22 | 8.62 |
| Number of Nouns ÷ Number of Adverbs = ..... | 9.83 | 13.53 | 18.66 |
| Number of Nouns ÷ Number of other words = ..... | 10.17 | 9.20 | 10.19 |

To show the effect of easy initial letters in the adoption of words into the vocabulary of a child, it may be interesting to compare the following table, in which the first line gives the order of frequency of the various letters of the alphabet as initial letters for adults, the second this order for M. H., the third for M. M. H., and the fourth for B. K.

TABLE SHOWING THE ORDER OF FREQUENCY OF THE VARIOUS LETTERS OF THE ALPHABET AS INITIAL LETTERS.

| | | | | | | | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| For adults..... | S | C | P | A | D | R | B | T | F | M | I | E | H | L | G | U | W | O | V | N | J | Q | K | Y | Z | X |
| For M. H..... | S | B | C | T | P | M | H | W | D | R | N | L | F | G | A | O | K | I | E | Y | J | U | V | Q | X | Z |
| For M. M. H..... | B | S | C | T | P | M | W | H | A | F | L | R | D | G | N | O | K | I | E | Y | U | J | Q | V | X | Z |
| For B. K..... | C | B | H | P | S | M | F | G | W | A | D | N | R | T | K | J | L | I | O | E | Q | Y | U | V | X | Z |



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This table appears to show that in general the order of frequency of initial letters is the same for adults and children, but that in the latter case this order is modified by the greater ease of pronouncing words with some initials. The cases of M, R, D, etc., illustrate this well.

In conclusion, I may say that I have the less hesitation in presenting this research in a field to which I am unaccustomed, as I am confident that the *facts* upon which it is based are correct and as I have given with it sufficient data for those more skilled than myself to correct any erroneous inferences I may have drawn. The tables here given seem to me to have more than a merely statistical value, as rightly studied they throw much light upon the mental processes of the child, and give us a clue as to the desires, impulses, and thoughts which continually seek and find expression. In the course of collecting these data, the comparatively great complexity of these processes has been repeatedly and vividly brought before me.

It may perhaps be asked why these vocabularies were not continued longer in each case, as they would thus acquire additional value.

In answer I would say that such a labor would be immense, as from my own experience I should consider it almost impossible to be as certain of the accuracy of the vocabulary of a child of three years as I am in the cases here given, so rapid is the acquisition of new words after the early part of the third year.

VOCABULARY OF M. H.,

*Comprising only words* USED *by her in every day speech during the twenty-fourth month of her life, October–November*, 1875.

A. NOUNS, ETC.—Auntie, Annie, apple, arm. VERBS, ETC.—Asleep. ADJECTIVES, ETC.—A, an, another, afraid. ADVERBS, ETC.—(All), away, again, along. ALL OTHER WORDS.—And, at.

B. NOUNS, ETC.—Bummer-laddie (to a doll), baby, book, bonnet, bed, bread, butter, berries, bureau, brush, bell, bottle, boots, boy, button, [bird, birdie], bumbl(y)-bee, bib, box, bricks, bath, bath-tub, bone, bundle, beef, bracelet, bubble, ball, bench, Bessie, biscuit, band (article of dress), bucket, button-hook, blanket, bit (a little bit), basket. VERBS, ETC.—Burn, bite, break, broken, brush, button, be, bring, brought, bump, blow, bark. ADJECTIVES.—Bad. ADVERBS, ETC.—Backwards, badly, better (may better) by-low. ALL OTHER WORDS.—By and by.

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C. NOUNS, ETC.—Caroline, corner, carriage, Charley Kirk,* cat, carpet, clouds, chicken-s, chair, comforter, crib, cradle, cigar, coat, collar, cuffs, clock, curtain, cotton, cane, cow, chin, closet, Captain Green,* comb, child, car, candy, chamomilla, coal, cellar, can, cake, cup, cap, curls, cloak, clothes. VERBS, ETC.—Come-(s)-(ing), catch, [cry, crying], cut, can, creep, cover, choke. ADJECTIVES, ETC —Cold, clean, cool. ADVERB.—Careful. ALL OTHER WORDS.—(Take) care.

D. NOUNS, ETC.—Dorothea, door, daughter, dress, [dog, doggie], darling, drawers (2), ducks, ding-a-ling (i. e. a bell), doll, dollie, dirt, double-gown, drink. VERBS, ETC.—Don't and do, dance, dancing, (go) (going), drink, drop. ADJECTIVES, ETC.—Dear, dirty, dressed. ADVERB.—Down.

E. NOUNS, ETC.—Eye, eyes, ear, ears, egg. VERB.—Eat. ADJECTIVE.—Enough.

F. NOUNS, ETC.—[Foot, feet], finger, flowers, fly, flies, floor, fire, Frankie, face, forehead, fur. VERBS, ETC.—Frighten, fall, [fix, fixed], [find, found], feed. ADJECTIVE.—Four. ALL OTHER WORDS.—For.

G. NOUNS, ETC.—Grandma, grass, [girl, girlie], geese, goose, garters, glass, glove, gas (meaning "gas-light"), Galileo (to a picture), gentlemen. VERBS, ETC.—Go, gone, going, give, get. ADJECTIVE.—Good. ALL OTHER WORDS.—Good-bye.

H. NOUNS, ETC.—Holden, hen, hair, hammock, house, hat, [horse, horses], handkerchief, him, her, home, head, herself, hole, hominy. VERBS, ETC.—Hurt, [have, had, has], hold, hush, hear, hang, hug, help. ADJECTIVES, ETC.—Hot, high. ADVERBS, ETC.—Horse-back, here. ALL OTHER WORDS.—Hi! there, hullo.

I. NOUNS, ETC.—I, it, ice and ice water. ALL OTHER WORDS.—In, indeed (as *no* or *yes* indeed).

J. NOUNS, ETC.—Jack and Jacky boy, Johnson. VERB.—Jump.

K. NOUNS, ETC.—Kittens, knife, knee, keys. VERBS, ETC.—Kick, keep, kiss, know.

L. NOUNS, ETC.—Lap, lady, looking-glass, leg, leaves (of a tree), Lulie. VERBS, ETC.—Love, lie (down), look out (to), let, likes, listen, lost, lend, laugh and laughing. ADJECTIVE.—Lazy.

M. NOUNS, ETC.—Mabel, mamma, match, mouse, meat, milk, mouth, man, Marie, Miss Reid, money, morning, moon, my,† me, music, Mrs. Green, Mrs. Kirk, medicine, machine (sewing), mother, matter (what is the), Mary, muff. VERBS, ETC.—Make, must and must not, may, mend. ADJECTIVES, ETC.—More, mad. ADVERB.—Much. ALL OTHER WORDS.—My gracious!

N. NOUNS, ETC.—Night gown, nose, needle, nurse, neck, Ned, nails (finger nails), napkin, name, nipple. ADJECTIVES, ETC.—Naughty, nice, new. ADVERBS, ETC.—Now, nicely. ALL OTHER WORDS.—No, never mind.

O. NOUNS, ETC.—One (as "Mabel have one"), oatmeal, outside, observatory. VERB.—Open. ADJECTIVE.—One. ADVERBS, ETC.—Out, off. ALL OTHER WORDS.—Oh! on, over, of.

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* Inseparable.

† Meaning *your*, as "get up in my (*your*) lap." Also used correctly.

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P. NOUNS, ETC.—Papa, pussy and pussy cat, picture, piano, pillow, potato, porch, peach, piece, pocket (used correctly), pocket (= pocket-book), pantaloons, pin, petticoat, pipe, plate, paper, pencil, pig, park, puppy, pony, parlor. VERBS, ETC.—Put, play, please, pull, poured-out, pick, point, push. ADJECTIVES, ETC.—Pretty, poor. ALL OTHER WORDS.—(For) pity's (sake).

Q, R. NOUNS, ETC.—Ribbon, rocking-chair, room, ring, rose, rice, roll, rooster, rope, rag. VERBS, ETC.—Rock, ring, ride, run-away (one word), read, rock. ADJECTIVE.—Rough. ADVERBS, ETC.—Ready, right-away, quickly, round.

S. NOUNS, ETC.—She, sacque, sash, sister, steps, sky, sun, song, sword, stove, sheep, sugar, string, street, [shoe, shoes], stick, spool, stairs, smoke, saucer, supper, scissors, stockings, stone, shirt, soap, shawl, spoon, spectacles, sir, shoulder, shelf, somebody, supper, skin (of an apple), shadow, side, something, soup, Struve (to a picture), slate. VERBS, ETC.—Show, [sit, sat], sing, see, smoke, shut, show, smack, swing, stand-up (one word), suck, sew, stay. ADJECTIVES, ETC.—Sick, sore. ADVERBS, ETC.—So, sleepy, still. ALL OTHER WORDS.—Shocking! (as an exclamation).

T. NOUNS, ETC.—Tongue, teeth, tail, toes, thumb, table, tick-tick (for a watch or clock), tea, tree, towel, to-day, thimble, trunk, them, table, toast. VERBS, ETC.—Tear, take, touch, tumble-down,* tie, trot, turn, tie it off (= untie), taste, tickle. ADJECTIVES, ETC.—Two, three, this, that, there. ADVERBS, ETC.—Two, to and fro,† together. ALL OTHER WORDS.—Thank you, the, to.

U, V. NOUN.—Veil. ADVERB.—Very. ALL OTHER WORDS.—Under, up, upon.

W. NOUNS, ETC.—Wagon, Willie, water, watch, window, wall, would, wind, wheel, wrapper. VERBS, ETC.—Wants, wake-up,* walk, wash, won't, wet, wrap, wag-his-tail (one word), wise, write-ing. ADJECTIVES, ETC.—Warm, wet. ADVERB.—Where. ALL OTHER WORDS.—What, with.

Y. NOUNS, ETC.—Yard, your-s. ALL OTHER WORDS.—Yes.

SUMMARY.

| | Nouns, etc. | Verbs, etc. | Adjectives, etc. | Adverbs, etc. | Miscellaneous. | Sum. |
|---|---|---|---|---|---|---|
| A. | 4 | 1 | 3 | 4 | 2 | 14 |
| B. | 37 | 10 | 1 | 4 | 1 | 53 |
| C. | 38 | 8 | 3 | 1 | 1 | 51 |
| D. | 13 | 5 | 3 | 1 | .. | 22 |
| E. | 3 | 1 | 1 | .. | .. | 5 |
| F. | 9 | 5 | 1 | .. | 1 | 16 |
| G. | 10 | 3 | 1 | .. | 1 | 15 |
| H. | 15 | 8 | 2 | 2 | 2 | 29 |
| I. | 3 | .. | .. | .. | 2 | 5 |
| J. | 1 | 2 | .. | .. | .. | 3 |
| K. | 4 | 4 | .. | .. | .. | 8 |
| L. | 6 | 9 | 1 | .. | .. | 16 |

* Inseparable. † Thoroughly understood and used correctly.

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| | Nouns, etc. | Verbs, etc. | Adjectives, etc. | Adverbs, etc. | Miscellaneous. | Sum. |
|---|---|---|---|---|---|---|
| M. | 24 | 4 | 2 | 1 | 1 | 32 |
| N. | 10 | .. | 3 | 2 | 2 | 17 |
| O. | 4 | 1 | 1 | 2 | 4 | 12 |
| P. | 23 | 8 | 2 | .. | 1 | 34 |
| Q, R. | 10 | 6 | 1 | 4 | .. | 21 |
| S. | 41 | 13 | 2 | 3 | 1 | 60 |
| T. | 16 | 10 | 5 | 3 | 3 | 37 |
| U, V. | 1 | .. | .. | 1 | 3 | 5 |
| W. | 10 | 10 | 2 | 1 | 2 | 25 |
| X, Y, Z. | 2 | .. | .. | .. | 1 | 3 |
| Sum, | 285 | 107 | 34 | 29 | 28 | 483 |

Total number of words, 483.

## VOCABULARY OF M. M. H.,

*Comprising only words* USED *by her in every-day speech during the twenty-fourth month of her life, December,* 1876.

A. NOUNS, ETC.—Auntie, Annie, apple, arm, apron, apple-sauce, anything. VERBS, ETC.—Asleep, ate, are. ADJECTIVES, ETC.—A, another, afraid. ADVERBS, ETC.—All, away, again, around. ALL OTHER WORDS.—And.

B. NOUNS, ETC.—Broom, breakfast, [baby, babies], book, bonnet, block, bed and bedstead, babyhouse, bread, butter, bureau, brush, bell, bottle, boots, boy, button, box, bath-tub, bundle, beef, BessieChew* (proper name), basket, bracelet, bubble-s, biscuit, bucket, buttonhook, (a little) bit,* bow-wow, bosom. VERBS, ETC.—Burn, bite, break, brush, button, brought, been, bump, bark. ADJECTIVES, ETC.—Bad, bare, busy, beautiful. ADVERB. —(Give it) back. ALL OTHER WORDS.—Because, (to get) by (an obstacle).

C. NOUNS, ETC.—Cold (= influenza), Caroline, corner, cat, carpet, chicken, chair, crib—cradle (two different articles), collar, clock, curtain, cotton, Charlotte, moo-cow, closet, comb, condensed-milk,* coal, cake, cup, curls, chariot, cracker, candle (meaning lucifer match), capitol, chocolate, cracked-wheat,* corner. VERBS, ETC.—Come, cry, crying, cut, can, (take) care, carry. ADJECTIVES, ETC.—Cold, clean, comfortable (meaning *un*comfortable), charming.

D. NOUNS, ETC.—Door, dear (as "you are a"), dress, dog, darling, drawers, doll, dustpan, dining room. VERBS, ETC.—[Do, did, don't], dance, drunk. ADJECTIVES, ETC.—Dear, down.

E. NOUNS, ETC.—Eye-s, ear-s, egg. VERB.—Eat. ADJECTIVE.—Enough.

F. NOUNS, ETC.—[Foot, footies], finger, flower, floor, fire, Frankie, fork, feeding-apron, fan, (naughty-)fellow,* flag, fountain. VERBS, ETC.—Fall, find. ADJECTIVES, ETC.—Fat, funny. ADVERB.—Further. ALL OTHER WORDS.—For.

G. NOUNS, ETC.—Grandma, girlie, goose, garters, (looking) glass, glove, gas, glasses (= spectacles). VERBS, ETC.—Go, gone, give, get, got. ADJECTIVE.—Good. ALL OTHER WORDS.—Good-bye.

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* Inseparable.

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H. NOUNS, ETC.—Holden, hair, hammock, house, hat, horse, handkerchief, her, head, herself, hand. VERBS, ETC.—Hurt-s, have, hanging-down,* hold, hug. ADJECTIVES, ETC —Hot, high, heavy.

I. NOUNS, ETC.—I, it. VERB.—Is. ALL OTHER WORDS.—If, in.

J. VERB.—Jump-ing.

K. NOUNS, ETC.—Knife, knee. VERBS, ETC.—Kiss, know, kick.

L. NOUNS, ETC.—Lap, looking glass, legs, lunch, leggings, Louise Chew.* VERBS, ETC.—Love, lie (down), look, [let, let's], like, leave, lost, laugh, light. ADJECTIVES, ETC.—Lame, little.

M. NOUNS, ETC.—Mabel, mamma, match,† mouse, meat, milk, mouth, man, money, morning, moon, my, mine, myself, me, music, Mrs. Singleton, mitten, Mary, mat, mantelpiece, map. VERBS, ETC.—Make and make-up (a bed), mend, move. ADJECTIVE.—(Too) much. ADVERB.—More.

N. NOUNS, ETC.—Nightgown, nose, needle, neck, Ned, (finger) nails, napkin, necklace. ADJECTIVE.—Naughty. ADVERBS, ETC.—Now, new. ALL OTHER WORDS.—No, never mind.

O. NOUNS, ETC.—Oatmeal, observatory. ADJECTIVE.—Open. ADVERBS, ETC.—(Go) out, off. ALL OTHER WORDS.—Oh! on, over, one, of, (my) own.

P. NOUNS, ETC.—Papa, pussy, picture-s, piano, pillow, potato, piece, pocket, pin, pincushion, petticoat, pipe, plate, paper, pencil, pig-s, parlor, pop (of a bottle), pitcher, pantaloons. VERBS, ETC —Put, play, please, pull, point, pin, pinning, pare (an apple). ADJECTIVES, ETC.—Pretty, precious.

Q. NOUN.—Quadruped.

R. NOUNS, ETC.—Ribbon, rocking-chair, room, Rose, rice, roll, remedy, rubbers. VERBS, ETC.—[Rock, rocking, rock over], ring, roll, read, reading. ADVERBS, ETC.—Ready, round. ALL OTHER WORDS.—Right-there, right-back.

S. NOUNS, ETC.—Story, sofa, Santa-Claus, sack, sash, sister, sky, sun, song, stove, sugar, shoe-s, stairs, saucer, supper, scissors, shirt, stockings, soap, shawl, spoon, St. Louis, sleeves, shadow, stone, side, soup. VERBS, ETC. —Slipped, show, sit, sing, see, swing, stand, sneeze, sew-ing, spill, step. ADJECTIVES, ETC.—Sick, sore, some, strong, sleepy, sorry. ADVERB.—Shocking.

T. NOUNS, ETC.—Tiptoes, tongue, teeth, tail, toes, thumb, table, tick-tick (= watch), tea, tree, towel, thimble-s, trunk, tumbler, tea-cup, toast, (nice) time. VERBS, ETC.—Take, turn, taste, tie, thread (a needle), throw, tell, trod. ADJECTIVES, ETC.—This, that. ADVERBS, ETC.—Too, there. ALL OTHER WORDS.—The, to, two.

U. Up, until.

W. NOUNS, ETC.—Water, window, wall, (this) way, wrapper, window-seat. VERBS, ETC.—Walk, [wake, woke], want, wet, [won't, will], wipe, writeing, whip, wait, wind (a watch). ADJECTIVES, ETC.—Warm, wet. ADVERB.—Where. ALL OTHER WORDS.—What.

Y. NOUN.—You. ALL OTHER WORDS.—Yes, your-s.

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* Inseparable. † Used correctly, and also to mean a candle.

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SUMMARY.

| | Nouns, etc. | Verbs, etc. | Adjectives, etc. | Adverbs, etc. | Miscellaneous. | Sum. |
|---|---|---|---|---|---|---|
| A. | 7 | 3 | 3 | 4 | 1 | 18 |
| B. | 31 | 9 | 4 | 1 | 2 | 47 |
| C. | 29 | 6 | 4 | .. | .. | 39 |
| D. | 9 | 3 | 2 | .. | .. | 14 |
| E. | 3 | 1 | 1 | .. | .. | 5 |
| F. | 12 | 2 | 2 | 1 | 1 | 18 |
| G. | 8 | 3 | 1 | .. | 1 | 13 |
| H. | 11 | 5 | 3 | .. | .. | 19 |
| I. | 2 | 1 | .. | .. | 2 | 5 |
| J. | .. | 1 | .. | .. | .. | 1 |
| K. | 2 | 3 | .. | . | .. | 5 |
| L. | 6 | 9 | 2 | .. | .. | 17 |
| M. | 20 | 3 | 1 | 1 | .. | 25 |
| N. | 8 | .. | 1 | 2 | 2 | 13 |
| O. | 2 | .. | 1 | 2 | 6 | 11 |
| P. | 20 | 7 | 2 | .. | .. | 29 |
| Q. | 1 | .. | .. | .. | .. | 1 |
| R. | 8 | 5 | .. | 2 | 2 | 17 |
| S. | 27 | 11 | 6 | 1 | .. | 45 |
| T. | 17 | 8 | 2 | 2 | 3 | 32 |
| U. | .. | .. | .. | .. | 2 | 2 |
| V. | .. | .. | .. | .. | .. | .. |
| W. | 6 | 10 | 2 | 1 | 1 | 20 |
| X, Y, Z. | 1 | .. | .. | .. | 2 | 3 |
| Sum, | 230 | 90 | 37 | 17 | 25 | 399 |

Total number of words, 399.

## VOCABULARY OF B. K.,

*Comprising only words* USED *by him in every-day speech during the twenty-fourth month of his life.*

A. NOUNS, ETC.—Apple, Artie, Ayre (proper name), Auntie, Alley. ADVERBS, ETC.—Again, asleep.

B. NOUNS, ETC.—Bath, back, bit, Biddy, button, bell, box, boat, book, beads, boy, bread, Bernie. ADJECTIVE.—Black. ALL OTHER WORDS.—Bo! bye.

C. NOUNS, ETC.—Cab, cough, car, cards, cart, cats, curl, coal, cup, cupboard, chocolate, cuckoo, chair, chain. VERBS, ETC.—Can and can't, come, cough. ADVERB.—Careful.

D. NOUNS, ETC.—Desk, door, doll, Daw (referring to a picture). VERB.—Don't. ADJECTIVE.—Down. ALL OTHER WORDS.—(Oh!) dear.

E. NOUN.—Egg.

F. NOUNS, ETC.—Fan, Fanny, fire, fun, foot, fog, finger, flannel. VERB.—Fall. ADJECTIVES, ETC.—Funny, four.

G. NOUNS, ETC.—George, girl, gas, grapes. VERBS, ETC.—Go, gone. ADJECTIVE.—Good. ALL OTHER WORDS.—Good morning, good night, good bye.

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H. NOUNS, ETC.—Ham, hair, Harry, hill, hole, Holden, house, hand, horse, handkerchief, Hilda. VERBS, ETC.—Have, hang. ADJECTIVES, ETC.—Heavy, high, hot.

I. NOUN.—Ink. VERB.—Is.

J. NOUNS, ETC.—Jig, Jimmy, Jack, Jill. VERB.—Jump.

K. NOUNS, ETC.—King, kiss, knee, key. VERBS, ETC.—Kick, kiss.

L. NOUNS, ETC.—Lot, leaves, lock.

M. NOUNS, ETC.—Ma, May, Mary, Maud, moon, mouse, me, Margie, milk, mess. ADJECTIVE.—Mine. ADVERB.—More.

N. NOUNS, ETC.—Nail, noise, note, nose. ADJECTIVE.—Naughty. ALL OTHER WORDS.—No, not.

O. NOUN.—Ox. VERB.—Open.

P. NOUNS, ETC.—Papa, puss, pump, pen, pocket, pony, poker. VERBS, ETC.—Pass, please, pinch, play, pull, practise, pop.

Q. NOUN.—Queen.

R. VERBS, ETC.—Run, roll, ride, rock. ADJECTIVE.—Red. ADVERBS, ETC.—Rainy, ready.

S NOUNS, ETC.—Spoon, sash, shoes, scissors, sugar, stool, side, sun, scratch, string. VERBS, ETC.—Sing, she, saw. ALL OTHER WORDS.—Six.

T. NOUNS, ETC.—Tea, tray, toe, train, tongs. VERB.—Tumble. ALL OTHER WORDS.—Two.

W. NOUNS, ETC.—Wood, water, watch. VERBS, ETC.—Won't, will, wash, write. ADJECTIVES, ETC.—Wet, white.

Y. Yes.

SUMMARY.

| | Nouns, etc. | Verbs, etc. | Adjectives, etc. | Adverbs, etc. | Miscellaneous. | Sum. |
|---|---|---|---|---|---|---|
| A. | 5 | .. | .. | 2 | .. | 7 |
| B. | 13 | .. | 1 | .. | 2 | 16 |
| C. | 14 | 3 | .. | 1 | .. | 18 |
| D. | 4 | 1 | 1 | .. | 1 | 7. |
| E. | 1 | .. | .. | .. | .. | 1 |
| F. | 8 | 1 | 2 | .. | .. | 11 |
| G. | 4 | 1 | 1 | .. | 3 | 9 |
| H. | 11 | 2 | 3 | .. | .. | 16 |
| I. | 1 | 1 | .. | .. | . | 2 |
| J. | 4 | 1 | .. | .. | .. | 5 |
| K. | 4 | 2 | .. | .. | .. | 6 |
| L. | 3 | .. | .. | .. | .. | 3 |
| M. | 10 | .. | 1 | 1 | .. | 12 |
| N. | 4 | .. | 1 | .. | 2 | 7 |
| O. | 1 | 1 | .. | .. | .. | 2 |
| P. | 7 | 7 | .. | .. | .. | 14 |
| Q. | 1 | .. | .. | .. | .. | 1 |
| R. | .. | 4 | 1 | 2 | .. | 7 |
| S. | 10 | 2 | .. | .. | 1 | 13 |
| T. | 5 | 1 | .. | .. | 1 | 7 |
| W. | 3 | 3 | 2 | .. | .. | 8 |
| Y. | .. | .. | .. | .. | 1 | 1 |
| Sum, | 113 | 30 | 13 | 6 | 11 | 173 |

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## VI.—*On the Text and Interpretation of certain Passages in the Agamemnon of Aeschylus.*

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These remarks on the Agamemnon of Aeschylus are submitted in an honest desire to throw light into some of the dark corners of this greatest of ancient tragedies. They are made in the full knowledge of the fact—of which indeed only very superficial scholars can be ignorant at this day—that there are still many passages of the Agamemnon which no skill of scholars has ever been able to clear up, and which will probably always remain a battle-ground for critics.

There is one source of knowledge to which many will think it is no longer of any avail to turn for new light on Aeschylus: I mean the manuscripts. The list of these is easily given, so far as the Agamemnon is concerned. The Medicean with its two copies, all sadly mutilated and containing less than a quarter of the Agamemnon; the two Venetian fragments; the Florentine and the Farnese, the only two which contain the whole tragedy;—these are the whole. And it might reasonably be thought that the careful collations of the older scholars had exhausted the resources of these few manuscripts and left them (to use Bentley's expression) like "squeezed oranges." I will first give a few examples to show that this is not entirely correct. A short inspection of the Codex Venetus (616 in the Library of St. Mark), containing Agam. 1–45 and 1095 to the end, showed that some gleanings yet remained in that fragment. In vs. 1196 this MS. reads plainly *τὸ μὴ δένᾳι*, i. e. *τὸ μὴ εἰδέναι*. Although this reading is adopted in many modern editions, it is always given as an emendation (see Paley's and Weil's notes). Hermann says: "Omnes [i. e. codices], *τό μ' εἰδέναι*. . . . Apertum est aut deesse negationem, aut ineptum esse *λόγῳ*." Others, as Schneidewin, accept *τό μ' εἰδέναι* on the authority of the MSS., and explain or emend to avoid the inconsistency which Hermann points out. Again,



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in vs. 1127 many editors accept μελαγκέρῳ in the belief that this is the original reading of the Medicean, which now has μελαγκέρωι with ν written over the final ι. But the first reading of the MS. was clearly -ων, which was made -ωι by correction and was afterwards restored by a third hand. Recent editors doubt whether πορθεῖν or ποθεῖν is the reading of the Codex Florentinus in vs. 342 (see Hermann's and Paley's notes), and Hermann accepts ποθεῖν partly on the authority of his collation of that MS., saying "idque ex Flor. mihi enotatum est." But ποθεῖν is really found in no MS. at all, the Florentine (like all the others) having πορθεῖν beyond question. Hermann cites the Codex Florentinus as authority for the singular reading in vs. 345, θεοῖς δ' ἂν ἀμπλάκητος εἰ μόλοι, where I have copied the reading of this MS. (I think correctly) θεοῖς δ'ἀναμπλάκητος. I am at least confident that there is no breathing or other mark over the syllable αμ. I can hardly believe that Hermann's reading could ever have been adopted into any text had it not been for this supposed authority. Apart from the sense, ἄν (belonging to γένοιτο) would be in an absolutely anomalous position thus imbedded in the protasis, which could be defended by none of the ordinary examples of double or triple ἄν in long sentences, still less by the formula οὐκ οἶδα ἂν εἰ, as in EURIP. Med. 941, οὐκ οἶδ' ἂν εἰ πείσαιμι. Besides, the sense of the MSS. reading, θεοῖς δ' ἀναμπλάκητος εἰ μόλοι στρατός, *but (even) supposing the army to reach home without offending the Gods* (as suggested in vss. 338–342), seems best suited to the thought of the following lines, in which Clytemnestra darkly hints that a reckoning awaits the victors after their arrival at Argos, even though they may not incur new wrath of the Gods by sacrilegious plundering at Troy.

The passages which I have selected for discussion belong chiefly to the large class in which it seems to me that the readings of the manuscripts have been needlessly called in question, and my object is therefore in great part a defence of the manuscript text. In many cases I fear that my attempt will seem both heretical and abortive to older students of Aeschylus, who have generally assumed that certain passages are corrupt, and to whom the emended text has in a measure become the vulgate.

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1. Vss. 105–107: *ἔτι γὰρ θεόθεν καταπνείει πειθὼ μολπὰν, ἀλκὰν σύμφυτος αἰών.* Most recent editors read *μολπᾶν* depending on *πειθώ*, and *ἀλκᾷ* depending on *σύμφυτος*, omitting the comma. Hermann reads *ἀλκᾷ* and retains *μολπάν*, but he takes *ἀλκᾷ σύμφυτος αἰών* in the sense of *the time that the war has lasted*, and puts it in apposition with *πειθώ* to express *id quo niteretur ea fiducia.* Other interpretations may be found in Paley's and in Weil's notes. It seems to me that the emendations are far more difficult to explain than the reading of the MSS. as given above. In this reading it is hard to see what there is in either sense or construction to which almost all editors have taken exception. The asyndeton and the chiastic order both suit the sense, and we may translate as follows: "For still (i. e. after these many years of waiting) persuasion from the Gods inspires me with song; still even my old age (literally 'the time that has grown with me' for 'the time that I have lived') inspires me with strength (to sing)." The first clause was clearly so understood by the Medicean scholiast who says: *πείθει γάρ με ἡ παρὰ θεῶν πίστις μέλπειν καὶ λέγειν ὅτι εὖ πράξουσιν οἱ Ἀτρεῖδαι ὅσον ἀπὸ τοῦ σημείου.* The meaning of *σύμφυτος αἰών* (sc. *μοι*) and the construction of *ἀλκάν* with *καταπνείει* are indicated by the succeeding scholion: *ὁ γὰρ σύμφυτός μοι αἰὼν—ὅ ἐστι τὸ γῆρας—διὰ τὴν εἰς θεοὺς πειθὼ μολπήν μοι καὶ ἀλκὴν καταπνεῖ· ὅ ἐστιν, εἰ καὶ γέρων εἰμί, ὅμως μέλψω τὰ γεγονότα· πέποιθα γὰρ ὅτι εἰς πέρας· αὐτὰ ἄξουσιν οἱ θεοί.* The words *διὰ... πειθώ* here show a reading and interpretation of the first clause which we cannot reconcile with any possible form of the words *πειθὼ μολπάν.* Paley suggests that this scholiast may have read *πειθοῖ*, but his version would require also *καὶ ἀλκάν* or *ἀλκάν τε.* In the Medicean *πειθώ* and *μολπάν* have been changed by a later hand to *πειθῶ* and *μολπᾶν.* Weil gives *μολπᾶν* as the reading of the first hand; but I feel confident that my own collation is correct here. Perhaps *πειθῶ* may confirm Paley's suspicion about *πειθοῖ.* The use of *σύμφυτος αἰών* (sc. *μοι*) in the sense of *the time* (or *age*) *which has grown with me* is well illustrated by Agam. 894: *ἀμφί σοι πάθη ὑρῶσα πλείω τοῦ ὀ υ ν ε ύ δ ο ν τ ο ς χρόνου*, i. e. *more accidents than could have occurred during the time I was sleeping* (*the time sleeping with*

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*me* being used for *the time I was sleeping*). See also Eumen. 286: χρόνος καθαίρει πάντα γηράσκων ὁμοῦ. Hermann quotes also SOPH. Oed. Col. 7: ὁ χρόνος ξυνὼν μακρός, and Oed. Tyr. 1082: οἱ συγγενεῖς μῆνες. It may be added that in the former clause ἔτι means *even now, after ten years' waiting for the fulfilment of the predictions*, referring to the omen of the two eagles and the hare, of which the chorus are about to sing, and the interpretation of it by Calchas; the faith of the chorus in the Gods and in the ultimate fulfilment of the predictions *still* remains unshaken. In the second clause ἔτι refers to the chorus *still* having strength afforded even by their old age, εἰ καὶ γέρων εἰμί. It is perhaps hardly necessary to say that the whole passage in question, ἔτι...αἰών, is a pure parenthesis, the following ὅπως...πέμπει being the development of the idea first expressed by ὄδιον κράτος, etc., in the leading clause.

2. Vss. 249–254. No passage in Aeschylus has been read and explained in a greater variety of ways than this. Between Hermann's τὸ προκλύειν δ' ἥλυσιν προχαιρέτω (τὸ μέλλον being joined with the preceding sentence) and Paley's τὸ μέλλον δ' ἐπεὶ οὐ γένοιτ' ἂν λύσις, προχαιρέτω there is room for an infinite amount of conjecture and ingenuity. A few recent editors, Schneidewin, Weil, and Enger (1874), adopt a reading which is essentially that of the Farnese MS. in all except the last verse; but none, I believe, now venture to retain the reading of the best MSS. through the whole passage. As the text is so much in question, I give (from my own collation) the exact readings of the three principal MSS. in the first part of the passage. The following is the text of the Medicean (the words and *colon* within the brackets being added by a later hand in blacker ink):

> Δίκα δὲ τοῖς μὲν παθοῦσιν μαθεῖν
> ἐπιρρέπει
> τὸ μέλλον[· τὸ δὲ προκλύειν]
> ἐπιγένοιτ' ἂν κλύοις προχαιρέτω

The Oxford fac-simile of this manuscript (ed. by Merkel, 1871) fails to mark the interpolation in τὸ δὲ προκλύειν, and no one (to my knowledge) has noticed that the colon after μέλλον

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is a part of the interpolation. Indeed, the total absence of punctuation in the Medicean is an important part of the record.

The Florentine MS. reads:

> δίκα δὲ τοῖς μὲν παθοῦσιν μαθεῖν,
> ἐπιρρέπει τὸ μέλλον. τὸ δὲ προκλύειν, ἐπεὶ
> γένοιτ' ἂν κλύοις, προχαιρέτω.

The reading of Ven. A (468), so far as it could be deciphered, seemed to agree with that of the Florentine, and it is so given by Hermann. In 1872 the words between προκλύειν and προχαιρέτω were no longer visible, even in the sunlight.

The reading of the Farnese MS. is as follows:

> Δίκα δὲ τοῖς μὲν παθοῦσιν μαθεῖν
> ἐπιρρέπει. τὸ μέλλον
> ἐπεὶ γένοιτ' ἂν κλύοις, προχαιρέτω.

The words τὸ δὲ προκλύειν had evidently been introduced into the text before the Florentine and Venetian MSS. were copied, so that these latter have τὸ μέλλον joined with μαθεῖν ἐπιρρέπει, while τὸ προκλύειν takes its place as the object of κλύοις. But this construction of τὸ προκλύειν is as fatal to the sense as the introduction of τὸ δὲ προκλύειν into the text at all is to the metre, which is in perfect agreement with that of the strophe without these words. It is obvious that the only construction which the original copyist of the Medicean could have had in mind in that which the copyist of the Farnese MS. (probably Triclinius) adopted in his text, either by conjecture or from some purer source than the interpolated Medicean text. Of course, ἐπιγένοιτ' in the Medicean is only a slip of the pen or the ear for ἐπεὶ γένοιτ', and we thus have the construction τὸ μέλλον ἐπεὶ γένοιτ' ἂν κλύοις, which requires only δ' after μέλλον to make both sense and metre complete. Davies objects to this reading on the ground that ἐπεὶ γένοιτ' ἄν is not a possible construction. But the construction is τὸ μέλλον κλύοις ἂν ἐπεὶ γένοιτο, *you can hear of the future when it comes*, the assimilating force of κλύοις (a force which is especially strong in poetry) causing what would otherwise be ἐπειδὰν γένηται to become ἐπεὶ γένοιτο. This is like τεθναίην ὅτε μοι μηκέτι ταῦτα μέλοι (MIMN. I. 2) and ὡς ἀπόλοιτο καὶ ἄλλος ὅ τις τοιαῦτά γε ῥέζοι

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(Odyss. I. 47), where assimilation alone makes the optatives more natural. Indeed, this example is a strong confirmation of the position on the whole subject of assimilation and its effect on moods which is maintained in the paper on "*Shall* and *Should* in Protasis," *Transactions* for 1876, pp. 102, 103. There is the same difficulty in translating γένοιτο here in English that is felt in translating μέλοι or ῥέζοι, above; and for the same reason. The position of ἄν, where a comma *might* precede, is not objectionable so long as τὸ μέλλον, which is a part of the clause containing ἄν, precedes the particle, and ἐπεὶ γένοιτ' is only an inserted clause. See ARIST. Pac. 137: ἀλλ', ὦ μέλ', ἄν μοι σιτίων διπλῶν ἔδει. The general principle that ἄν cannot be the first word in a clause, even after a comma, is subject to this limitation, not to speak of others.

The Medicean scholiast who wrote against vs. 249 τοῖς μὲν πεπονθόσιν ἡ δίκη δίδωσι τὸ μαθεῖν evidently had the original construction in mind. But the following note, δίκην γὰρ δόντες μανθάνουσι τὸ μέλλον, must come from some one who joined τὸ μέλλον with μαθεῖν in the text. When τὸ μέλλον is rightly taken with the following words, it will also be the natural subject of προχαιρέτω, which τὸ προκλύειν could hardly be.

In vs. 253 all MSS. and editions agree in ἴσον δὲ τῷ προστένειν. If the interpolated τὸ δὲ προκλύειν is left out of the text, τὸ προχαίρειν (sc. τὸ μέλλον) will be the subject; i. e. *for the future to be dismissed (bid farewell) before it comes is just as well* (ἴσον) *as lamenting it before it comes*, for it will surely come, whichever we do. When, however, τὸ δὲ προκλύειν was added, it was taken as subject here, and the meaning was supposed to be *hearing the future beforehand is equivalent to bewailing it beforehand*, on the ground that it must be full of sorrow. The later scholiast on this verse has this idea when he says: ὁ γὰρ προγιγνώσκων τὸ μέλλον καὶ προστενάζει. Indeed, it is highly probable that τὸ δὲ προκλύειν was first written in the margin as the subject (understood) of ἴσον ἐστίν, as it only adds confusion to all the other constructions.

A greater difficulty comes in the last line. Here there is little or no dissent among recent editors from the emendations of Wellauer and Hermann, τοφὸν γὰρ ἥξει σύνορθρον αὐγαῖς, for

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συνορθὸν αὐταῖς (Med. and Ven.). For συνορθόν Flor. and Farn. have σύναρθρον. The words σύνορθρον αὐγαῖς are sometimes understood as referring to the actual rays of the morning sun (just about to rise), sometimes to the metaphorical sunlight which is expected to break upon the darkness of uncertainty in which the Argives at home have been living. The objections to συνορθὸν αὐταῖς are, first, that συνορθός does not elsewhere occur, and secondly and chiefly, that αὐταῖς cannot be referred to the distant τέχναι without great violence to the sense and still greater obscurity. But συνορθός (or perhaps σύνορθος), though a ἅπαξ εἰρημένον, is no more so than σύνορθρος, and is, moreover, amply justified by the compounds ἄνορθος, *upright*, with the cognate verb ἀνορθόω, *set upright again*, and ἔξορθος with ἐξορθόω. We have the verb συνορθόω in Arrian (see Lexicon); and an adjective συνορθός, *coincident with*, would naturally be expected. Compare συνόμιλος, σύμμετρος, σύνοξυς, and other such compounds of σύν. A word thus analogically formed, and found in the Medicean MS. of Aeschylus, is not open to objection as a ἅπαξ εἰρημένον, provided it suits the sense of the passage. (See also 6, below.) We come now to αὐταῖς, which cannot be referred to anything nearer than τέχναι Κάλχαντος in vs. 248. But those terrible words τέχναι δὲ Κάλχαντος οὐκ ἄκραντοι, following the minute description of the preparations for the sacrifice of Iphigenia, and taking the place of an account of the sacrifice itself, suddenly bring before the mind the awful reality which faces the chorus as they think of the condition of things. These words give unity to the whole choral song, and show more plainly than any exact language could have done that the Argive state now stands on the brink of a new gulf of horrors, which may well exceed all the ancient horrors of the house of Pelops. Let us trace the course of thought which runs through the whole chorus, that we may see more clearly the exact relation of the verse in question to the whole. The first stasimon and the lyric parodos (from vs. 104) form in subject a single ode.

The chorus first describe the omen which was seen as the Argives marched forth to Troy, two eagles devouring a

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pregnant hare. This Calchas interpreted as portending the capture and destruction of Troy by the Argives. But, with an ominous reserve, he fears *only* that some divine displeasure may cast a gloom over the bright prospect; for Artemis is watching with envious eyes her father's winged hounds, the two eagles, and the two sons of Atreus whom they represent, and she "loathes the eagles' banquet." And as Artemis, the friend of all the beasts of the field, is asking her father Zeus to fulfil what the prodigy portends, the bad as well as the good, so the prophet in turn prays Apollo to prevent his sister from detaining the Argive fleet by any contrary winds, which he fears she may do in her eagerness for "a new sacrifice, a lawless one, of which no man can partake, a kindred worker of strife, that fears not man." "For," Calchas adds with double significance at the close, "child-avenging wrath (i. e. the wrath that avenges a child's murder) abides firm, terrible, ever rising afresh, haunting (directing) the house, treacherous, ever remembering." To the Argive chieftains just setting forth for Troy this was terrible enough, as reminding them of the vengeance that still was due for the murder of the children of Thyestes; while to the chorus, who quote it after ten years, it has gained a new and more terrible meaning through the "new sacrifice" at Aulis. To the chorus, therefore, and to the audience—who know even more than the chorus—these last words of Calchas pronounce the doom of the guilty race. The vague forebodings of the prophet—his fear lest some divine power might possibly darken the prospect, lest Artemis might detain the fleet, lest this detention might in some way cause "a new sacrifice"—had all been realized in the fullest sense; a child, the darling daughter of the King of Men, had been sacrificed to the father's ambition; and now nothing could save the race of Atreus from the double retribution of "child-avenging wrath." In this state of mind, with the hope of victory thus darkly clouded by the sure approach of retributive justice, the chorus again sing, in harmony with the words of the prophet, *αἴλινον, αἴλινον εἰπὲ, τὸ δ' εὖ νικάτω* (vss. 104–159).

The chorus now invoke the aid of Zeus, the only power

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which can relieve them from the load of anxiety which oppresses them. Uranos and Kronos, the elder divinities, are past and gone; but he who calls on the name of Zeus with willing heart shall gain perfect wisdom. But the law of Zeus makes wisdom the result of suffering; the "trickling of drops of torturing recollection before the heart in sleep" sobers men often in spite of themselves. And it is on the whole a gracious boon that this is so (vss. 160–183).

Then, by a sudden transition, the chorus describe the conflict in the mind of Agamemnon when he is told that his daughter's life is demanded by the army as a sacrifice to appease Artemis and still the opposing winds. He yields to the demand and to his own eagerness for victory. Then follows the graphic account of the preparations for the unnatural sacrifice, the maiden's prayers and cries to her father for help, the lifting of the victim "like a kid" upon the altar, her falling robes, the gags which checked her voice, and then her speechless appeal to the heroes whom she had often seen as her father's guests; Iphigenia lies upon the altar, ready for the sacrificial knife, "beautiful as a picture" (vss. 184–246). But here the chorus suddenly pause, and the last scene is left to be imagined. They say:

"*But what followed we saw not, and we tell it not. But* [we do say] *the prophetic arts of Calchas must bring fulfilment* (i. e. the vague horror of his predictions in vss. 147–155 must surely be realized). *But* [it is only by actual experience that we shall ever *know* what penalty is to be exacted for the sacrifice of Iphigenia, for] *Justice brings knowledge within the reach of those* [only] *who have suffered* (*πάθει μάθος*); *the future you can hear of when it comes; before that bid it farewell, and this is as well as to lament it beforehand; for* [whatever we do] *it will come out clear and plain in full accord with these* (prophetic arts)."

It seems to me that no one can thus take a connected view of the whole song without feeling that the interpretation here given to the transmitted text of the last verses is not merely possible but highly appropriate. There is a special force in *αἰταῖς*, referring to the solemn words *τέχναι δὲ Καλχαντος οὐκ*



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ἄκραντοι with emphasis at the end of a sentence which begins as parenthetical, but which thus leads the thought at the close back to the point from which it digressed. The gender of αὐταῖς, moreover, makes the reference to τέχναι much clearer in Greek than it can be made in English by our vague "them" or "these." Indeed, the ambiguity which *we* feel here can hardly be said to extend to the Greek.

The emended reading σύνορθρον αὐγαῖς, understood literally, *with the rays of the coming morning* (*orietur cum luce solis eventus*, Hermann), implying that the mystery is to be cleared up at sunrise, cannot give the correct meaning if τὸ μέλλον has been rightly explained above. For "the future" here includes not merely the question of the capture of Troy (which *was* to be decided at once), but also and chiefly the dreadful question of the doom impending over the race which had spread the Thyestean banquet and had sacrificed a royal princess on the altar of its ambition. This last question, as the chorus have said, can be decided only after the knowledge of the future has come through suffering; it is this knowledge that the chorus will bid farewell, for they have as yet no suspicion of the immediate doom which awaits Agamemnon on his return. The thought furthest from the minds of the chorus is that the coming dawn is to settle *this* terrible question. This interpretation is therefore opposed to the obvious sense of the preceding words. It is perhaps to avoid this that some recent editors understand the "rays of dawn" metaphorically, not of the morrow's sunrise, but of the future emerging from the darkness of futurity into the light of the present. In this view we have merely a strong expression for "the future will come to light plain and clear." As this cannot be called impossible, two questions arise: first, whether this interpretation is better suited to the whole sense of the passage than the one proposed above, which adds the idea that the future which is to come out "clear" must accord with the prophecy of Calchas; secondly, whether, if this is preferred, it is so superior to the sense afforded by the manuscript reading that it must be purchased by introducing into the text two conjectures, one a ἅπαξ εἰρημένον. I cannot doubt

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what answer will be given to these questions by unbiassed scholars, or by those who will reconsider their opinions from the beginning on a passage about which they have already made up their minds.

I have felt that the importance of these verses, which determine the final turn of thought in one of the grandest of lyric songs, and greatly affect the whole impression which the ode makes, is a sufficient justification of the space given to the discussion of them.

3. Vss. 931–943. These verses stand thus in the manuscripts (not to notice unessential variations):

ΚΛ. *καὶ μὴν τόδ' εἶπὲ μὴ παρὰ γνώμην ἐμοί.*
ΑΓ. *γνώμην μὲν ἴσθι μὴ διαφθεροῦντ' ἐμέ.*
ΚΛ. *ηὐξω θεοῖς δείσας ἂν ὧδ' ἐρδειν τάδε;*
ΑΓ. *εἴπερ τις, εἰδώς γ' εὖ τόδ' ἐξεῖπον τέλος*
ΚΛ. *τί δ' ἂν δοκεῖ σοι Πρίαμος, εἰ τάδ' ἤνυσεν;* (935)
ΑΓ. *ἐν ποικίλοις ἂν κάρτα μοι βῆναι δοκεῖ*
ΚΛ. *μή νυν τὸν ἀνθρώπειον αἰδεσθῇς ψόγον.*
ΑΓ. *φήμη γε μέντοι δημόθρους μέγα σθένει.*
ΚΛ. *ὁ δ' ἀφθόνητος γ' οὐκ ἐπίζηλος πέλει.*
ΑΓ. *οὐ τοι γυναικός ἐστιν ἱμείρειν μάχης.* (940)
ΚΛ. *τοῖς δ' ὀλβίοις γε καὶ τὸ νικᾶσθαι πρέπει.*
ΑΓ. *ἢ καὶ σὺ τήνδε δῆριος τίεις;*
ΚΛ. *πιθοῦ· κράτος μέντοι πάρες γ' ἑκὼν ἐμοί.*

In the interpretation of these much-disputed verses, I differ from Paley, where he has expressed his opinion, chiefly in regard to vs. 933 (906 Paley); but it is impossible to discuss a single verse of a *στιχομυθία* by itself. In the speech just finished, Agamemnon has expressed a decided repugnance to making himself a mark for divine vengeance, after his great victory, by walking into his palace upon a path spread with purple embroideries. He is well aware of his danger, already hinted at by the chorus: *τῶν πολυκτόνων γὰρ οὐκ ἄσκοποι θεοί* (vs. 461), and *τὸ δ' ὑπερκότως εὖ κλύειν βαρύ* (vs.469); and his mind cannot be entirely free from anxious recollections of Aulis and Iphigenia. Clytemnestra, who is still more awake to the importance of the crisis, is determined that her husband's last act shall be one of defiance against the Gods. But it is a time for coaxing and for arguments (especially *ad hominem*), not for open quarrelling with her husband. She therefore

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says (vs. 931): "Now don't say you won't walk on the embroideries, and so go against my wishes." I think that μή stands after its verb merely to make παρὰ γνώμην ἐμοί more prominent and to show that the interference with her pet plan for the king's reception is what she has most at heart. The poet says παρὰ γνώμην ἐμοί (rather than ἐμήν) as he might have said παρὰ γνώμην ἐμοί ἐστιν, *it is against my wishes* or *not to my mind*, opposed to κατὰ γνώμην ἐμοί ἐστιν. In this verse γνώμην means *wish*, *hope* (cf. DEM. Ol. I. § 16, p. 14: ἄν τι μὴ κατὰ γνώμην ἐκβῇ); but in the next verse (932) Agamemnon repeats the word with emphasis, giving it a slightly different turn by the change in expression. He says: "As to γνώμη, please understand that I shall not let my purpose (γνώμην) be weakened." This leaves Clytemnestra where she began; and she now tries a new style of argument, addressed to his sense of shame: "Could you possibly have vowed to the Gods in some time of fear that you would act thus?" The form of the question implies, with bitter sarcasm: "Surely *you*, Agamemnon, could never have had a moment of terror in which *you* could make such a vow!" Agamemnon has already (vs. 924) said that walking on embroideries is ἐμοὶ μὲν οὐδαμῶς ἄνευ φόβου. But he now replies with dignity and apparent firmness: "If ever a man declared a decision knowing perfectly what he was about, I have done it now." Hermann says of τέλος here: "Sic dictum ut sit pro decreto." This reply suits perfectly the meaning which I have given to the preceding verse, and is not at all open to the objection which Professor Kennedy (*Journal of Philology*, vii. 13, p. 17) makes to Mr. Paley's similar version, that it "is no reply to the previous words of Clytemnestra: it is a mere repetition of his refusal, '*No, I won't*,' in another form, rudely ignoring what his wife had said." Mr. Paley had omitted the interrogation-mark at the end of vs. 933 (906) and translated: "*You would have vowed to the gods to act thus in time of fear*, i. e. you are pursuing a course more like one in peril than a victor." But if we suppose Clytemnestra to have just suggested the possibility (or rather the impossibility) of Agamemnon's having been *frightened* into a vow that he would act with humility

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if he should ever capture Troy, the dignified reply of her husband is just what would be expected.

A third argument is now tried. Agamemnon is asked what Priam would have done if he had gained so glorious a victory; and he replies that Priam would undoubtedly have walked on embroideries. After he has been further asked to disregard human censure, and has replied that the voice of the people still has mighty power, Clytemnestra tells him that it is not desirable to escape the φθόνος of men, for "he who is unenvied is not an enviable man," i. e., he who escapes φθόνος is not the object of ζῆλος (is not ζηλωτός). It seems as if Agamemnon here decided that he was no match for his wife in "chopping logic," and that it would be better on the whole to make no more ungracious objection to her plan for his reception; and yet his scruples were by no means overcome, as appears in vss. 944–949, below. He shows his disposition to yield (as he had doubtless often yielded before) by saying: "It is not like a woman to be so eager for a fight as you are." The queen replies, now sure of her point: "It becomes the prosperous to submit even to defeat," i. e., they can afford to yield a point like this. Agamemnon rejoins, partly in scorn, but chiefly in jest: "Is *this* the kind of victory in a strife which *you* hold in honor," i. e., the victory (νίκη) which consists not in τὸ νικᾶν but in τὸ νικᾶσθαι. He speaks as if νίκη could be the equivalent of both τὸ νικᾶν and τὸ νικᾶσθαι, as τιμή is of both τὸ τιμᾶν and τὸ τιμᾶσθαι, and asks his wife if she adopts this principle for herself as well as for him. Professor Kennedy translates this verse: "Do you really care for victory in this dispute?" This requires a change of τήνδε to τῆσδε, which I cannot feel is necessary unless some objection can be urged against the interpretation given above. Nothing now remains for Clytemnestra but to ask that her husband's compliance may be not forced but willing.

I should thus translate the whole passage, following the the reading of the MSS. (as given above):

CL. And now don't say this and disappoint my wish (γνώμην).

AG. My purpose (γνώμην) be sure I shall never weaken.

CL. Could you ever have vowed to the Gods in any time of fear that you would act as you now do?

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AG. If ever a man declared a decision knowing well what he was about, I have done it now.

CL. But what do you think Priam would have done if he had accomplished what you have? (935)

AG. I am very sure *he* would have walked on embroideries.

CL. Now don't be afraid of the blame of men.

AG. Yet the voice of the people has mighty power.

CL. But the lot of the unenvied man is not enviable.

AG. It surely is not womanly to be (so) eager for a fight. (940)

CL. But it is becoming to the prosperous even to let a victory be gained over them.

AG. What! is *that* the kind of victory which *you* hold in honor (for yourself as well as for me)?

CL. Be persuaded (i. e. never mind which kind of victory it is): at all events let me prevail (here) by your consent.

4. Vss. 1025–1029:

*εἰ δὲ μὴ τεταγμένα*
*μοῖρα μοῖραν ἐκ θεῶν*
*εἶργε μὴ πλέον φέρειν,*
*προφθάσασα καρδία*
*γλῶσσαν ἂν τάδ' ἐξέχει.*

Every student of Aeschylus knows how unsatisfactory are all the widely divergent opinions of editors on these verses. Paley's translation—"But if the appointed law of fate did not hinder fate from getting further assistance from the gods, my heart outstripping my tongue would pour out these feelings"—seems to give a literal sense of the words in a perfectly grammatical construction; and his note on the last two verses shows, I think, that Schütz's emendation *καρδίαν γλῶσσα* is not only unnecessary but injurious to the sense. But can we rest satisfied with this interpretation of the first three verses? I trust that *any* suggestion on so obscure a passage will appear better than none.

I think, first, we must certainly take *μοῖρα μοῖραν* in a reciprocal sense, like *ἄλλος ἄλλον*; and secondly, *πλέον φέρειν* must mean *bear away more than its due*, after the analogy of *πλέον ἔχειν, to have more than is due.* Πλέον *φέρεσθαι* is common in the sense *have an advantage* (cf. Soph. Oed. Tyr. 500: *πλέον ἢ ἐγὼ φέρεται*); and a similar use of the active *φέρω* is familiar, as in Soph. Oed. Col. 651: *οὐκ οὖν πέρα ἂν γ' οὐδὲν ἢ λόγῳ φέροις.* The meaning of the passage will then be: "But did not one

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fate appointed by the Gods (sometimes) hinder another (fate appointed by the Gods) from securing more than its due, my heart would outstrip my tongue and pour forth its present burden." This seems to point to a doctrine of "interference" between two lines of fate, by which either may be checked or balanced in a course which would, if unhindered, prove too destructive. The chorus would thus imply that this last desperate hope is all that they can still see to warrant them in hiding their feelings longer ὑπὸ σκότῳ (vs. 1030). In this song the gloomy forebodings of the chorus assume a more definite form. The earlier songs have hinted darkly at coming disaster; while the description of the sacrifice of Iphigenia, the allusions to the slaughter at Troy, and the fears of the consequences of human pride, all disclose grounds for the gravest apprehension. But these fears are all vague and general; now, however, after Agamemnon has entered his palace, timidly πορφύρας πατῶν, and Clytemnestra has assured him in bitter irony that she has at her command the whole Ocean to supply "purple" to the royal house, the chorus feel that a deed of blood is close at hand. They do not yet divine its nature, least of all do they suspect that Agamemnon was walking to his death; but there is "murder in the air." The general tenor of their song is as follows:

"Why does this hovering phantom ever flit before my heart, and why can I not spurn it and restore confidence to my soul? I have seen the Argive host set sail for Troy; and now with my own eyes I have witnessed its return. But still my heart of its own impulse sings the Fury's lyreless dirge, and refuses to be encouraged by hope. And I know that this feeling within me is not all in vain, and that it points to some fulfilment of my forebodings; but yet I pray that my fears may prove groundless and without result.

"Great prosperity is ever insatiate to extend its limits, reckless of the close neighborhood of calamity; and human fortune as it sails onward often strikes a hidden reef. Yet the sacrifice of part of the cargo to save the rest may keep the ship from sinking and the fortunes of the house from falling, and one plenteous harvest averts all danger of famine.

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But far otherwise is it when the life-blood of a man has once fallen to the earth; this no incantations can recall. Were this not so, Zeus had never stopped Aesculapius from raising the dead. My only hope is in the thought that one line of fate fixed by the Gods may sometimes interfere with another line of fate and so hinder it from securing too much; were this not so,—had I not this desperate hope to encourage me,—my heart would outstrip my tongue and pour forth its burden. But, as it is, I can only hide my grief in darkness, sore vexed, and with no hope of ever seeing order come out of this confusion, while my soul is burning within me."

The passage in question thus supplies an important link in the chain of thought, and gives the ground on which the chorus decide to suppress their feelings a little longer. The appearance of Cassandra now gives a sudden turn to the play, and the affrighted chorus are for the first time made aware of the real danger which awaits them.

It may be said that no such doctrine of the interference of two lines of fate as is here supposed can be found elsewhere in the Greek religion. Even if this is true, I contend that such a doctrine appears *here* by the only interpretation of the language which is at once plain and consistent with the context. It cannot be too clearly understood that the ideas of fate which make the Μοῖραι the superiors of Zeus, and the King of the Gods merely a helpless agent in their hands, are not Aeschylean. The verses of the Prometheus (517, 518):

ΧΘ. *τούτων ἄρα Ζεύς ἐστιν ἀσθενέστερος;*
ΠΡ. *οὐκουν ἂν ἐκφύγοι γε τὴν πεπρωμένην.*

represent only the threats of a defiant rebel against the whole divine order of the world as this was established under Zeus; they refer moreover to a disaster which Zeus did avert by his own free-will. Greek orthodoxy—certainly the orthodoxy of Aeschylus—speaks plainly in the following verse (519), in which the chorus indignantly ask

*τί γὰρ πέπρωται Ζηνὶ, πλὴν ἀεὶ κρατεῖν;*

The doctrine of Prometheus probably represents a more ancient and gloomy view of inexorable necessity ruling both

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Gods and men, while the later view gave the government of the world to a wise and beneficent personal ruler, the director of other subordinate rulers, who had displaced a harsher dominion, and whose laws were made for the best good of mankind in general. These laws, however, the laws of nature, though beneficent on the whole, were inexorable and unyielding, often bringing misery upon the innocent children of a guilty race as the result of ancestral crime, but still by that very misery working out the great purpose of Zeus and making men wise through suffering. This stern, inexorable course of nature's laws, which all creeds must recognize, whatever they may choose to call it, seems to be the Fate of Aeschylus, the *μοῖρα τεταγμένα ἐκ θεῶν*. The Homeric *μοῖρα θεῶν* or *αἶσα Διός* stands in the same general relation to the more primitive government of the world by special interventions in which an earlier age believed. The frequent statues of *Ζεὺς μοιραγέτης* which Pausanias found in different parts of Greece show an absorption of an ancient idea of independent fate into the more advanced doctrine of the sovereignty of Zeus. (See Pausanias i. 40. 4; v. 15. 5; viii. 37. 1; x. 24. 4.) Now, if this was the poet's view of fate, that it was the onward march of nature's laws, the universal laws of the Gods, how could he have failed to see that the workings of several such laws, i. e. several lines of fate, may interfere with each other, like several mechanical forces, and produce a result which is different from any of them? In this view, the chorus simply express a last hope that the line of fate which seems to them to be leading directly to some new deed of blood may perchance be met and balanced by some other line of fate as yet unknown to them, so that the horrors which they see in prospect may be averted.

5. Vs. 1347: *ἀλλὰ κοινωσώμεθ' ἄν πως ἀσφαλῆ βουλεύματα.* This reading of the MSS. was emended by Porson to *κοινωσαίμεθ' ἂν πῶς* (interrogative). The emendation now generally adopted is that of Hermann *ἄν πως* (for *ἃ ἄν πως*). The latter is supported by two passages of Sophocles,—*ἀλλ' ἀναγκάσαι θεοὺς ἃ ν μὴ θέλωσιν οὐδ' ἂν εἷς δύναιτ' ἀνήρ*, Oed. Tyr. 281; and *φράσον τίς ἐστίν· ἃ ν λέγῃς δὲ μὴ φώνει μέγα*, Philoct. 574;—in both of



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which the sense makes ἄν for ἄν (= ἐάν) of the MSS. an almost certain correction. It is, however, quite as possible that ἄν πως in the MSS. is a mistake for ἤν πως, so that we should read ἀλλὰ κοινωσώμεθ', ἤν πως ἀσφαλῆ βουλεύματα (sc. ᾖ), *but let us take counsel, in case there shall prove to be any plans for safety*, i. e. *that we may adopt any plans for safety which there may be.* This is a case of the quite common absorption of the apodosis in the protasis, which sometimes gives ἐάν with the subjunctive the appearance of an indirect question. See PLAT. Rep. ii. 358 B: ἄκουσον καὶ ἐμοῦ, ἐάν σοι ταὐτὰ δοκῇ, *hear me too, in case the same shall please you*, i. e. *that then we may adopt it.* Here the construction is obvious; but in Rep. iv. 434 A: ἰδὲ δή, ἐάν σοι ὅπερ ἐμοὶ ξυνδοκῇ, many think they see an indirect question, though they cannot tell us what the form of the direct question would be. The change of ἤν to ἄν (= ἐάν) in the MSS. here supposed is confirmed by three passages of Sophocles,—ἤν φράσω, Trach. 672; ἤν...προσθῇ, Frag. 323 (Nauck); οὐδ' ἤν τὸν διδάσκαλον λάβῃ, Frag. 736, —in all of which the MSS. have ἄν. The further question, whether all four passages together do not furnish ground for an exception to the general doctrine that ἄν for ἐάν was *never* used by the tragedians, need not be discussed here. The meaning of the line with the reading ἤν πως agrees well with that of the preceding verse, τοὔργον εἰργάσθαι δοκεῖ μοι βασιλέως οἰμώγματι.

6. Vs. 1599: ᾤμωξεν, ἀμπίπτει δ' ἀπὸ σφαγῆς ἐρῶν. Here ἐρῶν is in most modern editions changed to ἐμῶν because ἐράω in the sense of *vomit* does not occur. But ἐρῶν seems amply defended by the compounds ἀπεράω, ἐξεράω, etc., and has rightly been restored (as I notice since reading this paper) by Weil. An instance of ἐξερῶν in this sense is found in Pherecrates (*Pers.* Frag. 2):

Ἁμαλάχας μὲν ἐξερῶν, ἀναπνέων δ' ὑάκινθον.

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## VII.—*On the Single Case-Form in Italian.*

By AUSTIN STICKNEY,

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The question as to which of the old Latin cases was the prototype of the single forms which remain in the declension of the Romanic languages has been often discussed, but is still unsettled. Opinions have varied between the nominative, accusative, and ablative, or two or more of these together; and some scholars, in despair of tracing any single case, have concluded that the ground-form alone survived.

The various views may be conveniently ranged under three heads. According to the first, no particular Latin case has survived in the modern languages; but the simple ground-form remains, divested of the old case-endings, and clothed in such new guise as each particular language has chosen. According to the second view, we have in the modern nominal forms the remains of some specific Latin case, differing in the various countries; and this case or cases survive in virtue of something like conscious choice or of a logical necessity. Professor Diez has stated and defended this view;* concluding that the normal cases are, for the Provençal and old French, the nominative and accusative; for the modern French, Spanish, and Portuguese, the accusative; for the Italian, the accusative with a considerable influence of the nominative. According to the third view the modern case-form is the phonetic result of the wearing away of the old endings, in consequence of which the cases came to coincide in a single form; as the old Latin dative *dominoi* and the ablative *dominod* were in time merged in the single form *domino*. Professor d'Ovideo † compares the old forms to pieces of money in circulation for a long time, and becoming by abrasion undistinguishable; and remarks that although in all morphological transformations there must be a mental process, yet the wheels, so to speak, on which the mind moves, are the phonetic changes.

* Grammar, II³, p. 5, ff.

† *Sull'origine dell'unica forma flessionale del nome Italiano*, p. 12. (Pisa, 1872.)

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Professor Ascoli* is very careful to exclude any idea of conscious choice or logical necessity; and the keynote of the theory is the explanation of the phenomena on the principle of phonetic decay.

It is often stated that in the transition from the Latin to the modern languages there has been a loss of cases; which of course is true of the flexional forms. But even M. Brachet† uses such language as this: "The tendency to simplify and reduce the number of cases was early noticed in the vulgar Latin; the cases expressed shades of thought too delicate and subtle for the coarse minds of the barbarians," etc. So M. de Jubainville‡ discovers a new principle of declension in the Merovingian times, consisting in this, "that in spite of the considerable number of forms" (he refers to the confusion of forms in the documents), "the number of functions which the mind conceives and requires to express in words is considerably reduced." But a moment's reflection ought to convince one that the logical relations still exist, and must find expression in any, even the simplest, language; it is only a question of *how* they shall be expressed. If for some reason the moderns no longer express these relations by the case-endings, they must do so in some other way; and they do so in fact by prepositions. Moreover, the barbarians were not so entirely unaccustomed to a nominal inflexion as to be quite overcome on meeting another; and the considerable preservation of the verbal inflexions with the creation of new synthetic forms in the modern languages shows that this at least is not the solution of the problem.

The facts are to a great extent conceded on all hands. At the end of the third century the final *m*, and at the beginning of the fourth, the final *s* were inaudible in the common pronunciation; for the *u* in final syllables of the classical language we find *o* in the earlier and vulgar Latin, which has remained in some of the dialects; and the endings *es* and *is* were to some extent interchangeable. Instead of the genitive and dative, the

* *Archivio Glottologico Italiano,* Vol. II., Part 3, pp. 417–421.

† *Grammaire Historique,* 10me ed., pp. 52, 147.

‡ *La Déclinaison Latine en Gaule à l'Époque Mérovingienne,* p. 160.

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popular language of all periods used more or less frequently the ablative and accusative with prepositions; and this increased with the loss of the case-endings. Which of the two was cause of the other it is not easy to say; but it seems more natural to suppose that the indistinctness of the mutilated forced a resort to the periphrastic forms.

Granting these differences between the written and spoken language, the vulgar Latin must at the end of the empire have presented in the singular of the first and second declensions, and in the parisyllables of the third, paradigms identical with the modern Italian; *rosa*, *domino*, *regno*, *pane*. The *u* stems of the fourth followed the analogy of the second; and the *e* stems of the fifth that of the first or third. In the stems in *ero* of the second which omit *us*, and in the *ri* stems of the third, the reduction would have left two forms, of which the oblique form has remained; there was, however, a wavering in the nominative forms in the original.

In the imparisyllables of the third with variable or invariable accent, the phonetic reduction would have left two forms in the singular, one from the nominative and one from the oblique cases; *sarto*, *sartore;* the nominative form has almost entirely disappeared, and the oblique form has inherited the succession, the exceptions being numerically of no importance.

The imparisyllabic neuters have occasioned the most discussion; here the nominative and the accusative, coinciding in a single form, had the weight of frequency in their favor, and have in Italian survived to a greater extent than the longer form. *Capo*, *volume*, *nome*, *fiume*, *seme*, *carme*, *germe*, *rame*, *legame*, *cece*, *lido*, *petto*, *pegno*, *tempo*, *sterco*, *ghiomo*, *lato* and others are nominative-accusative forms; *termine*, *fulmine*, *rovere*, *acero*, *cadavere*, *sovero-sughero*, *papavero*, *genere*, *rudere*, *ulcere*, *viscere*, *folgore*, are ablative forms; whereas in *vime-vimine*, *addome-addomine*, *pepe-pevere*, *marmo-marmore*, *solfo-solfore*, both forms have survived together. But the companion form may often be found in a dialect or cognate language when it has been lost in Italian; in Sardinia (Logudoro) we find *nomene*, *flumene*, *esaminu*, *ramine*, *legumene*, (Campidano) *nomini*, *semini;* so many of the longer forms in Spanish;

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*cadaver-cadavere* in Sardinia, *folg* in Friuli, etc. Professor Ascoli,* from whom many of these examples are taken, has discussed this point in detail, giving many other forms in Rumanian, in the dialects of Friuli and the Grisons; and has refuted the argument drawn from this class of words in favor of the accusative theory.

The reasoning of Professor Diez, for instance, is this: Italian *amore* can only come from Latin *amorem*, *amore; domino* from *dominom*, *domino;* that is, accusative or ablative; but *corpo* must be not from *corpore*, but from *corpus* nominative-accusative; and the several types are reconciled by considering the accusative as the normal case. But if the various dialects taken together show in this class of nouns a fair proportion of both forms, there was plainly no logical necessity for the adoption of either particular case; the survival was the result of a more frequent use of one than the other in this or that particular region; perhaps in some instances a mere matter of chance.

For the plural, we have in the first declension *rose*, which is the old nominative, perhaps the phonetic successor of *rosis;* but *rosas* is lost. In the second declension we have *domini*, the old nominative, perhaps also the successor of *dominis;* but *dominos* is lost; and even the nominative of the neuters has only survived in a few instances, the analogy of the masculines being too strong. In the third *principi* is probably a new formation after the analogy of *domini*, although some take it as the reduction of *principis* for *principes;* at any rate *principibus* is lost. The feminines after much hesitation followed the same analogy; as also the neuters when they have not gone over to the second declension; but the old forms in *ora* were very tenacious, and even for a time drew over some of the neuters of the second.

The recent advocates of the purely phonetic theory write the paradigms of the vulgar Latin with the periphrastic genitive and dative. This of course simplifies the matter very much; a form like the genitive plural is very inconvenient. But granting that it very early fell into disuse, if we

* Ibid., p. 423.

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are discussing the fate of the Latin cases, the disappearance of all the non-survivors must be explained.

The results of phonetic decay differ of course in the various countries according to the special aptitudes of the people. In France it would have left for the first declension one form in the singular, three in the plural; for the second, two in the singular, two in the plural; for the third masculine and feminine, two in the singular, and one or two in the plural; so that after slight losses and changes by way of analogy we should reach a system not far differing from that exhibited in the earliest texts; some outstanding forms still preserving their independence, like the accusative singular in *ain*, and the genitive plural in *or*.

The subsequent progress falls under our direct notice. An *s* is added to the nominative singular of the third declension after the analogy of the second, which was numerically the strongest, though the rule is not consistently observed in the texts; and gradually the *cas régime* comes to perform the additional function of the nominative, which as gradually drops out of sight; leaving still, as M. Meyer* observes, a declension of two forms, in that the two numbers are yet distinguished. If the process were to go on still further, this would also disappear; and even now it exists mostly only in the written language.

The history of the French declension throws light on that of the Italian, since the operating causes must have been in their nature the same. The moving spring in both cases is at first phonetic decay and change; when this has been at work for a time, there remains a mutilated declension of one, two, or three cases for each number, and only three of the five original schemes. It left in French a tolerably symmetrical system of declensions, owing to two peculiarities; first, the persistence of the final *s*, which saved the distinction of cases in the second; secondly, the general loss of the syllables after the tonic, which removed many differences, and left forms more nearly coinciding with the simple stem. On the other hand, in Italy, the native soil of the Latin, the paroxytones

* *Bibl. de l'École des Chartes*, V. V. 215.

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were tolerated or even favored, and the old endings remained vigorous much longer and in greater variety. Then ensued a struggle for existence among these forms; and aside from the simple phonetic currents, a sort of sexual symbolism, as Professor Flechia* calls it, was the strongest influence in determining the three schemes as they at present exist. Thus some of the neuter plurals of the second and third went over to the first; as also feminines of other declensions; and the scheme *o*, plural *i*, rallied most of the masculines. In fact most of the changes from the old declensions occurred for this reason.

In the third singular there remain distinct traces of an intermediate declension of two cases, as in the French; and a list of these *doppioni* might be made from those given by d'Ovidio, Caneleo,† Flechia, and Ascoli. But in general one form has perished, chiefly the nominative; or if both have survived, they have become practically different words. Examples of the waverings and irregularities may be found in abundance in Nannucci;‡ it was after all the grammarians that settled the rules; and the irregularities which still remain may generally be explained by the persistence of some of the old forms.

If the facts have thus far been correctly stated and explained, it would seem that neither of the three theories is sufficiently broad to include them all.

The theory of the retention of the mere ground-form of the old substantives seems to explain nothing whatever. Of course the substantial part of the word was retained; if not, what could have been retained? But if we ask *how* it happened, we are forced to answer: By the falling away of the old endings. If then they so fell away as to leave for a time in common use the naked root, which was afterwards clothed with new endings, the theory certainly does explain the matter. But is there any evidence of this? Is it not rather true that certain of the old endings never fell out of use, a few new ones came up, and the others were lost?

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* *Rivista di Filologia e d'Istruzione Classica. Torino,* Anno I., Fasc. 1, p. 91, etc.

† *Rivista di Filologia Romanza,* I. 132.

‡ *Teorica dei Nomi della Lingua Italiana,* passim. (Firenze, 1858.)

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The theory of the accusative or any specific case does not meet the difficulties. It may be true that the form of this or that case substantially reappears in the new form; but not because it was the accusative. The question recurs again: How and why? And as Professor Schuchardt* says, the theory resembles that time-honored rule of the Latin grammar, which we have all in early years received with bewildered and unquestioning trust, that of the genitive singular of the first and second declensions, and the ablative, etc., in answer to the question "Where?"

There remains the phonetic theory, to call it thus briefly; and here we must call attention to the form of statement. Professors d'Ovidio and Ascoli state it broadly, that the modern form is the result of the gradual wearing away of the old endings, and the coincidence of the previously different forms. The former allows that the generalization of the accusative may have operated the transformation "in a small degree"; and the latter, when considering the Spanish and Sardinian plurals, adopts the principle of natural selection.

But the phenomena are of too complex a character to be brought under so simple a statement; phonetic decay explains the singular number very well, but it is quite insufficient for the plural. The statement should then be made broad enough to include all the facts. It seems more reasonable to enlarge it somewhat after this fashion:

The present case-form is the result of the phonetic decay of the old Latin forms; but whenever this alone would have left more than one form for a number, there was a sort of natural selection, resulting in the survival of that one which, from its phonetic character, best suited the habits of the people using it, or which, oftener recurring in ordinary language, impressed itself more distinctly on the mind: in masculines and feminines this was generally the continuation of one of the oblique cases, chiefly the accusative; in neuters, generally the continuation of the common form of nominative and accusative; and where the present form is a new one, it has been made after certain analogies already existing in the old language.

* *Kuhn's Zeitschrift*, Vol. 22, p. 180.



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## VIII.—*On Wilmanns' Theory of the Authorship of the Nibelungenlied.*

By FRANKLIN CARTER,

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Some months since Dr. Wilmanns (now Simrock's successor at Bonn), whose work on the development of the Gudrun was marked by great originality, published a little book of ninety pages on the authorship of the Nibelungenlied, a problem which has perpetual interest for the Germans. Of late years the tendency has been to accept the authorship of *one* poet for our form of the composition, and one von Kürenberg has received the suffrages of the great majority of unitarian advocates. Wilmanns, whose work bears the title, "Beiträge zur Erklärung und Geschichte des Nibelungenlieds," plants himself at the outset in opposition to this tendency on Lachmann's ground, so far as to claim for Lachmann the first great effort to solve the many difficult questions concerning the poem, and further that what others have done, since Lachmann's annotations appeared in 1836, to promote an insight into the relations of the poem, is, in comparison with Lachmann's services, very little. He goes further, and seems to accept the Lachmann view of the manuscripts, uniformly translating from Lachmann's edition, which was based on A, though he alludes incidentally in some passages to the reading of C, and his discussion does not depend on his manuscript preference for validity. But he does not agree with Lachmann uniformly as to which are the interpolations, nor does he favor in the least the theory of twenty lays, which was the extravagance of Lachmann's confidence in the truth of his cause, though he admits that he endeavors to make progress in the path which Lachmann pointed out, to separate the different layers of the composition, and thus make clear the *development* of the poem, to handle the difficulties which lie the other side of the differences between the individual manuscripts—in other words, by analysis to ascertain if there are parts of the poem older and

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more genuine than others, and if any of these parts may have had an origin independent of one another.

"Since Lachmann's time," the writer says, "investigation has busied itself chiefly with matters of subordinate importance, and endeavored with the trifling means of philological craft, observations of style, of grammatical form, of vocabulary, of metrical usage, to construct the history of the poem." Wilmanns is undoubtedly correct as to the method of arriving at some knowledge of the construction of the poem. It is plain that there are many interpolations. A verse of some spirit and power is so often succeeded by three or four feeble dilutions of its contents, that but one conclusion is possible in regard to these, viz., that they must be interpolated. This once admitted, it follows inevitably that, as between an absurd explanation or dilution of an original inspiration and that original inspiration there may be a large variety of gradations in absurdity, or clumsy enlargement, or skillful combination, until these or most of them are determined, arguments from the omission of senkungen, or the use of inexact rhymes, or from peculiar grammatical constructions in order to fix the age or authorship of the poem; are very untrustworthy. If the interpolations are undetermined, or if the question is still an open one whether there is not here a "contamination" of two varying versions, the wildest and most inconsistent inferences may have support from the different parts of the poem.

The doctrine of this little book is that the poem is the result of a "contamination," as the Germans call a combination of two or more versions. Any presumption against this doctrine arising from the improbability that such a fact would have escaped the notice of all the distinguished scholars who have worked on the poem, is fully offset by the success which crowned the author's treatment of the Gudrun. There the confusion seemed more hopeless before he disentangled the various versions, but his skill in discriminating and readjusting was conspicuous. Though the Gudrun has been the subject of less study than the Nibelungenlied, the very fact of its admitted confusion made his success seem more noteworthy. But since there is sufficient apparent harmony in the Nibelun-

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genlied to render a reference of the poem to one author at least possible, a moment's reflection will make it clear that to discover and prove a "contamination" in it would be a triumph of greater skill than was required for the demonstration in the case of the Gudrun; but nevertheless, success with the latter, where the confusion was plain and where there was so much to invite previous investigators (and some of them were able) to the discovery, constitutes a claim to a respectful hearing, when our author presents analogous results from an investigation of the Nibelungenlied. For the latter is a national epic as is the Gudrun, and grew to its present form probably in somewhat similar conditions. If the Gudrun, as we have it, has been proved to be a "contamination" of two or more in important respects independent poetical versions of legendary and historical material, this proof excites a presumption that a similar process may have produced our present Nibelungenlied. Certainly in these days of recognition for development there should be no prejudice against such a possibility. That the Nibelungenlied has greater harmony and correcter perspective than the Gudrun may show that there was a final bearbeiter of the poem who removed some inconsistencies and adjusted sections to each other; but it cannot safely be assumed, on the ground of linguistic or metrical resemblances, that this bearbeiter or any poet is the *one author* of our one poem, unless also in the contents of the various parts of the poem such harmony and oneness can be proved, that on the removal of trifling interpolations, all the steps shall tend to one common end. The poet may indeed adopt elements of different legends, or even inconsistent elements in the treatment of the same legend, but he will remove contradictions, and looking backwards and forwards will adapt his materials to a conclusion. But if long sections, harmonious in themselves, have no account or consciousness of one another or even present incongruous elements, and especially if one section harmonizes in details and views with the finale while another is at variance with it, neither metrical nor linguistic agreement, nor a superficial onward movement can establish a oneness of authorship.

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Nor can the fact that Lachmann's twenty-lay theory is generally abandoned,* or the acknowledgment that there was possibly something of disingenuousness in his use of the heptads to support his hypothesis, blind us to the more important fact that must underlie all investigation, that the Nibelungenlied rests upon a growth of legendary and historical elements, extending in material over at least six hundred years from the time of the defeat by the Huns of the Burgundians in 437, under Gundicarius, to the Hungarian wars under Henry III. in the eleventh century. Of course it is not impossible that one author should fuse all these elements into a harmonious whole, but the natural repository for these ever varying legends and stories would be successive songs or metrical narratives. As these moved in different directions they must vary, and the Eddalieder and the Thidrekssaga present versions of parts of these stories, varying in certain main lines from the same recorded events as given in the Nibelungenlied. Does the Nibelungenlied itself present variations, differentiations of various parts of the story, is the question underlying Wilmanns' discussion. To this question he gives an affirmative answer.

Zarncke, who has never believed in manuscript A, or in anything that involved its superiority, asks in the *Literarische Central Blatt*, in order to ridicule this discussion of Wilmanns, "What has become of the Lachmann theory?" He says it has always had full scope in Prussia, and inquires where in popular estimation it now is. "What has become of the twenty lays?" is his main question, as though, if the twenty lays, which were published by Lachmann as the original songs of the poem, and afterwards appeared in a translation from Simrock, had not succeeded in crowding out of circulation the translated entire poem, as the manuscripts have preserved it, there can be no possibility that the poem is not an organic whole. As though, if it should be proved that twenty architects did not plan St. Peter's, it would follow that there is

* This abandonment can hardly be considered universal, and it may be questioned whether it is even general. It can do no harm, however, to admit so much.

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no possibility that two or three had the management of construction at different times and modified each other's designs unfortunately. That the whole poem, as Simrock translated it, has passed to a thirtieth edition, and the twenty lays of Lachmann, as Simrock translated these by themselves, have never passed to a second, does not show that the Nibelungenlied may not be a combination of two or three different versions, or that the German readers of the Nibelungenlied have more æsthetical discernment than sentimental patriotism. At the respectful mention of Lachmann's name certain scholars roll their eyes and at once assume a belligerent posture. And Zarncke, while giving honorable recognition to the perspicacity and acumen of these criticisms by Wilmanns, intending apparently fairly to recognize his complete independence of Lachmann's extravagance, cannot refrain from classing Wilmanns with Lachmann, and holding that the poem, as either regards it, is reduced to "ein dürres Geklapper wie von schlotternden Skeletten."

What are the subordinate propositions which Wilmanns attempts to establish, and which would lead to the conclusion that the poem, as we have it, is a "contamination"?

The first of these propositions is that a Ruedigersdichtung, a composition in which Ruediger and Kriemhild were the main characters, and Dieterich had as yet no share in the action, was the old basis of our poem. He claims to establish, secondly, that a poem or a part of a poem in which Dankwart was the main hero, was incorporated with this Ruedigersdichtung and caused certain changes in it, and rendered connecting interpolated verses necessary: in other words, that a Dankwartsdichtung was interpolated in the Ruedigersdichtung. The third proposition claimed is that another dichtung, in which Iring was the hero, was also incorporated in the Ruedigersdichtung, but entirely independent of the insertion of the Dankwartsdichtung, so that we have R. + D. for one form and R. + I. for a second. But it is also certain, in our author's judgment, that the Dankwart poet takes cognizance of the modifications of the Ruedigersdichtung by the poet who introduced Dieterich, so that the one form would be R. +

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Di. + Da., and the other remaining as before (for it is not clear that the Iring-poet knows anything of Dieterich's introduction into the poem) R. + I. we have for our Nibelungenlied, or rather for the last third of it, the "contamination" of R. + Di. + Da. with R. + I.

It is only the last third of the poem that Wilmanns discusses. He starts from the supposition that it is impossible that the advance of Dieterich to the fight near the close of the poem can have been occasioned by Ruediger's death. "Hildebrand brings him the intelligence of that death, he arms himself, and strides to the hall. Hagen discerns his intention and is ready for the conflict, so says our poem. Now what will he do? Why has he come? He will avenge the death of Ruediger, the death of his own warriors, of his best friends, his consolation in a strange country; he will demand of Hagen and Gunther atonement for shed blood, will retract peace and friendship from the Burgundians. This one would expect, but nothing of it takes place. Dieterich demands that Gunther and Hagen shall surrender to him—he promises them protection from the Huns and sure escort homeward, he spares their life in the battle and takes them captive at the risk of his own life, he leads them to Kriemhild and most urgently recommends them to her mercy. That this cannot be original, unitary invention is clear. That Dieterich struggles against fighting with the Burgundians, that he takes them captive, delivers them to Kriemhild and begs for their life, presupposes that he has begun the contest with reluctance, presupposes secondly, that Kriemhild has forced the conflict on him. In the legend as it appears at the end of our Nibelungenlied, Dieterich must similarly, as now Ruediger, have been pushed to the conflict by the entreaties of the revengeful queen."

There is difficulty in accepting as certain this last form of statement by Wilmanns. It is plausible to suppose that Dieterich, whose conduct bears marks of reluctance to slay Gunther and Hagen, may have been in some form of the saga identified with Ruediger, and impelled to advance against them by the entreaties of the queen. That he was entreated to advance to the fight is in our poem 1836, 1838, 1839.

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But is it impossible that he, after having advanced, especially as he had refused once to go, might be affected by the worn, wasted, bloody appearance of the two heroes, by the heaps of mutilated and gory corpses around them, by their awful isolation among the dead and in a strange country, or even by a feeling of awe at their colossal strength and valor, and under the influence of this emotion might refrain from piling their dead bodies on the ghastly hecatomb? The universal testimony of the saga literature is to the effect that Dieterich was himself a lonely exile. He is a grand, but mournful figure in the gallery of legendary heroes. Is it impossible that a fellow feeling for Gunther and Hagen may here be indicated? There may be here abridgment, combination, detrition in the legends, but can we be *certain* that Wilmanns' second assumption is correct, that Dieterich's reluctance to engage with and afterwards to slay these heroes, presupposes that in an original poem underlying part of ours, Kriemhild alone forced the conflict upon him?

There are indeed certain features that look like a close connection of Dietrich and Ruediger, or even a confounding of the one with the other. But instead of pushing the resemblance still further, may we not fear that the influence of assimilation has already gone beyond its proper limit, possibly that the reluctance of Dieterich in regard to slaying the two heroes is a reflex from Ruediger's unwillingness to fight? May we not even conjecture a cause for the poet's identifying these two, and believe that the original invention could not so far have assimilated them? What could be such a cause? What except the hospitality* of Ruediger to the exile, Dieterich, as mentioned in the poem "Dieterich's Flucht"?

Strophes 2094 and 2095 (Wilmanns regards these as interpolations), in which Ruediger speaks and is spoken to as if he were in exile, and 1614, 5, in which Ruediger declines the honor of a marriage for his daughter with one of the Burgundian princes, on the ground that he and his wife are ellende-exiles, apply to Ruediger what belongs to Dieterich. If

* Wilmanns rejects the line in 2251, wherein Dieterich claims a kinship with Ruediger.

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now we remember how the hospitality of Ruediger is praised, it is natural enough to regard Dieterich as bound to him by the enjoyment of this hospitality, and through this relation the confusion may arise by which the exile of Dieterich is transferred to Ruediger. Equally would such a relation account for Dieterich's movement to avenge Ruediger,* and the final conduct of Dieterich in taking the captive heroes to Kriemhild and recommending them to mercy need not then be regarded as presupposing that Kriemhild's entreaties alone forced him to the fight. That here is a combination of the Dieterich saga with the Ruediger saga seems probable, even certain, but that we can be sure exactly what line of movement occasioned Dieterich's final share in the tragedy as our poem originally presented is not clear. Wilmanns' supposition as to the influence of Kriemhild in determining Dieterich's movement is clever and plausible. It does not seem to be the only explanation possible for Dieterich's conduct.

But it does not follow, even if we do not regard the supposition that Dieterich was impelled to the fight by Kriemhild's entreaties alone as indisputable, that Wilmanns' processes of disentanglement lose their value. The steps in his discussion are in a measure parallel, at least somewhat independent, and one may believe with him that there is conclusive evidence that the Dieterich legend and the Ruediger legend are combined, and admit that his analysis makes that evidence clear, and that hereby a basis for the "contamination" theory is gained, without conceding that in every case the exact line of differing versions is or can be laid bare. In other words, the theory of "contamination" may solve some difficult problems so admirably that we accept it, and in some cases we may distinguish plainly the motives and lines of the combined versions, but that we can do this in every case the nature of "contamination" seems to preclude.

* It is claimed, for instance, by Richard von Muth, Einleitung in das Nibelungenlied, pp. 77–81, that Ruediger came into the Nibelungen cycle through Dieterich's relations to Helche, Etzel's first wife. All trace of such an introduction for Ruediger has disappeared from our poem. If it can be assumed as an original relation, a reminiscence of it might account for Dieterich's advance to revenge Ruediger.



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It is by the analysis and comparison of strophes, both as to form and content, that the old genuine verses are by the author first discovered, and then these are again further examined and compared, with such discriminating insight, with such comprehension of observation, with such sovereignty over the details of the poem, as to awaken admiration at nearly every step. In the excision of strophes interpolated by modern bearbeiter Wilmanns is extremely acute, and though he agrees in general with Lachmann, there are cases where he does not hesitate to disregard Lachmann's judgment, and does not always give reasons for doing so. As an illustration of his aptness at detecting the interpolator's work, the proposal of marriage for Gîselher at Ruediger's house may be cited: "Volkêr at the dinner has declared, if he were a prince he would like to sue for Ruediger's daughter. Modestly Ruediger declines so great a compliment, but Gernôt (1615) agrees with Volkêr, 'If I were to have a beloved according to my conception, I would be always happy with such a wife (as Ruediger's daughter).' After this outspoken declaration of love on the part of the unmarried man, Hagen extremely unnaturally speaks to propose the engagement of Gîselher. The latter has not as yet uttered a single word of desire for the maiden; why is he, the younger brother, pushed forward with violence when the elder is so eager for the marriage (heirathslustig)? 1614 and 1615 are evidently interpolations. Gernôt has here nothing to say, and der grimme Hagen has no share in the love affair." So much is admirable, and there can be no doubt as to the interpolation of these verses. But the author goes on: "By the side of these two strikingly bad interpolations the composition has undergone another better one, which is probably older than the two, certainly older than 1612, so that here two layers of refashioning overlap one another. When a person enters, then without having accomplished anything retires and must be summoned anew, we have, in refashioned poems, always reason to be on our guard (aufmerksam). "I believe," says Wilmanns, "that in the original composition the margravine remained in the hall, and was present when Volkêr made his

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proposition. Very noteworthy is the close of 1609, '*the noble fiddler cherished a kindly feeling towards the host.*' What does this remark here mean? Just before it is stated how strong an impression the ladies make on the men; afterwards it is related that they (the ladies) withdraw during the meal: how could a poet in such a connection come to mention Volkêr's affection for Ruediger? Further it is to be noted that in strophe 1613 the praise of Gotelinde is indeed suited to the situation, but not demanded by the progress of the narrative, that 1614 has no connection with this praise, and that here Volkêr is mentioned anew as talking, when he nevertheless already has the floor. One gets the closest connection if one lets 1614 follow directly after 1609. In the words which Volkêr here speaks, he exhibits the affection with which in 1609 the poet boastfully credits him." 1609 ends thus: "the noble fiddler cherished warm affection for the host." 1614 begins: "If I were a prince, spoke the warrior immediately, and should wear a crown, I would have your beautiful daughter to wife." The connection is thus good, but in regard to these excisions it may be noted, first, that Lachmann rejects 1609. It is certainly tame and superfluous, and Wilmanns gives no reason for retaining it, though it is clear why he wishes to keep it. Second, if it, with the succeeding strophes, is retained, the explanation of the "holden willen" is simply deferred to 1613 or to 1614, if 1613 is regarded as an interpolation; in other words, the explanation of "holden willen" finds expression, as soon as there is occasion. Third, by retaining 1611 and 1612, and thus having only the mother at the banquet (which middle German poetry would allow), the daughter is brought back and is present at the proposal, which presence Wilmanns seems to regard as desirable. Fourth, by retaining 1610, 1611, 1612 (there seems to be more reason for rejecting 1613) and omitting the verses which Lachmann regarded as spurious, there is harmony with less excision. These observations on Wilmanns' decisions in regard to interpolated strophes do not go so far as to concern his main conclusions, but they may perhaps show that such work is very delicate, and that there will be disagreement in different minds as to rejections.

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An illustration of what will seem to some as fine success in this separating process is found in the treatment of the passage relating to Ruediger's advance to the conflict. That passage *must harmonize* with the events that are connected with the entertainment of the Burgundians by Ruediger, and the two passages in what Wilmanns regards as their oldest form, are printed on page twelve of his little book. Any one who reads this presentation of Ruediger's advance to the fray will hardly note the "rattle of the skeletons" that so offends Zarncke.

From this passage, containing, as Wilmanns presents it, eleven and one-half strophes, twelve strophes of interpolations have been removed, and a fair consideration of the author's reasons for these excisions will result in an admission of their validity.

What account now do the defenders of the unity of the poem give of its inconsistencies? Heinrich Fischer at Greifswald in 1858 asserted in a critique on Lachmann's views, that there are but four positive inconsistencies in manuscript C. Doubtless he would have admitted more for A, and since the publication of Wilmanns' contributions to the question, the warmest advocates of unity will hardly renew a claim so preposterous for either of the three best manuscripts. So clearly does our analyst lay bare the incongruities, that Zarncke feels called upon to assign some cause for their frequency. He says: "It has not at all come into his (Wilmanns') thought to investigate if the style of the poem as it is, partly dependent on its previous history, viz., the oral delivery, partly also conditioned by a very painstaking (peinliche) strophe-form that breaks up the simple connection, with a declamatory pathos, with exaggerations aiming at effect, with its manner of expression by no means adequate to courtly correctness, accuracy, and individualizing, whether this style, as it lies before us uniform in the poem from beginning to end and cannot be exterminated, is not sufficient to explain those trifling carelessnesses and awkwardnesses which undeniably occur." There is much sound in this sentence, but what is its meaning? Does it mean that the poet was so conditioned by the oral form of tradition that he could not

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remove an inconsistency? That he was so limited by the strophe (and much may be said against its adaptation to an epic poem) that he could not tell a harmonious story? That "declamatory pathos and exaggerations for general effect" so far governed the writer that he could not remember from one page to another whether a character was present or not? A poet who should embody the majestic burden of this story in two thousand strophes, and at the same time be in such bondage to his metre and form, would surely be a wonder. We might as soon expect one who could not solve a simple equation in algebra to calculate an eclipse. Such a unity of authorship could claim neither authorship nor unity. Over against an investiture of mere style and form, with such power to account for incongruities, Wilmanns' theory of a "contamination" is simple and intelligible. One may grant that the logic of such a poem need not always be perfect, but something like harmony may be exacted. Wilmanns has made the most thorough attempt to establish a harmony, or to detect and account for incongruities, and in certain points his success is surprising. Take for instance Etzel's relations to the final movements of the poem. A cursory reading even of the Nibelungenlied will convey the impression that Etzel's part is insignificant. A second thought will suggest the propriety that this part should on internal grounds be very slight. Ruediger promises Kriemhild to defend her against all evil, and by that promissory oath wins her as a bride for Etzel, and escorts her to Etzel's court. Ruediger's relations are mainly then with Kriemhild, and in our poem, as Kriemhild is the moving power for the punishment of Hagen, Etzel must be in the background, and Ruediger must go to the conflict at Kriemhild's instigation.

This conception is strictly followed in Wilmanns' Ruedigersdichtung, which he claims is the old original poem. But in the poem as we have it, careful investigation shows that Etzel has a more significant part than he should have. In the scene (2072–2105) embracing Ruediger's resolution to fight against the Burgundians, Etzel is repeatedly brought into a prominence that the original conditions do not justify.

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2089, 2092, 2095, which represent Etzel as urging Ruediger to fight, not merely depress the original relation of Kriemhild and Ruediger, but are inconsistent with the representation of Ruediger himself after he has gone to fight, as in 2115, "the wife of King Etzel would not release me." Now by a comparison of the various appearances of King Etzel, Wilmanns makes out a strong case for the modification of Etzel's original status by interpolations, mainly by the introduction of a section of what is called the Dankwartsdichtung, running from 1787 to 1945, in which Dankwart is the hero. As Ruediger's resolution and conflict come later in the poem than the passages 1787–1945, which describe the going to the church, the knightly contest, the preparation for conflict, and the fight in the hall, which passages introduce Etzel in a way that would make his appearance in connection with Ruediger's resolution and fight necessary, the natural inference is that by the interpolation of these passages the bearbeiter was forced to introduce changes in the original relations of Ruediger, and brought him into a dependence on Etzel, and Etzel into a prominence that the old poem did not tolerate. No other supposition satisfactorily accounts for all the difficulties, and here Wilmanns deserves only praise for the solution. That there was a poem, be it called Dankwartsdichtung or otherwise, interpolated in the oldest form of the story may be regarded as proven, though here and there in the proof an incidental assumption may be questioned, or a verse differently assigned.

Points of difficulty in the main demonstration of this combination will be the impossibility of one authorship for the Dankwartsdichtung and the Iringsdichtung, or if this impossibility be accepted, the certainty that the two authors were absolutely ignorant of each other's work. The latter will be thought by some to rest on a slender basis. Views and style and metrical usages differ, but this hardly proves that they were not written by the same person, for instance, at different periods, much less does this difference establish that the writer of one dichtung never saw the other. On what does this "unverkennbare thatsache," as Dr. Wilmanns calls it, rest? On this that "the Dankwart poet would necessarily

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have mentioned in his description of the knightly contest and the fight in the hall with Dieterich and Ruediger, the heroes of Thüringen and Denmark, Irnfried and Iring, if he had known them, and the Iring-poet, who exhibits so clearly the effort to bring forward every hero, would surely not have left Dankwart out, had he known anything whatever of his hero-deeds. He does not appear in the entire adventure, of which Iring is the hero.

The burning of the hall in the version of the saga which our poem presents is a singular event, especially in its results. Hundreds of men are in this hall. Not one of them is injured. The Thidrekssaga presents this catastrophe in the same relations. Wilmanns' supposition that the old Ruedigersdichtung ended with the burning of the hall, and his claim that a "contamination" of this with another dichtung whose effects are in so many places seen, will alone explain that after a resultless hall-burning Dieterich puts an end to the fighting, have, after his previous analyses, much in their favor. Especially does the fact that in our poem the Burgundians, though they ask for deliverance *before* the fire is kindled, make no effort to escape after they are surrounded by flames, find explanation in the supposition that the more perceptible their extremity, the more absurd would be the absence of all destruction of life from the conflagration; but this absence of result was necessary, if Dieterich was to bring about in the combined poem the end which, in one version, the conflagration occasioned.

Zarncke objects to Wilmanns' skillful comparison of incidents in the Thidrekssaga with corresponding passages in the Nibelungenlied, and to the inferences or suggestions which he sometimes draws from the comparison. Zarncke has taken the position that this saga, whose present form is younger than the Nibelungenlied, rests on this poem. How Zarncke can hold this opinion will seem surprising to any one who merely reads Wilmanns' quotations from the saga. The publication, early this year, of a volume by Raszmann, which thoroughly refutes this doctrine and establishes the origin of both presentations in the Saxon songs, completely justifies Wilmanns' use of the quotations.

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The probability that some such "contamination" as Wilmanns claims to discover actually took place his demonstrations are sufficient to establish. That he has found all the exact lines of the oldest poem, which he calls the Ruedigersdichtung, he does not himself assert. It may be that he has here and there too absolute a confidence in his acute penetration. But no contribution to the Nibelungen question anything like as instructive as this has appeared since the days of Lachmann. Between the arguments by which on the one hand the Kürenberg hypothesis is urged, and the analysis by which on the other the "contamination" theory is supported by Wilmanns, he who swears by the tenets of no master or the excellence of no manuscript will not long hesitate. Even from a life-long student of the poem and a confessed antagonist of the theory of a composite origin for the poem, Wilmanns' method and movement have extorted admiration for brilliant perspicacity and strictly scientific language, and the confession that he had learned much from the little book. Only one third of the poem, it is true, is here discussed. But it is enough if "contamination" is made certain in two or three relations. For the reviewer it is established in the matter of Etzel's relations to Ruediger, and in the burning of the hall. He who admits so much must grant the truth of the theory. But it is possible that the two opposing theories, answering so plainly to opposite tendencies in the human mind (Herman Grimm calls the Wolfian view of Homer "eine fatale Hypothese"), will stand face to face as time goes on. The appearance of this book, however, makes it certain that the champions of unitarianism will never silence the combination advocates, as at one time seemed at least possible.

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## IX.—*On Herodotus's and Aeschylus's Accounts of the Battle of Salamis.*

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The Persian wars and the narrative of Herodotus may seem to belong to those classic subjects about which really the last word has been said. As for the battle of Salamis, a very recent writer on Greek History, Mr. Cox,* says: "So began the conflict in which the Athenians found themselves opposed to the Phenicians, who had the wing toward Eleusis and the west, while the Ionians toward the east and the Piraeus faced the Lacedaemonians. Beyond this general arrangement and the issue of the fight, the historian himself admits that of this memorial battle we practically know nothing."

Still I have undertaken a closer comparison of those accounts of the battle of Salamis and the events immediately succeeding, which are given by Herodotus and Aeschylus respectively. The late Professor M. Haupt, of Berlin, in his expository lectures on the *Persae*, a few years ago (1873), suggested that some tangible results might still be gained by such a separate and especial review.

In the preparation of this paper I have had occasion to make more or less use of recent contributions by two German scholars:

(1) Professor Adolph Kirchhoff, of Berlin, published in 1868 in the *Abhandlungen* of the Royal Berlin Academy, a research, "*Ueber die Abfassungszeit des Herodoteischen Geschichtswerkes*"; and to this he afterwards added a supplementary paper in the *Abhandlungen* for 1871;

(2) In the Munich Academy's *Sitzungsberichte*, 1876, there is a treatise by Wecklein, "*Ueber die Tradition der Perserkriege.*"

Kirchhoff has made a number of shrewd calculations about the time and the place of the composition of the several main portions of Herodotus's work. According to Kirchhoff's course

* "General History of Greece," p. 201.



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of argument, which I take pleasure in accepting, the latter part of book V.* and the remaining books (VI., VII., VIII., IX.), were composed by Herodotus at Athens, between 432–31 and 428–27, when the work was broken off. Wecklein† brings forward four main points:

1. Tradition affected by religious and moral conception of the Hellenes.

2. The tendency to depict the great past as splendidly and gloriously as possible, and to obliterate whatever might seem a blemish on the magnificent canvas.

3. The anecdotal and partly legendary character of tradition.

4. Personal inclination and dislike, hatred of factions, feuds of Greek commonwealths affecting tradition.

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The intentions of the historian were faithful and sincere, and, within a certain range, unbiassed; and there is certainly no cause for the passionate indictment found against Herodotus by the author of the treatise, *περὶ τῆς Ἡροδότου κακοηθείας*.‡ Herodotus made earnest exertions to gather authentic information, IV. 16: *οὐδενὸς γὰρ δὴ αὐτόπτεω εἰδέναι φαμένου δύναμαι πυθέσθαι*. . II. 28: *ἐπὶ μακρότατον ἐπυθόμην*, etc. As this honesty of intention may be safely taken for granted, we ask: What indications are found in Herodotus himself regarding his own sources of information? He says once for all (VII. 152), *ἐγὼ δὲ ὀφείλω λέγειν τὰ λεγόμενα, πείθεσθαί γε μὲν οὐ παντάπασιν ὀφείλω, καί μοι τοῦτο τὸ ἔπος ἐχέτω ἐς πάντα τὸν λόγον*. Similarly (IV. 195): *ταῦτα εἰ μέν ἐστι ἀληθέως, οὐκ οἶδα, τὰ δὲ λέγεται γράφω*. His *πυνθάνομαι* and his *ἤκουσα* have each its separate force. Mere *λέγεται* seems less distinct than *λέγουσι*, and this again less so than *λέγουσι* with a specified subject or subjects.

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* For this portion is the one which concerns us alone in the present investigation. Kirchhoff's arguments (*πίστεις* and *ἐπιπίστεις*) are especially strong in this portion of the paper.

† P. 241, sqq.

‡ See Volkmann, *Leben und Schriften des Plutarch* (Berlin, 1869). The strength of Boeotian patriotic feeling seems to betray itself, e. g. even in the estimate of Demosthenes and the slighting and unfavorable tone of many passages found in Plutarch's life of that man.

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The strange character of this or that subject matter sometimes seems to force a λέγεται from his pen. Authorities for tradition are but rarely indicated, as a rule only when conflicting traditions prevail. Thus (III. 9) οὗτος μὲν ὁ πιθανώτερος τῶν λόγων εἴρηται· δεῖ δὲ καὶ τὸν ἧσσον πιθανόν, ἐπεί γε δὴ λέγεται, ῥηθῆναι. Again (III. 120, 121), οἱ μὲν δή μιν φασὶ κ. τ. λ. . . . . οἱ δὲ ἐλάσσονες λέγουσι πέμψαι, . . the preference of the writer being thus indicated. See also I. 82, III. 1, 2, III. 32, IV. 154, V. 85, sqq., and most especially V. 44, in the account of the memorable feud between Croton and Sybaris.

To come nearer to the point under consideration: What evidence is there of Herodotus's acquaintance with, and utilization of, literature then existing? The statement (I. 12, 8) that Archilochus was a contemporary of Lydian Gyges, mentioning the latter ἐν ἰάμβῳ τριμέτρῳ, Stein calls an interpolation; Dietsch does not seem to have been offended by it. The expression (II. 2) Ἕλληνες δὲ λέγουσι· ἄλλα τε μάταια κ. τ. λ., according to Stein, refers to such Greek writers as had either written on Egypt, as Hecataeus of Miletus, or had caused the dissemination of incidental notices, like Pindar. Stein quotes similar noticing and criticizing of authors without direct mention being made of names: II. 16, 4; II. 20, 1; II. 45, 2; IV. 36, 6. In VI. 55, ἄλλοισι γὰρ περὶ αὐτῶν εἴρηται, ἐάσομεν αὐτά, . . Stein thinks reference is made to λογογράφοι like Hecataeus, Pherecydes, Charon. In II. 135, he mentions Sappho: ἐν μέλει κατεκερτόμησε κ. τ. λ.; in III. 32 he quotes a γνώμη from Pindar; in IV. 13 the Arimaspéa of Aristeas of Proconnesos; in IV. 32 Hesiod is mentioned, and the Homeric authorship of the Ἐπίγονοι is doubted; in V. 95 Alceus's escape at Sigéum is quoted from Alcaeus's own μέλη; the exhibition of Phrynichus's tragedy, Μιλήτου ἅλωσις is told with interesting detail, VI. 21; add also VII. 6. As for Aeschylus, his name is expressly mentioned only once—II. 156. Speaking of the view in Egyptian mythology that Bubastis (corresponding to Artemis) was the daughter of Isis (the analogon of Demeter), Herodotus goes on to say: ἐκ τούτου δὲ τοῦ λόγου καὶ οὐδενὸς ἄλλου Αἴσχυλος ὁ Εὐφορίωνος

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ἥρπασε τὸ ἐγὼ φράσω, μοῦνος δὴ ποιητέων τῶν προγενομένων· ἐποίησε γὰρ Ἄρτεμιν εἶναι θυγατέρα Δήμητρος.

Herodotus mentions (VI. 114) the feat of Cynegīrus, son of Euphorion, brother of Aeschylus, in the battle of Marathon. This must lead us to infer that Herodotus made especial inquiries amongst members of the family of Aeschylus—his primary interest, I should think, being in the person of Aeschylus himself.

It is very probable that, in his general dramatic arrangement, especially from the account of Xerxes's expedition on (Book VII. initio), Herodotus was not a little affected (consciously or unconsciously) by Aeschylus and the other Attic dramatists, and still more in the general moral and religious views in which the influence of Aeschylus is especially perceptible, as in the doctrine that the ὕβρις* of man is sure to be met by divine punishment. It is certainly noteworthy that, according to Kirchhoff, he resided at Athens during the composition of this portion of his history. True, he does not† mention Aeschylus at all in his entire narrative of that battle, of which the son of Euphorion was the veteran and the poet. Neither does he make any general allusion to the *Persae*—a reference, I mean, without direct quotation or mentioning the work or name of the author, so far as I can see. But the play of the *Persae*, if any of the Aeschylean dramas, had long ago become the common property of Athens; it had been exhibited (472 B. C.) some forty years before Herodotus wrote this portion of his book; and the *Ranae* of Aristophanes shows how thorough was the acquaintance with the tragedies of the ancient Μαραθωνομάχος which the general public in Athens must be believed to have possessed.

Wecklein, after making a comparative quotation of such passages in both authors as go to show that Herodotus derived much of his moral and religious treatment of tradition from Aeschylus, quotes passages where there seems to be

* Cf. especially the speeches and conversation of Artabanus in book VII.

† But any fine *argumentum de silentio* must fall to the ground. This is one of the occasions when we must recall the trite literary fact that ancient classic historiography intended to be, not a systematic exposition of documentary evidence, but a work of literary art.

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a resemblance even of language, which may perhaps be set down as reminiscences on the part of Herodotus:

{ Herod. VII. 16, 4, *σε...ἀνθρώπων κακῶν ὁμιλίαι σφάλλουσι.*
{ Pers. 753, *ταῦτά τοι κακοῖς ὁμιλῶν ἀνδράσι διδάσκεται.*

{ Herod. VII. 5, *Ἀθηναίους ἐργασαμένους πολλὰ δὴ κακὰ Πέρσας.*
{ Pers. 236, *καὶ στρατὸς τοιοῦτος ἔρξας πολλὰ δὴ Μήδους κακά.*

{ Herod., *δειμαίνω μὴ ὁ ναυτικὸς κακωθεὶς τὸν πεζὸν προσδηλήσεται.*
{ Pers. 728, *ναυτικὸς στρατὸς κακωθεὶς πεζὸν ὤλεσε στρατόν.*

The *Persae* begins to assume the tone of connected narrative from vs. 337. In Herodotus we may begin our review with VII. 75. Themistocles despairs of the willingness of the Peloponnesian portion of the Hellenic fleet to remain in the sound any longer; he sends his trusted slave Sikinnos to the Persian host to inform them of the intended movement of the Hellenic fleet. Now Aeschylus, from the truly Panhellenic and religious standpoint which he takes in the drama, foregoes the mentioning of any names on the Greek side at all. Herodotus then of course is more specific in this point as well as more exact. AESCH. Pers. 355:

*ἀνὴρ γὰρ Ἕλλην ἐξ Ἀθηναίων στρατοῦ*
*ἐλθὼν ἔλεξε παιδὶ σῷ Ξέρξῃ τάδε, κ. τ. λ*

But HEROD. VIII. 75: *ἄνδρα.....τῷ οὔνομα μὲν ἦν Σίκιννος, οἰκέτης δὲ καὶ παιδαγωγὸς ἦν τῶν Θεμιστοκλέος παίδων· κ. τ. λ.* The subsequent changes of position in the Persian fleet are presented as the immediate consequence of the wily Athenian's manoeuvre by both writers, AESCH. Pers. 364, sqq.

Regarding the *time* given by Aeschylus as a direct royal order (Pers. 364: "When Sol should cease to burn the earth with his rays"), Herodotus is more accurate, and I think consciously so. He may have gathered the more exact data from Athenian veterans of Salamis, possibly. He says (VIII. 76): "When midnight came" (*ἐπειδὴ ἐγένοντο μέσαι νύκτες, ἀνῆγον κ. τ. λ.*).

And now about the *position* of the Persian fleet:

(1.) What was it before the message of Themistocles? When the Persian fleet first came on from Euboea (VIII. 67 sq.), Phaleron was made the station of the fleet—council of war—order to move forward—the day too far advanced to make an actual attack: *ἀνῆγον τὰς νέας ἐπὶ τὴν Σαλαμῖνα καὶ*

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παρεκρίθησαν διαταχθέντες κατ' ἡσυχίην. This must mean along the coast of Salamis, and pointed towards the same. What portion of the coast? Of course opposite Salamis-town, where the Greek fleet lay in the harbor. At the same time the position of the Persian fleet, then, must have been one which left it possible for the Greeks to escape.

(2) What was it *after* Themistocles's message? In this matter the presentation of Grote will, I think, bear correction, as we shall see hereafter. The language of Herodotus is as follows (VIII. 76, 5): ἀνῆγον μὲν τὸ ἀπ' ἑσπέρης κέρας* κυκλούμενοι πρὸς τὴν Σαλαμῖνα, ἀνῆγον δὲ οἱ ἀμφὶ τὴν Κέον καὶ τὴν Κυνόσουραν τεταγμένοι κατεῖχον τε μέχρι Μουνυχίης πάντα τὸν πορθμὸν τῇσι νηυσί. Taking the attributive modification τὸ ἀπ' ἑσπέρης as a prolepsis coördinate to the following οἱ ἀμφὶ.....τεταγμένοι, which is Stein's view also, the rendering of the passage would be: "On the one hand they brought into position the wing destined for the west side (more accurately the northwest) of their line, effecting a blockade towards Salamis, and on the other hand those ordered to take position in the neighborhood of Keos and Kynosura moved into position, and thus held the strait entire with their ships up to Munychia."

Now Grote † professes to understand the first section of this movement, but not the second. He certainly misapprehends the meaning (viz. the temporal relation) of οἱ ἀμφὶ τὴν Κέον τε καὶ τὴν Κυνόσουραν τεταγμένοι. This, like τὸ ἀπ' ἑσπέρης κέρας, must be taken as a kind of proleptical statement, in which Herodotus has in view the actual condition resulting from the moving into position. From this πρῶτον ψεῦδος on the part of the eminent scholar results his rather wild topographical criticism. He will not admit any Keos or Kynosura on or near the coast of Salamis; he professes to know only of a cape Kynosura on the eastern coast of Attica, and of an island of Keos southeast of Cape Sunion (the well-known home of Simonides, Prodicos, etc.). In the first place he

* There was no necessity, of course, of adding anything like καὶ τὸ πρὸς τὴν ἠῶ, simply because that was already in position, being about the general headquarters of the fleet, the Piraeus.

† Chapter 41, Vol. IV., p. 476.

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confounds Κέος and Κέως.* Then, Κέος and Κυνόσουρα certainly were the names of points on the Salaminian coast, opposite the Piraeus. Between the latter harbor and the former points the Persians took position, reaching fully across, as Herodotus says.

The only difficulty would be about the duality of names, Κέος and Κυνόσουρα. Pape, and Stein likewise, take them as the two different names of the *same* cape. But they seem to have overlooked the repetition of the article ἀμφὶ τὴν Κέον τε καὶ τὴν Κυνόσουραν. Hence I for my part should prefer to join Spruner's view as expressed in his plan of the battle: Κυνόσουρα for the long "bill" (the English for the Greek "tail") running out straight eastwardly, and Κέος the next point jutting out, down the Salaminian coast.

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What now is the account of the eye-witness, Aeschylus?

These are his lines (which we may consider to be important) about the matter, given as the royal order, vss. 366, sqq.

τάξαι νεῶν στῖφος μὲν ἐν στοίχοις τρισὶν
(1) ἐκπλους φυλάσσειν (2) καὶ πόρους ἁλιρρόθους
(3) ἄλλας δε κύκλῳ νῆσον Αἰαντος πέριξ. . . . .

The words ἐν στοίχοις τρισὶν must, I think, be connected with the following epexegetical infinitives. Thus alone it would be made clear what the three στοῖχοι really are. The first line takes a position ἔκπλους φυλάσσειν, to watch the στενόν, the strait between the capes of Κέος and Κυνόσουρα, and the Attic harbor-line. The next στοῖχος is the one which takes the east side of the sound itself from the Piraeus to Eleusis, the πόρους ἁλιρρόθους. These two στοῖχοι tally exactly with the two lines of the Persian position indicated by Herodotus. He made the final composition of this portion of his narrative in Athens, about 429; how could he locate a cape of Κυνόσουρα and of Κέος when there were none? Besides, we cannot but think that in his first sojourn at Athens even before he went to Thurii, Herodotus must have closely studied and examined the region. It was the theatre, or a very prominent

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* See Pape, *Woerterbuch der Gr. Eigennamen*, s. v.

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part of the theatre, where the fifth act of the great drama was enacted which Herodotus even at that time must have comprehensively conceived *as* a drama. The naive piety of the Halicarnassian traveller certainly felt a lively satisfaction on account of the close correspondence of certain χρησμοί of "Bakis" with the actual events. But so far am I from dreaming with Grote that Herodotus was capable of arbitrarily saddling topographical names from the "Bakian" oracles on the coast of Salamis, that I do not hesitate to call such criticism absurd and unworthy of so eminent a scholar as Grote is, and I would rather assume, with Wecklein, that these oracles were in matter of fact *vaticinia post eventum.* The pious and reverential attitude of the good and honest historian I certainly do not share; at the same time I believe (as Wecklein does) that the names of places occurring in the "Bakian" oracles in the form then current agreed perfectly with the actual names of actual points on the Salaminian coast.

But we must return from this necessary episode to our comparative review. What about the third στοῖχος of Aeschylus:

ἄλλας δε κύκλῳ νῆσον Αἰαντος πέριξ ?

This is very probably the line between the western coast of Salamis and Megaris, the line mentioned by Diodorus (Ephorus) and Plutarch.*

As for the island of Psyttaleia, the Persians took possession of it at once, at the same time that their fleet moved into position. It was an important link in the chain of concerted measures. These were designed to make absolutely sure of nothing less than the annihilation of the Hellenes. The matter is mentioned by both, but in a different order by each author. Herodotus's office was not only to tell of events, but also to reconstruct their order. He gives it (76, 3, sqq.) as a part of the dispositions made by Xerxes in the night, immediately after Themistocles's message. But Aeschylus relates things as a veteran reproducing his recollections and impressions. He seems to retain the order in which things

* Diodorus, XI. 17. Plutarch, *Themistocles*, 12.

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came to his notice on the day of battle. It was the order in which the drama of the great engagement remained in his memory ever after. Hence in Aeschylus Psyttaleia is not introduced before v. 445, when he relates how the Greeks, in the advancing tide of their victory, made a descent on the island and slaughtered the Persians holding it. But at the same time there is expressed the original purpose of Xerxes in the measure (v. 450):

*ἐνταῦθα πέμπει τούσδ', ὅπως, ὅταν νεῶν*
*φθαρέντες ἐχθροὶ νῆσον ἐκσωζοίατο*
(1) *κτείνοιεν εὐχείρωτον Ἑλλήνων στρατόν*
(2) *φίλους δ' ὑπεκσώζοιεν ἐναλίων πόρων.*

Herodotus: *ἵνα τοὺς μὲν περιποιέωσι, τοὺς δὲ διαφθείρωσι.* Both authors indicate sunrise as the time when the ships began to move. Of Themistocles's stirring address Herodotus tells us, and describes its character (83): *τὰ δὲ ἔπεα ἦν πάντα κρέσσω τοῖσι ἥσσοσι ἀντιτιθέμενα ὅσα δή ἐν ἀνθρώπου φύσι καὶ καταστάσι ἐγγίνεται. παραινέσας δὲ τούτων τὰ κρέσσω αἱρέεσθαι καὶ καταπλέξας* ("winding up") *τὴν ῥῆσιν ἐσβαίνειν ἐκέλευε ἐς τὰς νέας.* In Aeschylus there is of course no mention made of Themistocles, the poet's description of the mutual exhortations of the Hellenes is poetic and ideal (v. 402):

"O, sons of the Hellenes, on!
Make free the fatherland, win liberty
For wife and child, our Gods' ancestral seats,
The tombs of our forefathers: all is now at stake."

Herodotus says that after the first mutual approach the other Greeks backed water in hesitation. But Ameinias of Athens, of the demos Palléne, rushed forward and made the first *ἐμβολή*. This personal notice Aeschylus of course omits, but also states distinctly (v. 409):

*ἦρξε δ' ἐμβολῆς Ἑλληνικὴ*
*ναῦς, κάποθραύει πάντα Φοινίσσης νεὼς*
*κόρυμβ'.....*

This latter statement Herodotus omits, and I cannot resist the impression that he does so because Aeschylus had already given this specification. Simply to repeat it he did not care.* From the same exact reference to Aeschylus I think

* Comp. above (IV. 55): *ἄλλοισι γὰρ περὶ αὐτῶν εἴρηται, ἐάσομεν αὐτά.*



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we must explain the positive side: he seems eager to supply deficiencies in the account of the poet, and to make his general statements more exact. He could easily get material for doing this by making inquiries amongst the Attic veterans around him.

Verses 412–432 are rather a general though vivid image of the course and character of the naval engagement than an enumeration of specific data and successive facts: at first the Persian fleet offered firm resistance (*ἀντεῖχον*), but soon became a helpless and inert mass on account of its excessive bulk, the ships impeding and damaging one another, while the Greek ships battered them from all around (*πέριξ ἔθεινον*) "as fishermen throw their harpoons into huge shoals of tunnies." Then the Persians all turn to flight, and the slaughter made amongst them is tremendous.

After this account there was indeed room for specifications on the part of Herodotus.

Thus (chapter 85) the deportment of the Ionians of the Persian fleet is described. Herodotus professes to know a goodly number of Ionic captains who took Hellenic ships, but he prefers, he says, to forbear mentioning any names excepting the case of two Samians. He further says that the Aeginetans and the Athenians destroyed more of the enemy's ships than did the other Greeks, . . . and of single men there distinguished themselves Polycritos of Aegina, Eumenes of the Attic demos Anagyrūs, and Ameinias of the demos Palléne, the two former probably *τριήραρχοι*, like the latter. Herodotus, friend of Athens as he was, did not share in the special hatred of Athens (432, sqq. B. C.) against Aegina, nor against Corinth and the family of the defender of Potidaea (431, sqq. B. C.), Aristeas of Corinth, son of Adeimantus. The Athenian contemporaries of Herodotus made the latter out to have fled ignominiously in the battle, with the Corinthian contingent: *τούτους μὲν τοιαύτη φάτις ἐχει ὑπὸ Ἀθηναίων, οὐ μέντοι αὐτοί γε Κορίνθιοι ὁμολογέουσι, ἀλλ' ἐν πρώτοισι σφέας αὐτοὺς τῆς ναυμαχίας νομίζουσι γενέσθαι· μαρτυρέει δέ σφι καὶ ἡ ἄλλη Ἑλλάς* (94). Of course Herodotus cannot but insert at some length (87–88) the fine episode of Artemisia's cleverness and

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coolness; he wished to give a permanent place to a local tradition, of which every Halicarnassian may well be supposed to have been proud. Other Herodotean episodes we may pass over. Such are the accounts of the rivalry between the Aeginetan Polycritus and the Attic commander and between Ionians and Phenicians before Xerxes. The next specific datum which suggests an instructive comparison between Herodotus and Aeschylus, is the massacre on Psyttaleia. Aeschylus is very explicit (454, sqq.): "Putting on strongly plated armor, they (the Hellenes) leaped from the ships, round about encircled all the island, so that they (the Persians) were at loss whither to turn, for they were being crushed with stones hurled at them, and arrows sent from the string of the bow struck and destroyed them; finally they (the Greek assaulting party) fall upon them in one rush, strike and cut to pieces the ill-fated men, until they had destroyed the life of every one." I am rather inclined to suggest that Aeschylus himself may have participated in this attack, which he describes with so accurate detail. Not much, it seems, was left to Herodotus to add or specify; he only gives us the special information that it was Aristides who headed the attacking party, and that the latter were "a good number of the heavy-armed stationed along the coast of Salamis." As for detailed description, Herodotus evidently omits to repeat the poet's account, but says summarily (95): *Ἀθηναῖοι.....οἳ τοὺς Πέρσας τοὺς ἐν τῇ νησῖδι ταύτῃ κατεφόνευσαν πάντας.*

Again, Aeschylus mentions the seat of Xerxes during the battle (466):

*ἕδραν γὰρ εἶχε παντὸς εὐαγῆ στρατοῦ*
*ὑψηλὸν ὀχθον ἀγχι πελαγίας ἁλός...*

Here again there was room for a specification, which Herodotus does not fail to give (VIII. 90): *κατήμενος ὑπὸ τῷ οὔρεϊ τῷ ἀντίον Σαλαμῖνος τὸ καλέεται Αἰγάλεως.** Another indication of this attitude of Herodotus towards the poet's account we meet in the following: Aeschylus (302 sqq.) gives the names of a

* For though Herodotus wrote at Athens, and drew many of such data from Attic tradition, the scope of the entire work was eminently Panhellenic, and written for *all* Greeks.

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considerable number of Persian leaders and nobles amongst the slain, and frequently adds data of a very precise nature.* Artembares, commander of ten thousand horse; Dadakes, commander of one thousand men; Tenagon, the champion of the Bactrians; Lilaeus, Arsames, Argestis, Arcteus of Egypt; Adeues, Pheresseues, Pharnuchos: these four from the same ship; Matallos, commander of ten thousand; Arabus the Magus; Artames the Bactrian; Amestris and Amphistreus; noble Ariomardes of Sardes; Tharybis, the admiral of two hundred and fifty ships, a Smyrnaean by descent; and Syennesis, the foremost in bravery:

> *εἷς ἀνὴρ πλεῖστον πόνον*
> *ἐχθροῖς παρασχών.....*

Of all this exact and varied information, Herodotus (89) repeats not a single item, but only says summarily: *πολλοί τε καὶ οὐνομαστοὶ Περσέων καὶ Μήδων καὶ τῶν ἄλλων συμμάχων (ἀπέθανον)*; but he adds one item to the list, and that a very important one: *ἀπὸ μὲν ἔθανε ὁ στρατηγὸς Ἀραβίγνης ὁ Δαρείου*, brother of Xerxes. I doubt not but that he would in this item give a very material supplement to the account of the poet, to which, in other respects, his own resources were unable to add anything.

As for the so-called "flight" of Xerxes, it was to the moral and dramatic conception of the historian the fifth act of the great tragedy. The Nemesis of the Gods is elaborated in this portion as much as was the *ὕβρις* of Xerxes in the seventh book, only eight years after the events.†

Aeschylus gives us verses which show how quickly tradition wrought out legendary and mythical elements, and how patriotic feeling added to the abject condition of the vanquished foe. Thus, 481, sqq.:

> *....οὐκ εὔκοσμον αἴρονται φυγήν.*
> *στρατὸς δ' ὁ λοιπὸς ἔν τε Βοιωτῶν χθονὶ*
> *διώλλυτ', οἱ μὲν ἀμφὶ κρηναῖον γάνος*
> *δίψῃ πονοῦντες, οἱ δ' ὑπ' ἀσθματος.*

* I cannot bring myself to join the view of Grote, and that of Teuffel in his commentary; many forms of names I admit may have been treated with poetic license. If all this was nothing but poetic figure-painting, Herodotus, from his general habit and from his accurate knowledge of Persian language and history, would not have failed to give some criticism of it.

† See Wecklein, l. c., p. 250, sqq.

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And 489, sqq.:

> καὶ Θεσσαλῶν πόλεις ὑπεσπανισμένους
> βορᾶς ἐδέξατ'· ἐνθα δὴ πλεῖστοι θάνον
> δίψῃ τε λιμῷ τ' ἀμφότερα γὰρ ἦν τάδε.....

Here again Herodotus makes additions from the ample stores gathered by him from tradition: the army of Xerxes not only consumes all the stored grain wherever it passes through (for that alone καρπός can be in the late autumn), but they also, when that was exhausted, turned to the grass and the leaves and bark of trees, and likewise devoured all tame and wild animals, and left nothing; this they did, Herodotus adds with charming epic breadth, from hunger. The diseases of which the retiring host is made to suffer in Aeschylus, Herodotus increases: ἐπιλαβὼν δὲ λοιμός τε τὸν στρατὸν καὶ δυσεντερίη κατ' ὁδὸν ἔφθειρε. Herodotus himself believes in this tradition. If he had not he would have introduced it in the way he does the story (118) of Xerxes's setting sail from the mouth of the Strymon for Asia direct: ἔστι δὲ καὶ ἄλλος ὅδε λόγος λεγόμενος, ὡς etc., to which he adds negative criticism of his own (119–120).

What quick work oral tradition makes with facts, how it adds, subtracts, invents, buries, paints over, can be seen in a hundred ways in the tradition which Herodotus met with and honestly gathered. This is all well enough; but that within eight years after 480 such a story as the freezing over of the Strymon could be started is remarkable (Persae, 495):

> .....νυκτὶ δ' ἐν ταύτῃ θεὸς
> χειμῶν' ἀ ω ρ ο ν ὡρσε πήγνυσιν δὲ πᾶν
> ῥέεθρον ἁγνοῦ Στρυμόνος.

But what follows plainly exhibits the handiwork of pious bias, and with Goethe: "Man merkt die Absicht und man wird verstimmt."

> θεοὺς δέ τις
> τὸ πρὶν νομίζων οὐδαμοῦ, τότ, εὐγετο
> λιταῖσι γαῖαν οὐρανόν τε προσκυνῶν.

The army begins to cross over, but the sun melts the ice and many are too late and go down. Thirlwall believed this. I should not go further than believing that Aeschylus believed

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it. Grote refuses to join Thirlwall. Of course. The best criticism is the absolute and very significant silence of Herodotus about this τέρας, the untruthfulness or impossibility of which to him, the experienced traveller, was too patent. But as for the great king's arrival back at the threshold of Asia, the Hellespontus, both authors leave to him but the barest remnant of an army (Pers. 510):

> ἥκουσιν ἐκφυγόντες, οὐ πολλοί τινες
> ἐφ' ἑστιοῦχον γαῖαν· . . .

and Herodotus (VIII. 115): ἀπάγων τῆς στρατίης οὐδὲν μέρος ὡς εἰπεῖν . . . . . The patriotic and religious idea demanded that the contrast between the first crossing and the second should be made to appear as strong and striking as possible; no one should fail to see how the ὕβρις of man cannot escape from the φθόνος and from the Nemesis of the Gods. And thus we may leave* the subject.

* While engaged in arranging the material of this series of observations, I happened to meet, in a late number of Fleckeisen's J B. '77, a brief paper by G. Loeschke: "*Ephoros-Studien.* I. *Die Schlacht bei Salamis.*" To consider the authority and value of Diodorus it need not be said was beyond the scope and limit of this paper. Loeschke takes up Diodorus XI. 18, which seems to say that the Greeks, *in sailing out of the sound*, met the Persians, whose line of battle had never been drawn up in any part of the sound itself. The amount of twisting and Procrustean treatment to which Aeschylus, and especially Herodotus, are subjected to gain some kind of semblance to this view is enormous. The fictitious results will hardly stand, and the entire paper is a painful effort of self-deception.

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## X.—*The Principle of Economy as a Phonetic Force.*

By W. D. WHITNEY,

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From the very beginning, early in this century, of the scientific study of Indo-European language, the history of the phonetic form of words has taken a leading place as subject of investigation. And from the beginning, also, has been recognized as a principal factor in that history, a tendency to economy, to the saving of effort, in the work of articulate utterance. It might not be easy to tell precisely how and by whom the recognition was first made, and by what steps it arrived at distinct formulation. Perhaps its inception lay, as much as anywhere, in Bopp's demonstration of *i* and *u* as "lighter" vowels than *a*. As a matter of scientific history, the question is not without interest; but I do not propose to enter into it at the present time. Enough for our purpose that the law of economy, as we may call it, has established itself in current linguistic science as the one most unmistakably exhibited, and most widely and variously active, in the transformations of the external form of speech: some, indeed, are prepared already to pronounce it the only existing or possible one. Among these are (as is natural) included not a few of those whose way it is to make easy and confident solutions of difficult questions. Like every other popular dogma, this has its unintelligent partisans and defenders. It would not be hard to cite striking examples of scholars whose application of the law is purely mechanical—who, for example, deduce empirically the prevailing order of succession of sounds in phonetic growth, and then cast about for reasons why the later sound may be declared easier than the earlier; or who endeavor to account for intricate and puzzling phenomena, like the Germanic rotation of mutes, by an arbitrary and baseless classification of the mutes in respect to intrinsic difficulty of utterance. There is hardly a possible abuse of the principle which has not been exemplified in recent discus-

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sions of language. And then, by a natural reaction, there have been and are those who deny not only the exclusive domination of the law, its power as a universal solvent of phonetic difficulties, but also its predominant importance, if not its very existence. Perhaps, therefore, a brief discussion of some of the matters involved may be found not untimely or undesirable.

It is evident enough, we may remark at the outset, that those who carry their skepticism so far as to refuse to the principle of economy at least a first-rate place in the external history of speech, display an unreasonableness not excelled by that of the most unenlightened partisan of the principle. Its existence and effects lie upon the very surface of the best understood facts of language. Nothing else is needed, or can be devised, to account for the whole body of phonetic changes falling under the two heads of abbreviation and assimilation. And—especially if we give the latter its full extension, as will be pointed out farther on—this includes the great mass of phonetic changes: those that remain are, whatever their importance and interest, the comparatively rare exceptions.

As much as this, too, may be inferred on appealing to what we know of the processes of the transmission, acquisition, and use of speech. These are matters now sufficiently understood to make them a fair test of the admissibility and adequacy of any general principle claimed to exercise a wide influence in linguistic history.

At present, and as far back in the life of language as our historical researches carry us, every living tongue has been kept in existence by a process of learning, of apprehending and reproducing what was already in currency. The child—and, in his own way and measure, the adult also—hears words and phrases which have come into use he knows not how, and which are brought to his sensorium by a physical agency totally obscure to him; and, when he understands their meaning well enough to use them himself, he reproduces them, as well as he is able, by a physical apparatus which operates, it is true, under the direction of his will, but of whose construction and mode of working he as a child knows nothing, and as

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an adult very little more. By experience, the possessor and manager of this apparatus acquires great dexterity in the execution of familiar movements; any combination of sounds accordant with those to which he is accustomed he becomes able to imitate with wonderful exactness. But he labors under two disabilities, of which one diminishes and the other increases with his growing age. Until experience has given dexterity, much in utterance is found difficult; the young learner bungles his first speech-imitations terribly, even to the extent of being wholly unintelligible, except to those who know him best. Some sounds are harder to catch and reproduce than others; and it would be practicable, and highly interesting, to determine by a wide observation and deduction what is the general scale of difficulty of acquisition among alphabetic elements. A certain degree of difference would be found between individuals: whether also between communities or races is a much more difficult question: I know of no facts which should lead us to expect to find it of appreciable amount. In general, certainly, it would be found that the sounds, and even the combinations, of all the various languages would be learned with practically equal ease, on an average, by speakers of any and every kindred. It is even more in the combinations than in the individual sounds that the difficulty of reproduction lies—in the quick and nice transition from one articulating position of the organs to another. The child, like the adult learner of a new language, is "thick-tongued" at first, and, even when he can speak correctly, cannot speak rapidly.

And then, the perfection of his conquest of this difficulty ushers in the other. He has begun with being equally awkward, and equally able to overcome his awkwardness, in dealing with the phonetic structure of any language; but when he has schooled his organs to the adjustments and changes required by one system of sounds and combinations, he is less able to adapt them to those required by another; and this new disability, the positive result of habit, grows with every added year of practice, until, after arriving at a certain (not exactly definable) age, one is utterly unable to



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acquire otherwise than rudely the pronunciation of a strange language.

Thus the attitude of every speaker toward the language which he uses is simply this: he hears, by means which he does not comprehend, signs whose reason is a mystery to him, and, by an apparatus of unknown character in his own throat and mouth, reproduces those signs, at first imperfectly, but later with exactness. Of the *rationale* of the whole process he is both ignorant and careless; to him the practical result is alone of importance. What he knows and realizes is that by such a process of action he makes himself understood by others, even as he understands them; of the advantage which his own mental acts derive from the possession of this instrumentality he is, for the most part, wholly unconscious.

The question is, now, how there should ever come about any change in the uttered form of the signs thus learned and reproduced.

And I think it must be sufficiently clear, in the first place, that to ascribe to sounds themselves an action of change, or a tendency to variation, in any other than a figurative sense, or for brevity (as when we say that the sun rises), is wholly destitute of reason; it is a retrogression from the scientific method to the mythological. Sounds are the audible results of the acts of human beings, and of acts which have no instinctive character (though, like everything else made habitual, they may come to be performed with absence of reflection), but are made by volition, in imitation of the similar acts of others. They can suffer no alteration which has not its ground in the action of the human will. And such action is always determined by motives—motives, often, which are not present to the consciousness of the actor, but which may nevertheless be brought to light and demonstrated. What we have to seek, therefore, is the motive or the variety of motives underlying the acts of men in the phonetic changes of speech. There is no question here of a difference of human capacities, making one individual unable to reproduce with accuracy the sounds made by another. Apart from rare individual peculiarities, of habit oftener than of constitution, of which the effect is

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completely lost in the accordant action of the community, the form of every word as at present used is capable of being perfectly learned and reproduced, and that from generation to generation; there is in the nature of things no necessity that it should ever change; and it never will change if there be not some inducement to its alteration, of a kind that is calculated to affect human action, being either identical or akin with motives that are found operative also in other departments of human action.

It does not need to be pointed out how entirely different all this would be, provided our sounds and their combinations were inherently significant; provided we made them as they are because our mental and physical constitutions are so correlated that certain particular movements of the mind lead naturally to certain particular movements of the organs of speech. Then, of course, changes of significance would be the motives that led to changes of form, and the latter would be the record in which we should study the former. It may be added that, as each person's conceptions are somewhat unlike those of every other, and are all the time changing with his changing knowledge and character, there could neither be unity of speech in a community nor persistency in an individual; the diversities and fluctuations of every language would be illimitable.

As things actually are, it is hard to see what motives can be brought to bear upon the outward framework of language save such as are connected, in one way or another, with increased convenience of use—all of which may be conveniently and fairly summed up in the one word "economy." All changes, indeed, both internal and external, are for the purpose of increased convenience of use; it is not, however, the part of phonetic change to provide new material for the expression of thought; but only to take what is provided in other ways and work it over into more manageable shape. Changes of form are not entirely unproductive of new material—as when phonetic variants of the same word are turned to account by being made to fill different offices: but such things are not only exceptional, they are also inorganic, unin-

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tended; they are happy accidents. The almost exclusive direction of movement in phonetic history is toward demolition and decay. Words which had been made up of separate elements first lose their etymologic distinctness, then are fused together, and even shrink into fragments of their former selves. Signs of modification and relation, made in the first place by phonetic change out of independent words, are worn out and drop off again. And what is true of words is also true of the elements which compose them. Mutual adaptation of sound to sound, with rejection of what will not adapt, is the prevailing law. By processes which are completely explainable as results of the tendency to economy, whole classes of sounds are lost from a language or are converted into others.

Just how widely this tendency works, what are the limits to its action, where the line is to be drawn between its effects and those of any other tendency or tendencies, or whether there are such other tendencies, no one has the right to claim to decide at present. That there are phenomena in phonetic history which have not yet been traced to the economic force, and which seem to offer little prospect of ever being so traced, is true enough. But this is by no means equivalent to saying that they never can or will be brought under it. While they resist, they forbid us to maintain with confidence —still more, with dogmatism—that convenience of use, in the form of economy of effort, is the demonstrated sole force at work, and suggest that other minor tendencies may be brought to light; but it will be quite time enough to accept those others when they shall be clearly made out.

The objections hitherto raised, in appearance, against the principle of economy itself have really only lain against the misunderstandings and abuses of that principle—which are common and conspicuous enough. Let us look to see some of the things involved in it.

In the first place—as a matter so much of course that it hardly needs to be pointed out—we have to avoid carefully any views which should imply a conscious and intended economic action on the part of the users of a language. No

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than the differences of intrinsic ease. Hence, apart from abbreviation, almost all phonetic history consists in adaptation; and this is mostly assimilation, although in special cases it may be dissimilation likewise; it may involve omission for the relief of a difficult combination, or, on the other hand, insertion of a transitional sound—and so on.

The phenomena ordinarily reckoned as assimilative are too familiar to be worth illustrating; but there are others, less generally recognized as belonging to the same class, to whose consideration a brief space may well be devoted.

We are wont to call our human speech "articulate," and to regard the fact that it is so as its most fundamental and distinctive characteristic. And this with good reason; only there are few who can tell what they really mean by *articulate;* and even many most reputable authorities are unclear or mistaken in their apprehension of the term. Articulation does not at all signify production by certain definite successive positions and actions of the organs: all utterance, human or brute, is of that nature; musical utterance would admit the same definition. Articulation is in reality what its etymology makes it: the breaking up of the stream of utterance into distinct parts, into *articuli* or 'joints'—which joints are the syllables: articulate and syllabic are essentially synonymous with each other. And the syllabic effect is produced by the constant alternation of closer and opener utterances; the closer, or consonants, serve as separators, and at the same time connectors, of the opener and fuller vocal tones, or vowels. The vowels are the main audible substance; but the aid of the consonants is required to give it articulate character: these divide it into individual parts, separate, but indefinitely combinable. Hence the transition from the close or conso-nanta[illegible]ons to the vowel positions, and the contrary, is onst[illegible] it is a fact of the very first consequence in [illegible]e [illegible] history of speech. For, in its performance, [illegible] vantage is gained by making the transitional ov[illegible]ter, by reducing the vibrating distance of the [illegible] s to say, by shutting less closely the organs [illegible] mediately to open again, and by opening less

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widely the organs which have immediately to close again. It is only when we give it this interpretation that we can accept as of any force or value the principle often laid down—that the utterances least remote from the medial or neutral position of ordinary breathing are easiest to make. That utterances of this class are easier in themselves, or in isolated use, is disproved by the testimony of young speakers, of early alphabets, and of the ruder existing alphabets. But when the power of swift and ready utterance is acquired, implying a degree of rapidity and accuracy of movement in the organs of speech which appears wonderful and almost incredible to one who looks at it closely enough to see what it is, then the amount of departure each way from the medial position becomes an element of importance. Then the medial sounds, though harder for the untrained speaker to catch and imitate, are found by the advanced and dexterous speaker a lightening of his task. No other reason than this, I believe, can be given why the *a*-sound (of *far*), which is the openest of the vowels, tends always to pass into the closer *i* and *u*, either directly or through the intermediate *e* and *o;* while, by an apparently contrary but really coincident tendency, the mutes are converted into fricatives: and so the medial classes of the alphabet are filled up. Sharpness of distinction and full resonance of tone thus give way to greater pliancy, smoothness, and ease. And the movement is evidently capable of being carried to the extreme of indistinctness and dimness; there is no necessary limit to the destructive action of the economic tendency; as it may strip a language once highly synthetic of nearly all its inflectional apparatus, so it may also reduce a clear and full phonetic structure to something approaching the mumbling murmur of one who is trying to speak faster than his organs will let him.

There is not in the phonetic history of our family of languages a movement of more constant action and wider reach than this. And its essentially assimilative character is obvious. It is a mutual assimilation of vowel and consonant: each great class exerts an influence to draw the other toward itself; the vowels are made somewhat closer or more conso-

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nantal, while the consonants are made somewhat opener or more vowel-like. I have pointed out in another place (above, p. 57) that a similar assimilative character belongs also to the ordinary interchanges of surd and sonant; thus, and thus only, are they to be brought under the action of the economic tendency; they stand in no natural and inherent relation of comparative ease or difficulty.

In the third place, while we may expect considerable accordance among different languages in the wider and more general results of phonetic change, there is nothing in the law of economy which should necessitate a correspondence in details. The minor movements depend on peculiarities of habit which can neither be prescribed nor foreseen, because they involve as an element the freedom of human action. Such peculiarities may be initiated no one knows why or how —by accident, as we say: and, from wholly insignificant beginnings, they may grow, with the aid of circumstances and under the shaping influence of other habits, into something very definite and marked; and, in their turn, they may exert a shaping influence on other habits, and lead to consequences which shall seem quite out of proportion to their own importance. In learning how movements of this character go on, the minute study of living modes of utterance, especially in what we call their dialectic varieties, will doubtless be of essential assistance; it is perhaps the most important result for the study of language which is to be expected from the modern science of phonology. But neither this nor anything else will do more than enable us to follow with fuller appreciation the recorded facts of linguistic history. The varieties of linguistic growth will always be of the same character as other varieties of historical development: incorporations of the varieties of human character and capacity, working themselves out under direction of the varieties of circumstance; to be traced out with more or less thorough comprehension, but not to be determined *à priori*.

If the law of economy be properly understood, it will be found fairly liable to none of the objections brought against it, and possessed of nearly all the importance ever claimed in



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its behalf. At present there appears to be no prospect that any other having the tithe of its importance will ever be put alongside it. We have, however, only to wait patiently to see what, in this respect, the future will bring forth, content with noting the absence thus far of any hostile or rival principle.

## XI.—*Did Der Von Kürenberg Compose the Present Form of the Nibelungenlied?* *

By FRANKLIN CARTER,

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In 1857 the late Moritz Haupt, then Professor in the university at Berlin, published under the title "Des Minnesangs Frühling," the edition of the early lyric poems of the Minnesänger which Lachmann had projected, and upon which he had expended, during the latter part of his life, a good deal of labor. The book appeared as the combined work of Lachmann and Haupt. In this volume there are fifteen strophes under the title "Der von Kürenberg," taken with their title from the manuscript of early German songs in the National Library at Paris. Thirteen of these strophes are in the metre of the Nibelungenlied, and there has been for some years a growing tendency among the littérateurs and scholars of Germany to impute the authorship of this poem, as we have it, to the von Kürenberg who is supposed to have written these strophes. Among those who have been leading champions of this opinion are Pfeiffer, Professor in the Vienna University, who died in 1868, and Bartsch, still Professor in Heidelberg. It was in 1862, in a session of the Imperial Academy at Vienna, that Franz Pfeiffer advanced his "scientific" proofs for this authorship of the poem, and

* This paper was prepared for the Society's meeting in 1876, but the writer was unable to attend the meeting.

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this event has been regarded as the first authoritative claim for the von Kürenberg, and certainly that was the beginning of the vigorous discussion of the question. But Dr. Vollmöller, whose essay on the newest theories concerning the origin and composer of the Nibelungen poem received a first prize from the Tübigen University in 1874, calls attention to a passage in F. J. Mone's treatise on the National German Poets of the Middle Ages, published in his "Badisches Archiv" at Karlsruhe in 1826, wherein the verses of the von Kürenberg are discussed as follows: "One single poem by this author has been preserved, to which evidently the melody has secured a long life, since this is set forth in the fourth strophe as something distinguished. For us the poem and contents are of the greatest importance. For the composition is in the epic measure, in the long four-lined strophe as in the Nibelungen, and the contents are in the spirit of the epos, with its figures and forms of speech. In both respects a remarkable poem, since it stands on the border between the epic and lyric poem, and shows us how the latter issued from the former. It is indeed, when one reads the poems of the Kürenberger, as if one had in hand an epic poet of the good old time, as for instance the poet of the Nibelungen, so similar are they in spirit and language." This language was used by Mone forty-one years before Pfeiffer's discourse at Vienna. This discourse, printed in the collection of essays from the pen of its author, entitled "Freie Forschung," aims to prove that the author of the Kürenberger-strophes and the present form of the Nibelungenlied are identical, but there is no reason to suppose that Pfeiffer had read the passage from Mone. Bartsch, in his investigations on the Nibelungen, says (p. 369) that the hypothesis of Pfeiffer, "wird beinahe zur Gewissheit," becomes almost a certainty. It may be noted in passing that Bartsch, when replying in the Germania, 1874, third heft, to a sharp attack from Scherer in Vol. 17 of the "Zeitschrift für Deutsches Alterthum," on the arguments of Pfeiffer and Bartsch for this identity of authorship, says: "I have never spoken of a demonstration, only of a high degree of probability." It may be doubted whether the words "becomes

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almost a certainty," do not justify Scherer's statement that Bartsch claims a demonstration. But certainly the question of the identification of the two authors, the von Kürenberg and the composer of the Nibelungen, is one of great interest. The number of strophes under the name "Der von Kürenberg" in the same metre as the Nibelungenlied is thirteen. It is then proposed to identify the author of a nameless epic containing twenty-three hundred strophes with the author of a lyric poem containing only thirteen strophes, on the ground of internal resemblances. It is a venturesome proposal, but there is considerable plausible evidence for accepting this identification.

The first argument and mainstay with Pfeiffer for this identification is the existence of a conventional law or usage until the middle of the thirteenth century, that no epic poet should employ the metre invented by another poet. That such a law existed in the thirteenth century with respect to *lyric* poetry is undoubtedly true, but the existence of such a law with respect to the *epic* poetry of the last part of the twelfth century or the first part of the thirteenth cannot be proven. Even to the assumption of the sweeping authority of such a law over the *lyric forms of the twelfth* century there is objection, and Wilmanns (who is referred to by all as authority), page 30 of his introduction to Walther von der Vogelweide, gives several instances of the borrowing of metrical form among these early lyric authors. Fischer endeavors in his appendix to "Die Forschungen über das Nibelungenlied," to show that most of the cases cited by Wilmanns are instances in which different authors have hit upon the same metre, as a variation of the old German metre of Otfried and others, where each line is divided into two half lines of four hebungen each. But even were this conceded, there is nothing to show that the metre of the Nibelungen poem is not also one of the early variations from that of Otfried. Attention is called by Fischer to the fact that most of the epic productions are in a variation of this Nibelungen strophe, and that therefore such a usage or law as prevented one poet from writing in the metre of another, must have been in force in the epic circle. But in at least three of the

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epic poems we find exactly the same strophe, and in regard to two of these three (König Ortnit* and Alpharts Tod) there is irrefragable evidence that they belong as early as the first fourth of the thirteenth century. That these two poems may not be allowed to have weight against the supposed law, two additional suppositions are devised to support the original supposition. First, the Alphart is assigned by Bartsch as a third poem to the authorship of Der von Kürenberg, and second, the Ortnit is pushed forward to a time when the supposed law is supposed to have lost its force. Did such a law ever prevail in the epic circle, it should naturally prevail at the time when the nnmber of epic poems produced was large and individual eminence was coveted, and not in the days of comparative unproductiveness. That the Nibelungen was one of the earliest of the well-known poems, the modification of its strophe to many different forms attests. But these different forms may be well explained, not as the result of a systematic ownership or copyright among epic poets, but as the natural attempts of an increasing and developing art to invent variations on a well-known strophe—the common property of all poets.

We have then, in order to accept the law respecting borrowing so confidently laid down by Pfeiffer, to modify it by two suppositions that greatly lessen our ability to accept it. These are, first, that the author of Alpharts Tod is also the von Kürenberg whose identity with the author of the Nibelungen poem this law would compel us to admit, and second, that the law in force at the end of the twelfth century had lost its authority at the time of the composition of König Ortnit, or in other words that König Ortnit cannot have been composed until the law had lost its force. Furthermore the existence of such a law, implying great effort to attain individual renown, would certainly have called out a far greater variety of strophe-form than we find in the epic circle, as it did call forth this variety among the lyric poets. Bartsch, fully admitting the difficulties that surround this

* Müllenhoff, in Haupt's Zeitschrift, Vol. 13, pp. 185–92, proves this in regard to Konig Ortnit. It is admitted by the Kürenberg advocates in regard to Alphart.

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supposed law, does not rest mainly upon it, but still adheres firmly to the Kürenberg hypothesis from reasons of an equally internal sort.

In one way the arguments from internal evidence in regard to such a poem as the Nibelungenlied are of great value, and that is in determining its unity or compositeness. It seems inevitable that any one who studies the poem seriously should believe that it has many interpolations. He who so believes cannot accept an argument from hebungen and senkungen, from verbal or grammatical forms, until he is sure that the strophes used as arguments, wherein these peculiarities are found, are all from one author. In other words, the subject matter and its presentation have a higher weight and authority than the form of presentation. He who assumes that the numerous inconsistencies in the details of the story are of small account against a unity of authorship, and then proceeds to determine the author by arguments between the language and metrical usages of this poem and that of some almost nameless and unknown poet, would seem to be straining at a gnat and swallowing a camel. Nevertheless it is worth while to note in what particulars besides strophe-form the verses of von Kürenberg agree with those of the Nibelungen poem, and even to weigh the force of these agreements.

Bartsch lays some stress upon the argument that both poems originated in the same region. There has been a wide difference of opinion in regard to the geographical relations of the Nibelungen poem, but there is good reason for conceding that the writer, or *some of the writers* of the Nibelungen were familiar with the region where the von Kürenbergs lived. The preponderance of authorities now assign the origin of the present form of the epic to Austria, on account of the geographical knowledge of Austria which the poem exhibits, and no objection to its origin there on the ground of linguistic coloring can have much weight. The von Kürenbergs belonged to the region of the Danube in Austria.

In regard to the *time* of the composition of the poem under discussion there is still greater divergence of opinion. Even some who accept as of great worth the laborious investigations

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of Bartsch on the Nibelungen poem, cannot admit his claim that the original German poem, of which we have a revision at two removes, runs back to 1140, his time for von Kürenberg. Zarncke has claimed that our Nibelungen poem cannot antedate 1130 because of the allusion to the Landgraves of Thüringen in strophes 2005–6, for the lords of Thüringen were exalted to the dignity of landgraves with great pomp in that year. But here we may note the danger which follows a dependence for argument on any single strophe. It is probable that strophe 2005, even though Lachmann did not so regard it, is an interpolation, and then all that could be claimed amounts to this, that the interpolation did not precede 1130. But in any case this allusion would favor quite as strongly a composition as late as 1170, as one so early as 1140, if Landgrave Irnfried is to stand for any historical character in the original poem. But Holtzmann (whose science partook a little of the sensational) placed the von Kürenberg much earlier, was disposed to identify him with Conrad the clerk, secretary of Bishop Pilgrim of Passan referred to in the Klage, who belonged between 971 and 984, and claimed that there are features in the Kürenberg strophes that identify his language with that of the Hildebrandslied and Notker's version of the psalms! Lachmann, whom Scherer follows, did not push the von Kürenberg back of 1170, and accepting the agreement of language and expression between his strophes and those of the Nibelungen poem (which agreement is by some strongly denied), the agreement in metrical form, and the somewhat general consent that the origin of our manuscripts of the Nibelungen poem is to be fixed at 1190–1210, there seems to be reason for the adoption of a date as late as Lachmann requires. Even then there is an opportunity to allow twenty or thirty years for the great changes that Bartsch claims to have proved by a comparison of manuscripts took place before the original composition of the Nibelungen in a German form assumed the shape of our present manuscripts, though of course Bartsch would either object to a difference of twenty years in origin for the two poems, or hold that this period is not adequate to account for

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the changes that he claims to have proved. But the general tendency among investigators is to admit a tolerably close coincidence in time for the *origin** of the Nibelungen, and the strophes that pass as composed by von Kürenberg. That the metrical form of the poems is in general identical, is involved in the hypothesis of Pfeiffer, and that there is a certain resemblance in the use of rhymes and in the omission of senkungen† has been shown by Bartsch. But one element in Bartsch's demonstration rests upon the acceptation of rules for the division of lines into accented and unaccented syllables, which revolutionize the metre of the strophe as Lachmann taught it. Many accept these rules, but they belong yet in a region of controversy. Even if it were established that the senkung generally fails between the second and third hebung in the eighth half-line, as is certainly the case in several of the strophes assigned to Von Kürenberg it carries the agreement no farther than the admitted general similarity. It is indeed an agreement in a minute particular, but for all we know, as Scherer suggests, that may be a natural, an organic evolution in the metre. That of the eighty strophes composed in the Nibelungen metre and interpolated in the Gudrun, forty-two exhibit the same omission of the senkung between the second and third hebungen would hardly be advanced as an argument that the von Kürenberg inserted these eighty strophes. On the contrary the fact indicates that even from the pen of a clumsy bearbeiter the omission of the senkung in this position came as a natural element in the strophe.

Much has been made by the champions of the theory under consideration of the coincidences in verbal expression between von Kürenberg and the Nibelungen poem. Similarities were adduced by Pfeiffer in his original discourse, and are to be found on pp. 25–28 of his Freie Forschung. Additions were made by Thausning, and Bartsch supplements these with

* The word origin would here mean for Lachmannites original songs, and for the Kürenberger advocates originals of present manuscripts.

† It ought to be noted that the omission of the senkungen varies in the different manuscripts. A has much more frequent omissions than either B or C, and B more than C.

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further contributions in his Untersuchungen, pp. 362–3. It does not seem noteworthy that the twenty-three hundred strophes of the Nibelungen should yield a large number of forms of expression found in the thirteen strophes of von Kürenberg. Dr. Volmöller has (pp. 16–33 of his prize essay) paralleled nearly every one of the coincidences adduced by Pfeiffer, Thausning, and Bartsch by quotations from other Middle German poets early and late. Is it not true that such coincidences of expression are not merely frequent, but inevitable, in an age when printing is unknown and the only means for the community of knowing poetry is hearing it, when literary influence is purely personal and plagiarism difficult to prove? Did not "The Flower and the Leaf" creep into the ranks of the Chaucerian poems and remain unchallenged there for a hundred years on the simple ground of coincidence of expression and style? By no means is it here claimed that "The Flower and the Leaf" is not a Chaucerian poem, but attention is called to the fact that able scholars object that this ground of proof for a Chaucerian authorship is insufficient. How many of Hoccleve's poems might be referred to Chaucer on the grounds whereby we are requested to accept von Kürenberg as the author of the Nibelungenlied? It is a pleasing study to seek the evidences of resemblance between the strophes of von Kürenberg and the Nibelungen. It gratifies the love of order and unity to assign the lyric verses to his youthful ardor, and the momentous story of the Burgundian tragedy to his later years. Perhaps there is no impossibility involved in the production by the same poet of these two differing poems. But never were lyrics more subjective than are those of von Kürenberg, and never was story more objectively told than is the death of the Burgundian heroes. There are lingering, descriptive traces here and there in the epic, but the descriptions are outward, concrete, not marked by the inwardness of lyric emotion, and never do we get a glimpse of the epic poet's personality. The poets may belong together in time, in geographical region, as they do in rhythm, but in the absence of all critical knowledge of either it can never be more than an ingeniously defended hypothesis



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that they are one. May they not belong together rather, as do Ben Jonson and Herrick, or Chaucer and Hoccleve, than be identical?

Scherer has made a point which it is very difficult, if not impossible for the identifiers to overcome. The groundwork was suggested by one of Haupt's notes to the strophes assigned to "Der von Kürenberg" in Des Minnesangs Frühling. Haupt conjectures that the title in the Paris manuscript may have been deduced from the strophe in which a lady is represented as saying: "I heard a knight singing extremely well in the Kürenberger's metre." Scherer regards that inference of title as certain, inasmuch as there is a variation from this metre among the Kürenberg strophes, so that we have two metres under the title Der von Kürenberg. Now if there was one metre distinctively and technically called "Kürenberges Wîse," and *but one*, as that term implies, there is not a vestige of *proof* that either of the two classes of strophes was composed by von Kürenberg himself. No exception can be taken to this position, and the assertion of Bartsch and Fischer that such a view removes all the poetry from the interpretation of the strophe in which the expression occurs may be true, but it is not poetry, or invention, or prettiness for these strophes that is sought here, but exactness in the application of the term "Kürenberges Wîse." It is then true that we do not know that *von Kürenberg himself* wrote one of these strophes. Do we even know that he invented either metre here used? Not absolutely, but we find some reason to believe that the second form of strophe, the one marking the lines in which Kürenberger wîse occurs, is the characteristic metre of von Kürenberg. In the first place its use in the strophe by the lady singing in response to the knight *who sang in it*, makes it probable that this is the "Kürenberger wîse." The lady sings (Des Minnesangs Frühling, p. 8. 3–8): "I heard a knight singing extremely well in the Kürenberges wîse: he must leave for me the land, or I will give myself to do with him." Then its use in the strophe of the knight, when he replies, gives further reason for regarding it as the Kürenberg metre. He replies (p. 9.

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29–36): "Now bring me very quickly my horse and my armor, for I must abandon the country for a lady. She intends to compel me to be devoted to her. She must ever be without my love." Here the same knight speaks whom she has heard sing in the "Kürenberges wîse." He replies in the same metre which she uses, probably the one used previously by him. It is the Nibelungen metre, probably the one that the lady calls "Kürenberges wîse," but it by no means follows that von Kürenberg himself wrote these strophes. On the contrary the title Der von Kürenberg *over two forms of strophe*, and the use of the term Kürenberg's metre, are inconsistent facts, and the natural conclusion is that von Kürenberg did *not write both classes of strophes* here printed, and that probably he wrote no line of this poetry, but simply invented and held a not very exclusive copyright to one of these two strophe-forms. But if von Kürenberg did not write these strophes, may not he who wrote them be the author of the Nibelungen poem? No; for if, as seems certain, the author of these strophes borrowed Kürenberg's metre, why may not another borrowing have taken place for the Nibelungen poem? Is it not more natural to suppose two, and possibly fifty borrowings, rather than a borrowing by a single author, for poems so widely different?

It is involved in most of the arguments for the identity of the authors that the lyric poetry grew out of the epic; that the strophe was composed for epos and afterwards devoted to lyric purposes, and what seems to have been preserved from the early records of various countries favors this supposition. That it really was so in the Middle German epoch is open to doubt.* That in every country the natural songs of love are as early as the national songs of heroes, human nature would seem to make possible. That von Kürenberg was the earliest known lyric poet in the twelfth century (if indeed he can be said to be known) does not preclude the possibility of songs of love long previous to him. Elements of a lyric nature are found in nearly every great epic, and the internal evidence is against the invention of this strophe primarily for epic pur-

* Scherer believes that it could not have been so.

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poses. Any one who has read Gervinus' remarks on the strophe of the Nibelungenlied will perceive what is here meant. Any one who has read the poem in the original must be captivated by a theory to regard the strophe as created for epic purposes. Lyric it is and always was, and probably was the invention of a von Kürenberg. Borrowed by some nameless poet or poets to embody the majestic contents of some old Latin poem, or more probably the heroic traditions of the people, it is ill adapted to the mighty burden which it conveys. But so grand was this burden, that the poem impressed itself on all who heard it in spite of its crudity, and became a model for epic poems for generations.

No account has been taken of Vollmöller's claim that wîse means simply melody, and that metrical form was often borrowed. That wîse did once mean only that is certain, but at the end of the twelfth century its meaning probably embraced also the metrical form. But König Ortnit and Alpharts Tod are stubborn facts against Pfeiffer's argument from metrical proprietorship. They alone make the supposition three-fold less supposable. Bartsch's argument from metrical agreements are either theoretical or so involved in the identity of metres as to have no great force. The arguments from coincidences of expression are, in the absence of very definite points in time, not weighty enough to prove anything more than unconscious imitation. An agreement in respect to the time of origin within a quarter of a century may be conceded. An agreement as to geographical origin is probable. But over against the theory of Bartsch and Pfeiffer, an ingenious hypothesis sustained by a combination of hypotheses, some ingenious but some opposed by facts, may be placed as an explanation of agreements between the so-called Kürenberger strophes and the Nibelungenlied another hypothesis, which many weighty facts in the history of German literature and in the history of poetry and the analysis of these two poems favor, and to which the arguments of scholars of authority seem to point, viz.: Der von Kürenberg invented the "Kürenberges wîse," or the Nibelungen strophe-form. This metre was borrowed in at least four well-established

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instances, and probably indefinitely, as it was invented before the ambition for or attainment of literary eminence insisted on a copyright. The four certain instances before a score of years of the thirteenth century had passed are the second form of strophes in the Minnesangs Frühling under the title von Kürenberg, König Ortnit, Alpharts Tod, and the Nibelungen. The beauty of the so-called Kürenberger strophes may have influenced the composer of the Nibelungen in his choice of metre, and led him unconsciously to adopt forms of expression identical with some elements in the few strophes that easily remained in his memory and inspired his imagination. But there is not a vestige of *proof* that Der von Kürenberg wrote either one of the lyric strophes in the Paris manuscript under his name or the Nibelungen, and had it not been for the German scholar's unwillingness to leave so grand an epic nameless, we may believe that this theory would never have been promulgated.

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## XII.—*On Dissimilated Gemination.*

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Students of etymology work on the theory that every letter in a word is a bearer of meaning. The roots, indeed, in the Indo-European tongues are generally accepted provisionally as in some sort integers, and their ultimate elements are left untormented; but every letter which is added to them, and every change which is made in them, is treated as a modifier of the sense. An addition is often a plain compounding with another familiar word, as in the Gothic *libái-dêdum*, lived-did; and when it is not so, the added letter is taken for a relic of such a word, as the d of lived is a relic of did. Scientific etymologists accept the derivation of no word as complete, until the original words from which each letter

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sprang have been fully made out. This procedure has given rise to some of the most important discoveries of the modern Science of Language. A surprising number of obscure combinations have been traced back and explained, and the investigations have led to new comprehension of the laws of language and of thought, and especially of the formation and meaning of declensions and conjugations and all the apparatus of grammar. Even the most obscure vowel changes, which had been supposed to found in rhythmical laws like the Chinese significant tones, are found in Indo-European to spring from contractions of early compounds, or to be accentual effects resulting from added syllables. The euphonic additions of the old grammars have nearly all disappeared. Third rate investigators, to be sure, who must always have some indefinite power to explain the unknown, have come to use pronominal roots pretty freely as a refuge for ignorance, but if the scientific etymologist looks twice at a "pronominal root," he looks three and four times at a "euphonic addition."

This general theory is, however, to be applied with some modifications, and is subject to some exceptions.

1. After a form has become establisht it will be used in new words by conformation without any precedent composition. We write telegraphed in conformation with lived, loved, washed, and the like, without making a contraction of telegraph-did. (2). A like conformation sometimes takes place where there is no sense in it, simply by the assimilating force of some striking and frequent combination of sounds, as in *escarboucle*, *carbunculus*; *escrevisse*, German *krebiz*, *krebs*, which in Old French assume an *es* through the assimilative power of words beginning with *esc-*. (3). Where there is varying pronunciation, e. g., some speakers dropping their h's, others sounding them, a letter may be attacht to a word by blunder, as in hermit for eremite: it has lately been attempted to exalt this fact into an important law of speech, explaining "Grimm's Law," and many other hard problems. (4). Somewhat similar is the use of new letters when a race takes up a word from another race having different habits of speech, as in the Norman *gu* for the German *w*: *guile* for

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wile; *guerre* for war, Old English werre; *Guillaume* for *Wilhelm*, and the like. (5). The bringing together by syncope or ecthlipsis, or by composition, of letters hard to pronounce together.

But these are all rare except the first, which is rather an interpretation of the general rule than an exception to it.

A more important exception is found in the doubling of letters. Sound gravitates to accented syllables. According to the received opinion, the accentual systems of most modern languages have changed. The grammatical system, in which that syllable is accented which last modifies the general notion, so that the affixes of declension oftenest receive the accent, gave way in Greek and Latin to the rhythmical system, according to which the number and quantity of the syllables of a word determine its accent, and gave way in the Germanic languages to the logical system, in which the accented syllable is the first of those expressing the main notion. It is also believed that stress has come into use as the essential quality of accent in the place of pitch, which had been most noticed by the Indians, Greeks, and Romans. These changes in the place and the nature of accent have produced considerable changes in the letters of many words. Unaccented syllables tend constantly to lighten and disappear, accented syllables tend to lengthen. And these processes are often so related that one compensates for the other, and the time of the whole word is unaltered, as though time were more constant than any quality of sound. The time added to the accented syllable is sometimes given to the vowel and sometimes to the consonant. The simple prolongation of a vowel sound does not usually attract much attention, though it is often denoted in the Germanic languages by writing the letter twice. When a continuous consonant is prolonged or a mute is held, we hear the closing of the organs in connection with the preceding vowel, and the opening of them in connection with the following vowel, and we count the closing and the opening as separate sounds, and represent them by writing the consonant twice; the first *p* in happy represents the closing of the lips in hap-, the second *p* represents the opening of the

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lips in -py. The secund *p* is said to be caused by gemination —a sort of fissiparous generation. In English it is so frequent a fact that an accented root-syllable doubles its final consonant when a syllable of inflection or derivation is added, that it is sometimes given as a rule. There are also many other words in which gemination of a consonant has taken place in connection with the shortening of the preceding vowel; mummy, Old French *mumie;* manner, French *maniere;* Late Latin *maneria;* dinner, French *dîner;* gallop, French *galoper;* quarrel, Old French *querele*, Latin *querela;* matter, Old French *matere*, Latin *materia*, and many more which may be found collected in Maetzner, I., 180–182.

But it often occurs that the following letter, or some other cause, modifies the sound produced by the parting of the organs so as to render it unlike that produced by their closing, and in place of a simple doubling a strange letter then appears. This may be called dissimilated gemination.

If the term be applied with a certain freedom to all cases where an emerging letter is added to another letter by a slight modification of the closing or opening movement of the older letter, it affords a convenient classification for a considerable part of the examples of epithesis and epenthesis heretofore unexplained.

The continuous consonants give the most striking examples, and among these the nasals.

The labial nasal *m* is often doubled; but the same movement of the organs which makes *m* with the nose open, will make *b* if it be closed; hence we find *b* appearing in place of a secund *m*. The most common case is before *r*, or *l*. These were originally trills, *r* of the tip, *l* of the edges of the tongue, and they required a strong current of breath in the front part of the mouth. To give this we stop the openings at the back of the mouth, closing the nasal veil. But thought runs ahead of the movements of the organs in speech, anticipates the coming sounds, and often prepares for them before their time. Anglo-Saxon slumerian (Old Norse *sluma*) has in German simple gemination and appears as *schlummern*, in English the lips close in slum-, but the anticipation of the coming *r*

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leads to stopping the nose as they part, and what would have been -mer turns out -ber; and so we have slumber by dissimilated gemination. Similar are timber, Gothic *timrian*, German *zimmern;* embers, Anglo-Saxon æmyrie; number, Old French *nombre*, Latin *numerus;* remember, Old French *re-membrer;* Latin *memorare;* and with *l*, grumble, Low Dutch *grommelen;* nimble, Anglo-Saxon nêmol, Old English nimmil; fumble, Low Dutch *fummelen;* mumble; crumble; tumble; stumble; humble, Old French *humele*, Latin *humilis;* encumber, French *encombrer*, L. Latin *combrus*, Latin *cumulus*.

This dissimilation used to be common at the end of words. The continuous consonants are generally held or prolonged when final. Final *s* and *l* are usually written double, *m* and *n* used often to be; mann is almost as frequent in Anglo-Saxon as man. The parting of the organs is not now as striking in these sounds as it is in final mutes. It is natural that the prolongation should attract the chief attention, but it seems to have been the habit in early times to open the organs from *m* or *n* more audibly than now. And the geminated *m* or *n* is often dissimilated, mb appearing for mm, nd for nn. Examples of mb are limb, Anglo-Saxon lim; thumb, Anglo-Saxon þuma; crumb, Anglo-Saxon cruma; numb, older num. Why *m* should here change to *b* is a problem of some difficulty. It seems to be contrary to the law of least effort; limb is harder to utter than limm, it requires an additional movement of the nasal veil. The *b* soon became silent. If mb final stood alone, it might perhaps be explained away as bad spelling, or conformation of some other kind, but other continuous consonants are affected in a similar manner; changes of nn final to nt, ss to st are frequent; the vocal gesture ss-s is often closed with *t*. In all these cases the law of least effort seems to be contravened.

This law has not yet been as distinctly formulated as it needs to be for scientific application, and is apparently differently interpreted even by cautious philologists. Some use it as meaning the mechanical force necessary to produce



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the movements of the vocal organs. The closing organs are moved further in making a complete closure for a mute than in making an incomplete one for a continuous consonant. The law of least effort requires therefore that mutes must change to continuous and not the reverse. The tip of the tongue is more easily moved than the back; therefore gutturals must change to palatals and linguals. When a consonant comes between two vowels, it requires, to make a surd, a movement of the vocal chords which is not required to make a sonant; therefore surds between two vowels tend to change to sonants, all as a sort of mechanical result. Thus defined the law of least effort expresses the action of a very large number of important factors of speech, and is capable of being brought to certainty and applied to the solution of the unknown as a working law ought to be. But it has by no means the universality which is generally attributed to the law of least effort. Mechanical or physiological forces operate on speech mainly through their action on the minds of speakers. The proximate laws of speech are laws of human action, and for laws of force to be also laws of speech, force must be translatable into what Jonathan Edwards calls motive. The will is always as the strongest motive. For the law of least effort to be good in speech, force must vary inversely as motive. As soon as this reference to motive is made, it is obvious that a thousand motives may be found which will overmatch, any one of them, the set towards a weak use of the voice in the mind of any individual man at any particular time. Children will shout with all their might by the hour for the fun of it. In fact, speaking at all implies a stronger motive than that to least effort, and any kind of motive which prompts speech may prompt to more effort than is necessary to speak. The exceptions to the law thus defined are numerous, and its firm establishment in the classic languages is due to the fact that the number of persons is so great who must concur to establish a change in such speeches, that individual peculiarities are eliminated. A considerable number of such peculiarities may be looked for in dialects, and some of them attain

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position more or less approved and permanent in classic monüments. There are further changes connected with change of stress in accent, in which whole nations move together from weaker to stronger utterances.

Others speak of the law of least effort as though it were convertible with the law of strongest motive. They take it to mean that we always speak as it is easiest on the whole for us with our character and wishes and conditions. Such a law would have its place among rhetorical topics for illustrative lectures, rather than among working formulae for investigation and discovery.

Whichever view we take of the law, it is equally in place to use motives to explain unusual changes which have occurred in speech. Thus the closing of a final continuous consonant to a mute, changing mm to mb, ss to st, nn to nd or nt, may be ascribed to a desire to mark the end of the word, to make a positive stop instead of letting the sound die away, or other like motive. This motive was strong enough to induce the utterance of mb as long as final m was habitually uttered with long-drawn vigorous nasal resonance, but has proved too weak to preserve it after our slighter modern m. We have changed from Anglo-Saxon þum-a to thumm, then to thumb, then to thum.

When a surd, for example, t or s, follows a dissimilating sonant, it assimilates the sonant to itself. Anglo-Saxon emtig would have gemination to emmtig, and this, simply dissimilated from nasal to mute, would give embtig; but the conception of the coming t leads to the parting of the vocal chords and the sending up of surd breath too soon, before the parting of the lips for m, and this makes p, not b, and we have empty; so Northampton, Anglo-Saxon Norð-hâm-tûn; tempt, Latin *tentare*. S has the same effect in sempster, sempstress, Anglo-Saxon seâmestre; Thompson for Thomson; Sampson for Samson; and see Maetzner I., p. 175, Koch, I., p. 161. Many more words of this kind are found in Early English than in our present speech. Several examples still current in colloquial speech have disappeared from books. On the other hand the spelling

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is sometimes retained where the pronunciation has become disused or unusual. In the dictionaries empty, tempt, sempster, are all given as having p silent, and some of the speculators say that p can not be pronounced between m and t or m and s. Universal negatives about facts are always suspicious, and none more deservedly so than those which deny the possibility of making unusual combinations of familiar letters. It often happens that phonetic theorists who know only their own language, or perhaps two or three kindred languages, affirm combinations to be unpronounceable, which are among the most frequent in other languages. Sounds which one tried all last week and could never make, may be caught to-morrow and come easy ever after. The organs of speech will do almost any thing in their kind, if they are tried and trained long enough. It is perfectly easy to make the p of empty or tempt or Sampson, and it is in fact made by a large part, if not the larger part, of careful speakers. There is no question of the power of saying hemp or lamp, and the addition of a t or s has nothing specially difficult. That temt cannot be uttered without putting in p might be said, since the surd breath required for t changes m to p, and it is often laid down as an axiom that a syllable consists of a single impulse of voice, and it might be thought that the same impulse could not give both surd and sonant breath. This however contradicts the plainest facts. In pronouncing temt surd breath is used for t, sonant for e and m, and then surd again for the end of the word. The vocal chords are placed in the stream of breath or removed from it at pleasure, and the adjustments of the chords and the nasal veil for the final surd sound may be made at will before or after the parting of the lips from m, in the first case making mpt, in the other mt.

Before n some examples are found in Old English: solempne, solemn; dampnation. These are to be explained like final mb, as additions of impulse; they fill a hiatus. They are different therefore from the regular change of Latin mn to mbr in Spanish, sometimes found also in Old French and other Romanic languages: Spanish *hombre*

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from Latin *hominem; nombre* from *nomen*, and the like (Diez, I., 201). In these n changes first to r, a common weakening (Diez I., 203), as in French *ordre*, order, from Latin *ordinem;* French *coffre*, coffer, Latin *cophinus;* the r then brings in the b by regular dissimilation as before explained.

Quite similar are the changes of the lingual nasal n.

1. Before r or l, when there would be gemination of n, the closing of the nasal veil changes the latter n into d; thunder, Anglo-Saxon þunor; kindred, Old English cunrede, from Anglo-Saxon cynn; gender, French *genre*, Latin *generis;* spindle, Anglo-Saxon spinl, spindl, Old High German *spinnala;* spider, Old English spinnere.

2. Final n (nn) dissimilates to nd; sound, Old English soun, sôn, Latin *son-us;* lend, Old English lênen, Anglo-Saxon læn-an; ribband, French *ruban.*

3. Surd dissimilation, nn to nt, is found final in words from the French: tyrant, Old French *tirant*, *tirant*, Latin *tyranus;* parchment, Old French *parcamin*, *parchemin*, Latin *pergamenum;* ançient, Old French *ancien*, Latin *antianus;* cormofant, French *cormoran;* pheasant, Old French *phaisan.* And see further Maetzner, I., pages 186, 187, Koch, I., § 166, 168.

The guttural nasal ng, as in sing, long, cannot be so easily traced as the other nasals, on account of the defective notation for it of the Roman and Greek alphabets, neither of them having a letter for it. In English, however, in the positions where the other continuous consonants double in the middle of words, we find that a dissimilated gemination of ng takes place in pronunciation, though not in spelling.

1. Before r or l, when there would be gemination of ng, the closing of the nasal veil changes the latter ng into the mute g: longer, from long, parts its nasal ng into ng + g, long-ger, in which the latter g has the same relation to ng that b has to m in number, and d to n in thunder; so stronger, i. e. strong-ger; younger, i. e., young-ger; ang-ger; fing-ger; ling-ger; hung-ger; and cong-ger, mong-ger, which are not quite plain. Similar

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is the formation of tangle, i. e., tang-gl, from tang; spangle, i. e. spang-gl, from spang; and a number of somewhat similar words from Latin will be referred to further on.

2. Children frequently geminate with dissimilated gemination final ng: longg, strongg. Final nk is regularly pronounced ngk = ŋk: rank, i. e. rangk, raŋk, French *rang*, German *rang*, from the same root as ring, seems to give an example of surd dissimilation analogous to tyrant, ancient, etc.; so clanŋk, from clang, and clinŋk, German *klingen*, *klang*, Latin *clangere*, Greek κέκλαγγα.

Before surd th many pronounce ng as ngk, strength as strengkth, length as lengkth, but that does not seem to be a recognized pronunciation of the dictionaries. Final dissimilation to ngk, thing to thingk, nothing to nothingk, is a well known vulgar or dialectic pronunciation.

Very similar to this gemination of the nasals is that of l before r into ldr. Anglo-Saxon alr, German *eller*, would geminate to al-ler, but really changes to al-der. So in Shakespeare's alderliefest, 2 Henry VI, I., 1, in which alder is from the Anglo-Saxon eal-ra, of all. So also in the Spanish and French. Latin *pulver-is* appearing regularly in French as *polv're*, then *pol're*, would geminate to *pollre*, but is found as *poldre*, or, by another common change, *poudre*, English, powder. See more in Diez, Gram. Rom. I., 104. This change is also agenst the law of least effort; the tongue, which leaves loose edges in uttering l, is raised to a tight stop for d. The motive seems to be the same as that which leads to the change of mmr to mbr, the getting of breath for the difficult trill of the coming r.

Anglo-Saxon baldsam, Latin *balsamum* is exactly parallel with Sampson, Thompson.

The same motive which leads to saying drownd, gownd makes it natural to say mould for French *mouler*, though it is likely that the original *d* of the Latin *modulus*, from which the Spanish and Portuguese make *molde* by metathesis, may have had its influence. So in analogy with tyrant, parch-

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ment, might be explained salt, Latin *sal*, but such t's are held to be affixes. Curtius, Gr. Et., 482.

Following the analogy of l we should expect to find its twin brother r geminating sometimes to rd, rt: French *épinard* from Old (Provençe) *espinar*, Portüguese *espinafre*, Latin (*spina-fer*) *spinifer*, spinage; gizzard, French *gésier*, Latin *gigerium;* laniard, French *lanière*, Latin *lanarius;* Spaniard, German *Spanier*, Belgic *Spanjaard;* and dialectic millart for miller; mizzert for mizzer, etc. Maetzner, I., 440. The same movement which gives rd will give rn if the nose be open: bittern, Old English bitore, French *butor;* martern, French *martre*, marten.

The gemination of s final has already been referd to; the sound of this letter is a natüral vocal gestüre easy to prolong, and the closing it when emphatic by shutting off, rather than gradüally withholding voiçe, and so making st rather than ss, is made easy to every one who speaks English by the frequençy with which st is uttered; it is the ending, for example, of the süperlative degree and of the second person singülar of the verb. In Gothic, when any two dentals combine, the result is st, and the tendençy there most fully exhibited shows itself in the other German tongues. The plainest examples of this kind of gemination are given by particles from old genitives. Anglo-Saxon mid, genitive middes, gives rise to amidst; Anglo-Saxon to-gegnes, agenst; Anglo-Saxon on-mong, Old English amonges, to amongst; Anglo-Saxon hwile, Old English whiles, to whilst: so also alongst, anenst, onst, dialectic wunst, once, betwixt; behest is from behaes, but the -t may be partiçipial as in bequest from becwethan, bequeath. The second person singülar of the verb ends in s from the second pronoun, *twa*, *tu*, *su*, in Gothic, Old Saxon, Old High German as well as in Latin, Greek, Sanscrit, and kindred tongues. The st which appears in Anglo-Saxon and Frisic, and has spread in the German family, has often been explained as an imitation of two or three preteritive verbs, in which this person happens to end in st; Anglo-Saxon wâst (knowest), Old Saxon *wêst*, Gothic *vaist* is from the root *vid*,

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see; the d of the stem combines with the ending to make st, according to the Gothic law mentioned above. And there are a few like words. It may be that they started the ending, but it is plain that they would have no power according to the common operation of conformation, they could only be finger marks to direct the flood into a natural channel; with Anglo-Saxon *glitian*, *glitnian*, *glisian*, *glisnian* and later *gliteren* appear glisten, glistnen, glistren, which may be explained as dissimilated from glisian and the others; so also tapestry from French *tapisserie*, which has a stem *t* hanging about it, Latin *tapete*, Old English tapet.

In German a similar appearance of *t* is found in *obst*, *axt*, *morast*, *palast*, *pabst*, in the verb, and in the dialects in many words.

The examples thus far given have shown continuous consonants dissimilated into mutes. The dissimilation of mutes oftenest produces fricatives. The first examples to which we direct attention are the German changes of *p* to *pf*, *kupfer*, copper, is from Late Latin *cuprum*. A strongly accented *p*, represented by *ph*, first appears in Old High German, which is followed in Middle High German by *pf*. The lips open from the closure of *p* so slowly that they remain in the f-position long enough to make the sound of that letter audible. This is a prevailing habit of articulation among the Germans; they utter *pf* for the old *p* regularly in certain positions, so that the two letters together are spoken of in etymological works as the aspirate which according to Grimm's law takes the place of the *p* of Low German languages. Whether the change is a weakening or strengthening may be a matter of doubt. Regarding it as a dissimilated gemination however, it is safe to say that the gemination of *p* to *pp* is a strengthening, while the dissimilation of *pp* to *pf* is a weakening; the first implies more breath, the last suggests a slower and feebler movement of the opening lips. The fact that the change is most thoroughly carried out at the beginning of words looks like strength. On the other hand it is a step towards a plain weakening, for the *pf* changes to *f*.

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Completely analogous is the change of *t* to *ts*. Latin *puteus*, pit, becomes in Old High German *phuzi*, in Middle High German *pfutze*. The opening of the tongue for *t* is so slow that it remains in the s-position long enough to give that sound distinctly. This *ts* is represented by a single character in German, and is one of its most striking peculiarities.

An analogous change is found of *k* to *kh*.

The transition is easy from these to the dissimilated gemination of a sonant to a sonant followed by a surd, e. g. of d into dt, z into sz, and the like. This is pronounced by the minute observers to be a frequent fact, though seldom or never recorded. To make a perfect sonant there should be sonant murmur before the closing and after the parting of the organs. But it is said that the German habit is to part the vocal chords before parting the organs of the mouth, so that their final sonant consonants when prolonged end in surds; *und* is sounded as *undt*, *ab* as *abp*, *klug* as *klugk*, and the like. And our great authority in such matters, Mr. A. J. Ellis, tells us that final v, z, th, zh before a pause are pronounced in England vf (fi vf), zs (izs), th th (smoodth th), zh sh (roozh sh = *rouge*). "The prolongation of the buzz," he says, "is apparently disagreeable to our organs, and hence we drop the voice before separating them, thus merging the buzz into a hiss unless a vowel follows, on to which the voice can be continued, or a consonant, which naturally shortens the preceding one." Early Eng. Pronun., Part IV., p. 1104.

Initial surds on the contrary tend to geminate into surds plus sonants; the tardy opening of the organs gives time for the following vowel to impart its sonancy; German initial *s*, says Mr. Ellis, is sounded as *sz*.

A few examples are found of the mute linguals t, d, geminating into the lingual trills r, l: tr appears in cartridge from French *cartouche* from Italian *cartoccio* from Latin *charta;* treasure, French *trésor*, It. Sp. *tesero* from Latin *thesaurus*, Greek θησαυρός, may be mentioned: dr appears in French *perdrix* from Latin *perdix*, partridge; tl is in myrtle, French *myrte*, Latin *myrtus*, Greek μύρτος, where the substitution of syllabic l for e may be favored by t, but it occurs



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after other mütes, as in periwiŋkle, Latin *pervinca*, prinçiple, syllable, and the like, where it looks like the simülation of a syllable of formation.

These minüte explanations of words in which the latter half of the original letter undergoes dissimilation will make the process clear by which the former half is changed. The nasals here also afford the best examples: *bb* to *mb*—Latin *labrusca* changes to *lambrusca* in Italian and Spanish, *lambruche* in French; the nasal veil is left open while the lips are closing to *b*; *tt* to *nt*—Latin *palatium* changes to Old High German *phalantsa;* Latin *lutra*, French *loutre* appears in Italian and Portüguese as *lontra;* Latin *pictor* precedes French *peintre*, painter, and *laterna*, French *lanterne*, lantern, but in the two last words the *n* in related words may have had its influençe; *dd* to *nd*—porringer, i. e. porrindsher from porridge; messenger, i. e. messendsher, Old English messager; passenger, French *passagier*; cc = ss to ns—ensample from essample, and see more in Maetzner I., 174; *c* to *ŋc*, *ŋg*—Latin *cucumer* is French *concombre*, *locusta* is *langouste*, *joculator* is *jongleur*. To the frequent appearançe of emerging nasals in the older tongues, attention will be called further on. Of the other consonants it must suffiçe to mention that *r* occasionally appears before its kindred linguals *s* and *th*, as in hoarse from Anglo-Saxon hâs, swarth from Anglo-Saxon swa*đ*; and *s* before as well as after its kindred *t*, e. g. idolaster, for idolater, though such examples savor of simülation of the ending *-ster*, as much as of phonetic dissimilation.

If we turn to the semi-vowels, a number of facts present themselves which may be classified as dissimilated gemination, if we allow the term a liberal application. Such are those where a stem ending *i* or *j* changes in inflection to *ig* = *ij* = *iy*: Anglo-Saxon *lufian*, to love, is found written also *lufigan* and *lufigean*. It is generally thought that the *i* represents the semi-vowel *y*; then the explanation of *lufigan* is that the voiçe is sent forth for *g* = *y* before the organs have reached the consonant closeness, and is heard as the vowel. Similar in appearançe and explanation is the declension of nouns

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with stems ending in *w* or *u*; *ealu*, ale, has for its genitive *ealwes*, *ealuwes*, *ealowes*. The vowel is produced by sending voice through organs almost closed to *w*. These are regular appearances in the inflection of large classes of words in Anglo-Saxon, and are well known in other tongues; Old High German *palu*, bale, makes *palawes;* Sanscrit *sunu*, *sunawes*.

The other syllabic consonants show somewhat similar phenomena; *l*, for example, frequently geminates into a vowel and consonant, its vowel effect being represented usually by u = u: *milc*, milk, often appears as *miluc*, *meoloc;* so *r*: Latin *metrum*, French *metre*, gives rise to meter; *m*: Anglo-Saxon *bôsm* to bosom; n: Anglo-Saxon *glisnian*, glisten; *s*: French *espace*, Latin *spatium*, and so regularly in words beginning with *sc*, *sm*, *sp*, *st*. Diez, I., 224.

That large and very important class of lengthenings, where a vowel under the accent rises to a diphthong, might also be naturally grouped with the foregoing. Such are those known as guna and vriddhi in the Sanscrit grammars, and in Anglo-Saxon and English grammars more often as progression, according to which *i* changes to *ai*, and *u* to *au* regularly in modern English, *wîf* being now pronounced waif and hûs haus. What takes place is that voice intended for *i* or *u* is thrown out as the organs begin to move toward the proper closure, and is heard as a glide from a much opener position up to the close *i* or *u*. A considerable number of mixed vowel sounds, as *o* from *a*, *e* from *a* and *i*, are sometimes of similar origin.

The reverse of this process occurs when the voice is thrown out while the organs are opening, giving a closer letter gliding to a more open one, as in the breakings of the Teutonic languages, in parasitic *w* and *y*; cyar for car, gyarden for garden; hwaet, what, for haet, and the like; so also in the change of *y* to *dy*, whence arise French *j* and English *j* = dzh.

Thus far illustrations have been found mainly in the modern languages of Europe, most of them in English. The history of the words in these languages is best known, and

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therefore they furnish the best material for scientific study. But if the view here presented be correct, the appearances which we have called dissimilated gemination are produced by movements of the organs of speech so natural that we may expect to find them in all languages. In Latin and Greek there appears to have been no movement of the accent during the classic period, such as afterwards produced the abundant new gemination to which attention has been before directed, and doublings with or without dissimilation, which are found in the earliest remains are open to etymologic as well as phonetic explanation.

The etymologic is held the preferable, and as there are pronominal roots always equal to any emergency, all considerable classes of words which present these appearances are duly explained by them. It will answer our present purpose, to suggest a doubt in regard to one class of roots or stems ending in what might be a nasal dissimilation, such as λαμβ-άνω, which might be a geminated λαβ, i. e. λαββ dissimilated to λαμβ; λανθ-άνω, which might be from λαθθ; θιγγ-άνω, i. e. thiŋ-gano, which might be from θιγ; so Latin *lamb-o* from root *lap; find-o* from root *fid; fingo*, i. e. fiŋg-o, from root *fig* = θιγ. The class is numerous, and gives rise to a number of English words: see page 154, *spangle*, *tangle*, and others before referred to. The current explanation of them is that a pronominal *na* or *nu* was originally added in such a way as to be phonetically an insertion of *n*; then by the euphonic laws, *n* before a labial or palatal is changed into the cognate nasal, and all is supposed to be clear. Cl nasal dissimilation would not be less clear.

There are only left in the latest scientific grammars a few words in which "insertion" is admitted. Roby, Latin Grammar, I., 14, gives *sumptus* for *sum-tus; hiemps* for *hiems; ἀνδρός* for *ἀνερός*, *μεσημβρία* from *μέση ἡμέρα; Alcumena* for *Ἀλκμήνη; Tecumessa* for *Τέκμησσα; Æsculapius* for *Ἀσκληπιός*. Corssen, Schleicher, and the others do not essentially enlarge the list. Mr. Ferrar, Comp. Gram., p. 175, gives the *s* in *monstrum* as a similar insertion, but the others have a pronominal stem for that.

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In Mr. Peile's "Introduction to Latin and Greek Etymology," which is one of the latest and best of the works in English on that subject, and uses in a scholarly way the labors of Curtius, Schleicher, and the other great German authorities, there is a considerable chapter (Chap. IX.), devoted to "changes produced by want of clearness in pronunciation." A letter is slurred generally through laziness, he says, and an indefinite amount of indistinct sound is produced after the letter thus slurred, which in time takes the form of the nearest sound in the existing alphabet. Thus two letters grow out of one, and the old saying is justified that lazy people give themselves most trouble. He treats of the emergence of $u = w$ after *k* and *g* as in *equos*, *pinguis;* of $i = y$ after *k*, *g*, and *t*, described above in cyar for car, gyarden for garden, tüne for tune; of *d* before *i* and *y*, as in Jacob i. e. Dzhacob from Iacob; of added h; of added vowels, of which he gives examples from the Greek, initial like *ἀ-στήρ*, star; *ὀ-δόντ-ος*, tooth; medial like *ἤλ-υ θον* from *ελθ;* and the so-called "connecting vowel" which Bopp and Schleicher and the like make a pronominal stem; and finally added consonants, of which we treated first. They are despatched in a single page. He says: "These are not very numerous either in Greek or Latin." "They are among the most decisive signs of a decomposing language, and therefore are rather to be looked for in more modern tongues, as *gen-d-re* and *nom-b-re* in the French." Most of his space is given to English. "Plenty of examples in English may be seen by turning over the pages of any dictionary, e. g. a-d-miral (= emir-al, Milton's amiral), a-d-vance (a-vancer, *ab-ante*), a-d-vantage, al-d-er (Anglo-Saxon alr), etc. An auxiliary consonant is also found at the end of words, as lamb (Anglo-Saxon lam), sound, the vulgar gownd, etc."

The examples looked at in the light of the preceding discussion do not seem happily chosen. The labial *m* of amiral would not dissimilate into the lingual *d*, neither would the *v* of *avancer;* these are examples of etymological simulation; they affect to be compounded with ad-. Lamb has a stem *b*; the Gothic is *lamb*, and the *b* or *p* is in Old

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High German, Old Saxon, Anglo-Saxon, Old Norse, Celtic and elsewhere; Lapp has *libbe*, *libba*, with *lamb*, and Gaelic has *lubhan* with *lumhan*.

Of more interest is it perhaps, to notice that Mr. Peile's theory of laziness and slurring seems to strike him as inapt in one at least of his English examples. He says: "In 'thunder' the *d* is interesting, because it does not occur between two consonants; the very full sound of the first syllable seems to be the cause of the need which is felt of a connecting link between it and the following vowel."

We have seen that in this whole class of appearances it is greater and not less stress which brings out the new letter, and that the change of *n* to *nd* under the accent is no more a weakening than that of *i* to *ai* in wife or *u* to *au* in house.

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In page 13 of Proceedings, first line of 5, read: ἡν πως, for the MS. reading.

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# American Philological Association.

1876-7.

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# AMERICAN PHILOLOGICAL ASSOCIATION.

BALTIMORE, Tuesday, July 10, 1877.

The Ninth Annual Session was called to order at 4 o'clock P. M., at the Johns Hopkins University, by Professor B. L. Gildersleeve, Vice-President.

An address of welcome was made by Mr. John T. Morris, President of the School Board of the city of Baltimore, to which the Vice-President replied.

The Secretary being absent, Professor J. M. Garnett, Principal of St. John's College, Annapolis, Md., was elected Secretary of the meeting.

A report was presented from the Executive Committee, announcing that the persons whose names follow had been elected members of the Association:

Mr. C. C. Bates, Plymouth, Mass.; Professor E. S. Holden, Washington, D. C.; Professor C. K. Nelson, St. John's College, Annapolis, Md.; Professor Austin Stickney, late of Trinity College, Hartford, Conn.; Professor W. B. Webster, Military Institute, Norfolk, Va.; Rev. R. F. Weidner, Phillipsburg, N. J.

The Treasurer presented his report, showing the receipts and expenditures of the past year to be as follow:

| RECEIPTS. | |
|---|---|
| Balance in treasury, July, 1876, | $692.52 |
| Fees of new members and annual assessments, | 400.00 |
| Interest, | 43.75 |
| Sales of publications, | 45.00 |
| | $1,181.27 |

| EXPENDITURES. | |
|---|---|
| Printing Transactions, 1875, | $234.47 |
| Printing Proceedings, 1876, | 168.60 |
| Expenses of New York meeting, | 77.90 |
| Postage, expressage, stationery, etc., | 27.79 |
| Profit and loss (coupon unpaid), | 17.50 |
| | $526.26 |
| Balance in treasury, | 655.01 |
| | $1,181.27 |

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The Association is the owner of one bond of the Connecticut Western Railroad for $500.00, on which there are three unpaid coupons of $17.50 each.

On motion, Dr. J. Hammond Trumbull and Professor C. H. Toy were appointed Auditors of the Treasurer's report.

A paper on "Temporal Coincidence of the Aorist Participle with the Primary Verb," by Mr. Augustus C. Merriam, of Columbia College, New York City, was read by Dr. C. R. Lanman.

It has been pretty generally conceded that the aorist participle with λανθάνω, τυγχάνω, φθάνω, περιοράω, ἐφοράω, and some other verbs, does not represent time past with reference to the primary verb, but coincides with it in time. But beyond its occurrence with the first three verbs just named, this usage does not appear to have been remarked in the Homeric poems. The question as to the admissibility of a wider use of it there presents itself in ϑ 564, where Alcinous, speaking to Ulysses of a prediction of his father's, says:

ἀλλὰ τόδ' ὥς ποτε πατρὸς ἐμὸν εἰπόντος ἄκουσα.

There is nothing in the context to determine whether he means, "I heard my father say," or, "I heard (from another) of my father's having said." The usual construction of the tense requires the latter. Is the former allowable? Upon a consideration of all the instances of ἀκούω with the participle in Homer, it becomes apparent not only that the former is admissible, but that it is the probable rendering.

Take first ι 497, where Ulysses is washed back by the refluent wave produced by the mass of rock hurled before his vessel by the Cyclops, and has pushed off again in silence out of reach. He then, wishing a second time to taunt the monster, is addressed by his comrades, who beg him to restrain himself, declaring that they thought themselves dead men when driven to shore, and they add:

εἰ δὲ φθεγξαμένου τευ ἢ αὐδήσαντος ἄκουσεν,
σύν κεν ἄραξ' ἡμέων κεφαλάς, κ. τ. λ.,

"If he (Polyphemus) had heard any one of us cry out or utter a word, he would have broken our heads."

Secondly, as the supreme crisis of the Odyssey is drawing on, the disguised Ulysses accompanies Philoetius and Eumaeus without the palace and asks them what part they would take in case their old master suddenly returned. They both respond with the fervent prayer that he may come and promise they will show him what might is theirs in his behalf. He thereupon discloses himself to them and says (φ 210):

τῶν δ' ἄλλων οὐ τευ ἄκουσα
εὐξαμένου ἐμὲ αὖτις ὑπότροπον οἴκαδ' ἱκέσθαι,

"Of the others not one have I heard pray that I again homeward might return."

Thirdly, in K 276, as Ulysses and Diomed are starting out upon their nocturnal scouting expedition, Athene sends a heron close by their pathway, and "they saw it not with their eyes through the night's darkness, but they heard it scream":

τοὶ δ' οὐκ ἴδον ὀφθαλμοῖσιν
νύκτα δι' ὀρφναίην, ἀλλὰ κλάγξαντος ἄκουσαν.

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Again, when Ulysses in disguise is struck in the banqueting hall with a footstool by Antinous, the poet says (ρ 492):

τοῦ . . . . . ἤκουσε περίφρων Πηνελόπεια
βλημένου ἐν μεγάρῳ,

"Penelope in the women's apartments heard him when struck."

Here are four crucial instances where the context shows beyond any doubt that the act of hearing is an immediate one, and that the aorist participle stands as regards time for the common prosaic present which meets us so frequently in Xenophon and Plato.

In short, the instances of participial construction in Homer after ἀκούω and the kindred verbs κλύω, ἀΐω, πυνθάνομαι, ξυνίημι, may be grouped under two general heads:

I. Where the object of perception is *immediate*, and falls directly under the sense, i. e., one hears such and such a thing with his own ears.

II. Where the object is only *mediate*, i. e., one hears from another that such and such a thing is so.

Under I., the following passages may be cited where the aorist participle occurs, and these are best divided into three classes; (a) where the primary verb means simply to hear; (b) to hear with implied idea of heeding and granting prayer; (c) to hear with implied idea of heeding and obeying:

I. (a). ϑ 564, ι 497, φ 210, K 276, ρ 492, Υ 380, B 182, K 47, Π 76, δ 505.
(b). A 380, Π 531, A 453, Π 236.
(c). Z 281, γ 337.

The instances of the present participle belonging here are:

(a). O 506, κ 221, A 396, Φ 475, ϑ 95, 534, δ 76, K 189, Λ 463, μ 265.
(b). Π 515, ζ 325, I 509, T 77.
(c). O 199, β 423, τ 419.

II. Aorist Participle:
δ 728, β 375, δ 748, P 377, N 521, P 427, T 322, 337, ρ 115.
Present Participle:
λ 458, α 289, β 220, Ω 490, π 301, H 129, γ 94, δ 324, A 257, δ 732.
Infinitive: Z 386.

It will be remarked that we have under I., sixteen cases of the coincident aorist to fifteen of the present participle, not reckoning phrases repeated. If the six cases of (b) and (c) belonging to both tenses be put aside as admitting the more common explanation (and while this is possible, it is quite unnecessary in the light of the present investigation), the aorist participle has a majority of one over the present, demonstrating that this usage of the aorist was a deeply rooted one in the early stage of the language. And yet, in all the instances under II., where the object is mediate and secondary, the usual distinction of tense is fully maintained.

This coincident use of the aorist after verbs of hearing I do not find in the Homeric hymns, in Hesiod, Pindar, Aeschylus, or Xenophon, although full indexes have been examined. In fact, I have met with only two instances outside of Homer: one in a fragment of the poet Xenophanes (6), and the other in the Argonautica of Apollonius Rhodius (i. 278).

Such circumstantial participles as φωνήσας (Δ 284, α 122, et saepe), φθεξάμενος (K 139, Ω 170), ἀύσας (Δ 508, Z 66, N 413), ὁμοκλήσας (E 439, Z 54), ἐπευξάμενος

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(Z 475), ἀπειλήσας (φ 368, H 225), usually preceding a speech and accompanying a verb of saying, as for example, σφεὰς φωνήσας ἔπεα πτερόεντα προσηύδα (a usage continued through into New Testament Greek, where the phrase ἀποκριθεὶς εἶπεν is very common), have been explained by Classen as inceptive, in the sense of "having lifted up the voice," "having begun to pray," etc. But the question becomes a pertinent one whether this apparent awkwardness of expression may not find its explanation in the same use of the tense which has been proved to exist after verbs of hearing. The common καί μιν φωνήσας ἔπεα πτερόεντα προσηύδα, would readily bear the rendering, "and to him in speech winged words he spake"—the participle referring merely in an indefinite way to the time embraced by the primary verb. The numerous instances of present participles also occurring in these phrases show the same interchange as after the verbs of hearing.

Mr. E. G. Sihler, of Johns Hopkins University, read a paper on "A Comparative Review of the Accounts of the Battle of Salamis given by Herodotus and Aeschylus."

Editors of Aeschylus's *Persae* or of Herodotus have of course been in the very proper habit of illustrating their commentaries from the other author respectively; and one of the latest writers on Greek History, Mr. Cox, seems to consider the battle of Salamis a morsel from which nothing more may be gotten. But within the last few years two valuable contributions by Kirchoff and Wecklein have appeared, on which this paper is based. In the introductory portion three questions were proposed:

(1.) What indications are there found in Herodotus himself about his own sources of information? Herodotus emphatically designates himself as a gatherer and preserver of tradition, mentioning any specific authority but rarely, and then mostly in conflicting traditions.

(2.) What were the intentions of the author? They were fair and his veracity is unquestionable. Of bias and prejudice but little is noticeable. At the same time, the tradition which he gathered could not but be to some extent colored by several factors; such as the tendency of allowing no blemish on the picture of past grand national events, or the sectional and personal dissensions and feuds, or the religious and moral conception of the author himself.

(3.) What did Herodotus know of existing literature? He knew it minutely and critically, and, not least, the dramas of Aeschylus, though mentioning him but once (II. 156).

The more detailed comparison of the two accounts was begun with Persae 337 sqq. and Herodotus VIII. 75.

The results were these: Herodotus takes Aeschylus into most accurate consideration throughout. Where Aeschylus gives only a general and poetical picture, Herodotus adds specifications of names, time, number, and locality. (Aeschylus from his Panhellenic standpoint forbears mentioning any names of individuals or commonwealths on the Greek side.) Again, where Aeschylus has been very explicit and detailed, Herodotus is satisfied with presenting a brief and concise summary. One of the most striking illustrations of this attitude is in the account of the massacre on Psyttaleia.

In treating of the Persian disposition of their fleet as caused by the Themistoclean message, the author believed himself to be able to show full harmony

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between Aeschylus and Herodotus; the τρεῖς στοῖχοι of Aeschylus being one line along the coast *in* the sound extending from Eleusis to the Piraeus, the second across the straits from Salamis to Munychia, and the third closing the western escape between Megaris and Salamis.

Grote's disbelief in the correctness of Herodotus's topographical propositions was exhibited as arbitrary and unwarranted. The plan of the battle which the author presented as his own comes nearest to that of Spruner, and differs most from that of Grote.

A paper by Professor Austin Stickney, late of Trinity College, Hartford, Conn., on "The Single Case-Form in Italian," was read by Mr. C. J. Buckingham.

The question as to which of the old Latin cases was the prototype of the single forms which remain in the declension of the Romanic languages, has been often discussed, but is still unsettled. The various views may be conveniently arranged under three heads. According to the first, the simple ground-form remains, divested of the old case-endings and clothed in such new guise as each particular language has chosen. The second opinion is that we have in the modern nominal forms the remains of some specific case, differing in various countries, the survival being in virtue of something like conscious choice or logical necessity. Those who maintain the third view hold that the modern case-form is the phonetic result of the wearing away of the old endings, in consequence of which the cases came to coincide in a single form.

The facts of the phonetic decay of Latin are generally conceded; its results differ, of course, in various countries. In France, it left a tolerably symmetrical system of declensions, owing to the persistence of the final *s*, and the general loss of the syllables after the tonic. In Italy, the paroxytones were tolerated or even favored, and the old endings remained vigorous much longer and in greater variety.

But phonetic decay, while it explains very well the singular number in Italian, is quite insufficient for the plural. After an examination of the facts as they appear in the language, it would seem that a complete statement of the origin of the nominal form should be made somewhat as follows: The present case-form is the result of the phonetic decay of the old Latin forms; but whenever this alone would have left more than one form for a number, there was a sort of natural selection, resulting in the survival of that one which, from its phonetic character, best suited the habits of the people using it, or which, oftener recurring in ordinary language, impressed itself more distinctly on the mind; in masculines and feminines, this was generally the continuation of one of the oblique cases, chiefly the accusative; in neuters, generally of the common form of the nominative and accusative; and when the present form is a new one, it has been made after certain analogies already existing in the old language.

A recess was then taken till 8 o'clock.

EVENING SESSION.

The Association was called to order by the President, Professor S. S. Haldeman, of the University of Pennsylvania.

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The Secretary reported the election of a new member:

**Professor Jacob Cooper, New Brunswick, N. J.**

Professor W. D. Whitney, of Yale College, New Haven, Conn., read the first paper of the evening, on "Surd and Sonant."

Professor Whitney said that the true nature of the distinction between, for example, a *p* and a *b*, or an *f* and a *v*, had long been one of the disputed points in phonetics; for some time past there has seemed to be a decided tendency to agreement among phonetists in the acceptance of the opinion that the *b* and *v* get their distinctive character from accompanying vibrations of the vocal chords; but, very recently, there are signs of a reversal of this movement, seeming to render a new discussion of the point desirable.

It is generally conceded that the English *v* (along with the other so-called "sonant" consonants) does actually differ from its correspondent "surd," *f*, by an accompanying sonancy or tone. It is also well established that through a great part of Germany, at least, there is no sonancy in either member of any such pair, but, instead of it, a difference in force of utterance, the *v* etc. being weaker. The question at issue concerns the relation and the comparative value of these two kinds of difference.

It may fairly be claimed by those who practise the English mode of distinction that in their utterance the difference of force is of no account whatever. Either *f* or *v*, either *p* or *b*, may be and is pronounced with every possible degree of force, without any detriment to its individual identity. The same is true of them as of all our alphabetic elements. The genesis of an *e* or an *i*-sound is usually and regularly by a phonetic "weakening" of an *a*-sound: but an *i* strongly pronounced does not become an *a*, nor an *a* weakly pronounced an *i*. Just so with an *f* in its relation to a *p*. The *a* or *i* or *f* or *p* or *v* or *b*, once worked out as a constituent of the alphabetic system, may be uttered, unimpaired, with the whole range of force, just as of length or quantity, and just as (in the case of the sonant elements) of pitch.

But then, as alphabetic distinctions are capable of being founded on differences of quantity (as in the case of the long and short vowels), or on differences of pitch (as in the case of the Chinese tones), just so a difference in force of utterance of the same articulated sound may rise to absolute value in an alphabet. A great part of the German people, we are told, have no sonant *b* or *v* (and so on), but instead of them a weaker non-sonant correlative to *p* and *f*. Such a sound is an essentially different thing from the other; the two may be historical equivalents, but they are in no proper sense phonetic equivalents: witness the fact that, to real utterers of a sonant *b*, a German is distinctively one who inextricably mixes and interchanges his *b*'s and *p*'s. If, now, it can be shown that the Indo-European and Sanskrit-Greek-Latin *b* was not a sonant counterpart of *p*, but a weakened counterpart, then this German sound has a right to the name *media*, and to the sign *b*. But if (as is generally and doubtless correctly held) the sonant pronunciation is the older and more original one, and the weakened pronunciation without sonancy a later and locally restricted substitute for it, then the name and sign properly belong only to the English *b*, and not to the German, which is rather to be called a weak *p*.

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Of course, the question whether and why the sonant letter alone is in practice subjected to a weakened utterance is one by itself, quite independent of the other.

The writer examined in detail some of the objections recently brought against these views—especially those presented by Professor Sievers, of Jena, in his valuable manual of Indo-European phonetics—and endeavored to show that they are insufficient or untenable.

Professor J. B. Feuling, of the University of Wisconsin, Madison, Wis., next presented a paper on "The Rime in Latin and Greek Poetry."

This paper was prepared by the author with reference to a resolution of the Executive Committee adopted at the fourth annual meeting of this Association: "*Resolved*, That a section of Linguistic Pedagogics be established in this Association, to which section shall be referred for discussion all papers respecting methods of teaching," etc.

The author stated that he would not have thought of bringing the subject before the Association if it were not for the entire neglect into which the rime seems to have fallen in the reading of Greek and Latin poetry. The view of Mr. George P. Marsh expressed in his *Lectures on the English Language* that it was a rule of both Greek and Latin composition to avoid all coincidences of sound, has caused undoubtedly this apparent neglect in the present generation.

The rime is found in its three-fold form, in the beginning, in the middle, and at the end of words; or, as the Germans call it, *Anreim*, *Inreim*, and *Endreim*. In Plautus we find the *Anreim* (alliteration) so frequently as to conclude that all verses which the poet wished to emphasize were adorned with alliterations. There seems to be no doubt that in early Roman poetry, which relied more on accent than on quantity, alliteration had a similar importance to that which it had in old Germanic poetry.

Through the influence of Greek models quantity gradually prevailed over accent, and with it alliteration lost its ground. In the later classical period we find the *Anreim* not so frequently as the *Inreim* and *Endreim*, but the latter frequently enough to lead us to suppose it was employed intentionally, although poetry had lost its popular character.

In Greek the vowel system is less corrupted than in Latin; this fact was the cause of a greater variety of sounds which facilitated assonances and rimes so much that the poet felt obliged to avoid full *Endreims* lest his verse should become a jingle. Marsh's explanation of the difference between Homer and Hesiod in regard to the occurrence of rimes—"that the more numerous critical revisions which the works of Homer passed through eliminated some instances of what to the Greek ear was offensive"—is an assumption which overlooks the fact that *Anreim* and *Inreim* occur more frequently in Homer than in Hesiod. In the fragments of the lyric poets, in Pindar, and in the dramatists, the *Anreim* and *Inreim*, without counting the *Endreim*, occur so frequently in significant passages as to convince us that the rime was employed as a foil to the meaning.

Professor Fisk P. Brewer, of Iowa College, Grinnell, Iowa, read the last paper of the evening, on "The Modern Greek Language in Cyprus."



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The speaker referred to the several collections recently made by Greek scholars of the folk lore of different localities, as Crete, Trebizonde, and Cyprus. In the latter island Professor A. A. Sakellarios, now of Athens, resided and taught for several years, and took down from the mouths of the people their proverbs and conundrums, their nursery songs and lays and tales in prose, the whole constituting one hundred and seventy-three pages of vernacular Greek. The longer poems, some of them five hundred lines in length, occupy the first hundred pages of this book. They seem to have been composed at different periods, one of the earlier lays describing the capture of Cyprus by the Turks in 1571. Specimens of the minor verses were given, together with an outline of two of the prose tales. One of the latter had a resemblance to the old tale of the Attic tragedy respecting Oedipus and Jocasta.

The value of the collection is in the side-light it throws on the development of the language of ancient and modern Greece. This was illustrated especially by a discussion of the origin of the modern form of the verb 'to be' in the third person. An earlier form, from which it was probably derived, is very common in Cyprus. Examples were given of ancient words retained among the common people, and of new words not used elsewhere in Greece.

The scholars of modern Greece were greatly commended for their diligence in collecting the facts respecting the present state of their language.

The Association thereupon stood adjourned to 9 o'clock Wednesday morning.

### Wednesday, July 11—Morning Session.

The Association resumed its session, the President in the chair.

The Secretary reported the election of new members:

Rev. Cecil F. P. Bancroft, Phillips Academy, Andover, Mass.; Mr. Porter C. Bliss, Editor of *Library Table*, New York City; Rev. Richard E. Call, Mohawk, N. Y.; Professor William R. Harper, Denison University, Granville, O.; Professor John B. Kieffer, Mercersburg College, Mercersburg, Penn.; Dr. Charles R. Lanman, Johns Hopkins University, Baltimore, Md.; Mr. R. F. Leighton, now in Leipzig, Germany; Mr. William A. Reynolds, Wilmington, Del.; Mr. W. E. Thompson, Genesee Wesleyan Seminary, Lima, N. Y.; Professor A. B. Stark, Logan Female Institute, Russellville, Ky.

On recommendation of the Executive Committee, it was

*Resolved*, That the length of papers read before this Association be limited to thirty minutes, unless the time is extended by unanimous consent.

On motion, it was

*Resolved*, That this Association accepts, with thanks, the invitation to attend this evening a reception tendered to it by the Athenaeum Club.

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On motion, it was

*Resolved,* That a committee of five be appointed by the Chair to recommend a suitable time and place for the next meeting.

The President appointed as such committee, Dr. J. H. Trumbull, Professor C. H. Toy, Professor W. A. Stevens, Dr. George R. Entler, and Mr. T. C. Murray.

On motion, it was

*Resolved,* That a committee of five be appointed by the Chair to nominate officers for the next year.

The President appointed as such committee, Professor W. W. Goodwin, President W. C. Cattell, Professor J. B. Feuling, Professor T. D. Seymour, and Professor W. B. Carr.

Professor Franklin Carter, of Yale College, read the first paper of the morning, on "The Views of Wilmanns respecting the Authorship of the Nibelungenlied."

Dr. Wilmanns (the successor of Simrock at Bonn) is a moderate Lachmannite, and has published a pamphlet—"*Beiträge zur Erklärung und Geschichte des Nibelungenlieds*"—claiming to establish the composite origin of the great Middle-German classic from internal evidence. His previous success with the Gudrun in proving what the Germans call a "contamination" or combination of two or more separate versions in that poem, was so marked as to leave in the mind of any candid reader a firm belief in his originality and insight. The present brochure will not lessen the respect which the former inspired. Starting in the latter theme of the poem from two propositions which are probable (but which one may hesitate to admit as certain), he goes by sure and regular steps to the detection and exhibition of, first, an old original Rudegersdichtung, a composition in which Rudeger and Kriemhild are the main characters and Dieterich had as yet no share in the action. This was the old basis of the latter third of our poem. He claims, second, that a poem or a part of a poem in which Dankwart was the main hero, was incorporated with this Rudegersdichtung, and brought about certain changes in it and rendered connecting interpolated verses necessary. In other words a Dankwartsdichtung was interpolated in the Rudegersdichtung. He claims to establish, third, that another poem in which Iring was the main hero was also incorporated in the Rudegersdichtung, but entirely independent of the insertion of the Dankwartsdichtung. Thus we should have R. and Da. for one form and R. and I. for another. But it is also plain to Wilmanns that the Dankwart poet takes cognizance of the modifications of the Rudegersdichtung by the poet who introduced Dieterich, so that the one form would be R. + Di. + Da., and the other remaining as before R. + I., we have for our Nibelungenlied, at least for our theme of it, the contamination of R. + Di. + Da. with R. + I.

Various steps in the analysis are open to criticism, but the main lines of proof (the premises being granted) are not easy to confute. Strong objections exist to some of Zarncke's positions taken in the *Literarisches Centralblatt* against Wil-

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manns. His reference of the inconsistencies of the poem to the peculiar style of the poet assumes the unitary individuality of the authorship and thus begs the whole question. The distinction between Lachmann's lay theory and Wilmanns's combination is ignored by Zarncke. The question has been shifted by Wilmanns and the firing of the old guns is of little account. The artificial reasons in support of the Kürenberg hypothesis are seen to be unsubstantial in the light of a searching analysis. In a decision between those reasons and Wilmanns's steps the candid judge will accept the latter. But by reason of a want of absolute certainty in the premises, the exact lines of the versions cannot be insisted upon, and on this account it may be doubted whether Wilmanns's little book will command the assent that it really deserves.

A paper by Professor F. D. Allen, of Mt. Auburn, Cincinnati, O., on "A certain apparently Pleonastic Use of ὡς," was read by Professor T. D. Seymour.

The paper related to the expressions *ὡς ἄλλως*, *ὡς ἑτέρως*, and *ὡς αὐτως* (*ὡσαύτως*). After adverting to the unsatisfactory explanations hitherto current, the writer took the ground that the above phrases are simply the ablatives of *ὁ ἄλλος*, *ὁ ἕτερος* and *ὁ αὐτός* respectively, and mean strictly "in the other manner," "in the same manner." It is well known that the adverbs in *-ως* are in their origin simply the ablative case of their respective adjectives; and it may well be that in these expressions *ὡς* is the ablative of the article *ὁ*, not, as commonly, that of the pronoun *ὅς*. The use of the stem *ὁ-* for this adverb, in preference to the other article-stem *το-*, was illustrated by reference to the adverbs *ὡ-δε* and *οὕτως*. The writer furthermore disapproved the form *αὔτως* still defended by Nauck and others, and contended that *αὔτως* (from *αὐτός*) is the only correct spelling.

Professor W. W. Goodwin, of Harvard College, Cambridge, Mass., next read a paper on "The Text and Interpretation of certain passages of the Agamemnon of Aeschylus."

The author remarked that it was wrong to suppose that no further light could be thrown on Aeschylus from inspection of the manuscripts, and instanced the reading *τὸ μὴ 'ὁέναι* (for *τὸ μὴ εἰδέναι*) in Agam. 1196, found by himself in Codex Venetus 616, where the necessary reading *τὸ μὴ εἰδέναι* has been believed to be merely a conjecture of modern scholars. See Hermann, who says "omnes *τὸ μ' εἰδέναι*." So late editors of the Agamemnon doubt whether *πορθεῖν* or *ποθεῖν* is the reading of Cod. Flor. in vs. 342; but *ποθεῖν*, which is adopted by Hermann chiefly on the authority of this MS., is found in no MS. at all, and the Cod. Flor. (like all the others) has plainly *πορθεῖν*.

The passages of the Agamemnon which were discussed are the following:

1. Vss. 105, 106: *ἔτι γὰρ θεόθεν καταπνείει πευθὼ μολπὰν, ἀλκὰν ξύμφυτος αἰών.* This reading of the MSS. was defended against the common emendations *μολπᾶν* and *ἀλκᾷ*.

2. Vss. 249–254. Here the reading of the MSS. was defended against the modern substitutions, which can hardly be called emendations. The reading advocated was:

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*δίκα δὲ τοῖς μὲν παθοῦσιν*
*μαθεῖν ἐπιρρέπει· τὸ μέλλον δ'*
*ἐπεὶ γένοιτ' ἂν κλύοις· προχαιρέτω·*
*ἴσον δὲ τῷ προστένειν.*
*τορὸν γὰρ ἥξει συνορθὸν αὐταῖς.*

This is the original reading of the Medicean MS., with the addition of *δ'* after *μέλλον*, and the obvious correction *ἐπεὶ γένοιτ'* (as other MSS. have it) for *ἐπι γένοιτ'*. The fac-simile of the Codex Mediceus, published by Merkel at Oxford in 1871, gives *τὸ μέλλον· τὸ δὲ προκλίνειν ἐπι γένοιτ' ἂν κλύοις, κ. τ. λ.* But *τὸ δὲ προκλίνειν with the preceding colon* is inserted by a later hand with different ink. The position of *ἂν* with *κλύοις*, although preceded by the inserted clause *ἐπεὶ γένοιτ'*, is amply defended by such passages as Arist. Pac. 137: *ἀλλ', ὦ μέλ', ἂν μοι σιτίων διπλῶν ἔδει.* No valid objection can be brought against *συνορθόν* (cf. *ἀνορθος, ἐξορθος*), which as an *ἅπαξ εἰρημένον* is not singular in Aeschylus. (See 6, below.) There is great force, finally, in the emphatic *αὐταῖς*, at the end of a passage which is chiefly parenthetic, recurring to the solemn expression (vs. 248) *τέχναι δὲ Κάλχαντος οὐκ ἄκραντοι.* The whole passage may be freely rendered thus (from vs. 247): "But what followed I saw not, and I speak not of it. I do say, however, that the prophetic arts of Calchas cannot fail to bring fulfilment (i. e. the vague horrors of Calchas's prediction in vss. 147–155 must surely be realized). But [it is only by actual experience that we shall ever *know* what is to be the penalty for the sacrifice of Iphigenia, for] Justice brings *knowledge* within the reach of those [only] who have suffered (*πάθει μάθος*); but the future you can hear of when it comes; before that bid it farewell, and this is as well as to lament it beforehand; [and one or the other we must do] for it will come clear and plain, in exact accordance with those (i. e. the forebodings of Calchas)."

3. In vs. 942 *νίκην τήνδε* was understood to mean *that kind of νίκη*, referring scornfully and sarcastically to *τὸ νικᾶσθαι* in vs. 941, as if *νίκη* could be the equivalent of *τὸ νικᾶσθαι* as well as of *τὸ νικᾶν*, as *τιμή* is equivalent to both *τὸ τιμᾶν* and *τὸ τιμᾶσθαι*. This interpretation, which is essentially that adopted by Paley, was defended against that advocated by Kennedy in a recent number of the Philological Journal.

4. In the difficult passage vss. 1025–1029, which can hardly be said to be *interpreted* at all by most of the explanations, notice was called to the strong antithesis of *μοῖρα* and *μοῖραν*, which gives the meaning: "But did not one fate appointed by the Gods hinder another (fate appointed by the Gods) from securing more than its due." This seems to point to a doctrine of "interference" of two lines of fate, by which either may be checked or balanced in a course which would, if unhindered, prove too destructive. This last desperate hope is all that the chorus can now see to warrant them in longer restraining their feelings *ὑπὸ σκότῳ*.

5. In vs. 1347, *ἀλλὰ κοινωσώμεθ' ἂν πως ἀσφαλῆ βουλεύματα*, the MS. reading *ἂν πως* (for *ἐάν πως*, sc. *ᾖ*) was defended, the meaning being "But let us take counsel together, in case there may be any safe plans (for us to adopt)," or "But, if any safe counsels shall occur to us, let us impart them to one another." The latter gives essentially the same idea, without emendation, as Hermann's *ἂν πως*, for *ἃ ἂν πως* (see Paley's note and translation).

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6. In vs. 1599, ᾧμωξεν, ἀμπίπτει δ' ἀπὸ σφαγῆς ἐρῶν (vulg. ἐμῶν), it was contended that the MS. reading ἐρῶν should be retained, the simple ἐράω, in the sense of *vomit*, being amply supported by the compounds ἐξεράω, ἀπεράω, etc., though it does not elsewhere occur.

Professor W. D. Whitney, of Yale College, then read a paper on "The Principle of Economy as a Phonetic Force."

The tendency to economy of effort, or ease of utterance, has long been recognized as the dominant, if not the exclusively governing force in the phonetic changes of language. But, as is apt to be the case with such things, there is, on the one hand, an inclination on the part of some to make an improper use of the law, applying it in an artificial and mechanical way to the explanation of phenomena; and, on the other hand, by a natural reaction, some are beginning to doubt or deny the prevalence of any such law. Professor Whitney's paper was an attempt to correct both these tendencies, by defining more strictly the law in question, by pointing out the classes of facts already fully explained by it, as well as the remnant for which it is not yet made to account, and by showing the consistency of the law with all that we know thus far of the nature and history of language, and the extreme improbability that any other law of phonetic growth will be established in any degree whatever comparable with it. In the present imperfect state of our comprehension of linguistic facts, it would be premature to claim that everything is to be referred to this law; but there is also no reason for yet declaring any problematical phenomena inconsistent with it and proving it inoperative.

Professor C. K. Nelson, of St. John's College, Annapolis, Md., presented a paper on "Semitic Studies, especially as Illustrated in Hebrew, and their Relation to Modern Philology."

Professor B. L. Gildersleeve, of Johns Hopkins University, next read a paper entitled "Personal Reminiscences of Friedrich Ritschl."

In an association which numbers among its members devoted pupils of Ritschl, some mention should be made of the loss which not only classical philology but ideal philology, I may say, has sustained since we last met. Full of years, full of honors, but full of sorrows and sufferings as well, the master has passed away and left us rich with the record of his long and active and brilliant career, but poor by the loss of an incomparable teacher, of a gracious and loving friend. The philologian Ritschl, the great Latinist, the restorer of Plautus, the leader of Roman epigraphy, the keen critic, the unwearied student, a model to the last in his generous acquisition of new truth, of all these things others better fitted than I am have spoken; and recognizing the impertinence of praising a man who is above my praise, I am content to be silent of matters in which laudation would presuppose a right of judgment. But of Ritschl the teacher, Ritschl the friend, the humblest of his pupils and his friends has the right to speak. Germany has many learned men, and he who seeks learning there will not easily go astray; but after a longer sojourn he will find as he looks back that only

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a few have been teachers to him. Of all mine—and I count among them Böckh with his regal sway over Hellenism, K. F. Hermann with his fervid copiousness of thought and illustration, Schneiderrin with his subtle appreciation of the Greek tongue—Ritschl made the deepest and most abiding impression. I first knew him in the time of his full vigor, at an age when enthusiasm and insight are oftenest found in beautiful balance; and while he never declined, but retained his power to the last, it seems to me that it was a privilege to have seen him when the glory of his achievements was fresh about his head, and when the school which he founded was aglow with its recent consecration. The atmosphere of his *auditorium* was an atmosphere of life. In other lecture-rooms it was possible to doze, and the drowsy stream of learning would trickle through the ear and the arm into the pen down to the paper, and the somnolent student might wake up at the end astonished at the vast amount of erudition which had been thus fixed in black and white. But no such lazy note-taking was possible in Ritschl's lecture-room, and indeed he had a sovereign contempt for the textual repetition of his every word. The traditional deification of the *heft* had no hold on Ritschl's discipline, as the master himself had no reverence for the published notes on the lectures of professors, no matter how worthy the professors may have been. The lecture was to stimulate, to impregnate, not simply to present a congeries of facts; and valuable as was the new material which Ritschl set forth, it was his method of combination and presentation, the scientific spirit, the earnestness and ardor of his delivery that made his lectures so memorable. To follow one of his arguments was to watch the advance of a flying column; it had all the inspiration of a battle, the flush of a triumph. His work was everywhere thorough, everywhere clean in style. *Cum pulvisculo exhaurire* was one of his favorite expressions. Exhaustive thoroughness in research, and sharp, clear, nervous expression of result are rarely paired as they were in him. A healthy model as well as an inspiring. But what shall I say of the divining-rod with which he revealed the springs of life within each pupil? To be with such a man was to become conscious of a higher scientific purpose; to listen to his kind words of encouragement was to rise to a sense of a noble vocation. Ritschl's life has many lessons for us all. He was the model of a classical philologian and the model of a university teacher, but the great lesson of all is the great lesson of the one most perfect life led on earth. He loved his work, he loved those who worked with him, and when I think of the stilted bearing of so many inferior men, the professional stiffness of so many who are nothing if not professors, I contrast with these the simple kindliness of the prince of German philologians, and I thank God that I early learned to love and admire and emulate the spirit of this great scholar. All cannot be Ritschls, but we can all be honest, and we can all strive to cast out the besetting sin of self-contained arrogance, which is too apt to mar even the fairest work of the philologian's life. And let this word of exhortation close this feeble testimony to the departed master, teacher, friend.

The last paper of the morning, by Dr. William J. Hoffman, of Reading, Penn., entitled "Notes on the Migrations of the Dakotas," was read by Dr. J. Hammond Trumbull.

The writer's personal observations amongst various tribes of Indians, from Lake Superior westward, and through Nevada, California, and Arizona south-

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ward, and even into Mexico, illustrated the fact that there was either a difference of origin or an immense gap since the time of their breaking up into bands, tribes, and sub-tribes. He regards the idea that these tribes are from one stock, as not worth the time of a second thought. In seeking answers to the questions, Whence come the Dakotas? and, By what course did they reach the country at present occupied by them? the only safe course is to trace, as far as possible, the historical and traditional progress of the Dakotas, keeping their language as our safest guide, until even that becomes lost in a conglomeration of dialects from which there is no perceptible outlet. Whether the comparisons presented in this paper exhibit coincidences that are results of *intrusion*, or actual affinity, is submitted to the consideration of philologists.

These comparisons are introduced by remarks on the recent history, the traditions, and the geographical position of the Dakotas. Dr. Hoffman regards the Asiatic origin of this family as pretty well established. He alludes to various indications of the Asiatic origin or affinity of tribes on the Upper Pacific coast, and to the similarity, pointed out by Professor Roerig (*Smithsonian Report*, 1871, pp. 434, *et seqq.*), in the grammatical structure of the Dakota and Ural-Altaic languages. The affinity of the Dakota to languages of the west and southward is more particularly considered. In the comparison, preference is given to the so-called *Teton* dialect of the Dakota, which, as Dr. Hoffman believes, retains its original purity and is less corrupted than other Dakota dialects by foreign words. The Teton is spoken by the Onc-pá-pas, "Cut Heads," "Blackfeet," and "Two Kettles"; and these sub-tribes lived in the vicinity of the writer's station, communication with them being frequent. The Yanktonais were also frequent visitors. The difference between the Teton and Yanktonai dialects consists, chiefly, in the use of *d* and *n* by the Yanktonais, in the place of the Teton *l*.

Dr. O. Loew, in his report on the ethnology of Southern California and adjacent regions, has noticed the fact that the words for 'water,' 'hand,' and 'bird,' seem to have the same root in many western languages which otherwise are totally distinct. He observes that the radical *ma* in words for 'hand' can be traced as far south as Brazil, but is not found east of the Rocky Mountains nor with the Esquimaux, and that the syllable *pa* for 'water' is not found east of the Rocky Mountains, though of frequent occurrence in the west and southwest. Reproducing Dr. Loew's tables, Dr. Hoffman suggests that, though the Dakota name for 'hand' is *na-pe* ('right hand' *etape*, 'left hand' *chalka*,) yet, in all proper names, we find the word *ma-chpa*, as *Mach'pa Lúta* = 'Bloody Hand' (or "Red Cloud" as that famous warrior is known among the tribe); the name originating in his habit of besmearing his hands with the blood of his victims. The same syllable survives in other names in which the word 'hand' occurs. We have, also, the radical *m* or *ma*, in Aztec *ma*, *toma*, *maitl*, and Mosquito *mita*. Again, among the Pueblos, we find

Keres, *marquin; nash'-kai-ni;*
Acoma, *ha-match-ti i-ni;*
Kioway, *mor'ta;*

And we can trace the similarity to Dakota *nape*, *etape*, and *chalka*, in Keres *na*SH-K*ai*-ni, Aztec TO*ma*, *mai*TL, Tschuktchi *tatlichka*. In these, there appears, to the writer, to be some similarity, although they may prove to be mere coincidences. The syllable *pa* or *pal* for 'water,' found in many western

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dialects (and in languages of the eastern continent), disappears in Dakota *mini*, but in all other Dakota words having reference to water (as, 'stream' or 'river'), with few exceptions, we find *pa-la:* e. g. the Yellowstone and Heart Rivers are known to the Dakotas, respectively, as *Ech-á ka-ha Wak-pá-la* ('Buck-Elk Creek') and *Chante Wak-pá-la* (or *Shanté Wakápla*). Dr. Hoffman notes, also, the similarity of the Dakota *mini* 'water,' with the Hebrew *ma-ym*, and with Esquimaux names, in several dialects, as well as with the Japanese *mitsu* and German *meer*; which, he thinks, "may result from Asiatic origin of the Esquimaux on one side, and a similar source of the Hebrew on the other." In another table he shows that the syllable *tsi* for 'bird' occurs in many Indian languages, including the Dakota, as well as the Chinese, Japanese, and Esquimaux names: and in several other tables, he exhibits the evidences of relationship in words for 'father,' 'mother,' 'head,' 'stone,' 'earth,' 'tree,' etc., in the Dakota and various languages of the west and southwest—and, occasionally, in Chinese, Japanese, and Hebrew.

In conclusion, he observes, *First:* that, accepting the theory and facts that some of our races are of Asiatic origin, and that a similar one has, with good reasons, been claimed for the Dakotas, we find that their migration was northward from the borders of Mexico, or near it, from the fact that the present Indians have dialects closely allied in many terms, which can scarcely have resulted from *intrusion*. *Secondly:* that, accepting the numerous affinities with dialects of California, which are, in turn, allied to Oriental languages, we may trace the start of their migration from the Pacific coast of the lower portion of California. *Thirdly:* that their migration was eastward from this point, before the Aztec migration southward, or after the Pueblos and Cliff Dwellers departed upon their journey into Mexico—this latter hypothesis being scarcely plausible; and *Fourthly:* if we permit the mythology of the Dakotas to influence our investigations, they at one time lived in a "Land of Flowers and Lakes," to which frequent reference is made in their romances, dreams, and tales of remote times, and in which some of them hope to live hereafter.

A recess was then taken till 3 o'clock.

## Afternoon Session.

The Association met, Professor B. L. Gildersleeve, Vice-President, in the chair.

The Secretary reported the following names of new members:

Major John W. Powell, Washington, D. C.; Mrs. Elizabeth Thompson, New York City.

The President, Professor S. S. Haldeman, delivered his address.

After giving an account of the recent progress of philological studies and enumerating several recent works illustrating them, he proceeded to speak of the reform of English spelling.

An association like ours should pay constant attention to the English tongue, its antecedents, its grammar, and the inherent laws of speech from which its



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pronunciation should be deduced, instead of the vagaries of an uncertain alphabet, which misleads even the most careful orthoepists of the empiric school, who have been accustomed to regard its combinations as of more importance than the speech they are conventionally supposed to represent. Without an alphabet adapted to our speech, our spelling has not been controlled by science, or even by common sense. It is so perverse that its use constitutes a superstition. It may be compared to an incubus which presses words from the natural course of their development, and gives us many spurious forms which are retained by the despotic power which imposed them—a power which allows ignorance to pervert words and spellings at discretion, but forbids intelligence to restore them. The old English spellings, 'sovran' of Milton, and 'forain' of Chaucer, have given way to forms in which *g* has been ignorantly placed, and we are required to write *l* in 'could,' which never had an *l* in speech. We also use *l* to a great extent merely as a mark to indicate length in vowels (as in 'chalk,' 'calm'). This perversity in the use of *l* is exceeded in that of *u* in connection with *q*, a letter made for a sound somewhat like *k*, but different. It is heard in the Semitic languages and occurs in many biblic names, such as Ezeqiel, Rebeqâh, Jaqōb. In Latin the use of a single letter (*v*) for the vowel of 'ooze' and the consonant of 'wo' was inconvenient, and the difficulty was partiali obviated where a *cay*-sound preceded, by writing this with *q* as an indication that the succeeding *u* was consonantal, as in the English derivatives 'quart,' 'quiet.' In time, however, the *u* in *qu* was neglected, and this combination of letters became the representativ of *cay* alone, as in Spanish and French, where *qui* spells *kee*. Being without *k*, it should have seemed natural to take *q* instead, but according to the demand of a literary superstition, *q* and *u* must not be separated, as they constitute but one letter.

The *cay*-sound having been properly represented in the Roman alphabet, it was not at any time proper to use *q* or *k* for it; old spellings should have represented old sounds and new letters should have been adapted to new sounds. But when the *cay*-sound was replaced by others, a system of expedients was introduced which has caused the absurd combination *ch* to have a different power in German, French, Italian, and Spanish. The absurdity is fourfold in English which has adopted all of these powers, the German in 'loch,' the French in 'chaise,' the Italian in 'architect,' the Spanish in 'chirp.'

The adoption of the French rule that *c* is a form of *s* before certain vowels, as in the book-word 'cist' beside the speech-word 'chist' and its corruption 'chest' (Latin *cista*, Gr. κίστη), has led to the formation of spurious words, for certainly there is no law of English speech which requires 'sacc-iform' (sack-shaped) to be called *saxiform* (rock-shaped) or 'sceptic' to be spoken of as *septic*; or that should prevent the noun 'talc' from being present in the adjective *talc-y*, no matter under what spellings they appear.

English started as Anglish (Anglosaxon), taking Norman and interfusing with it, each having its own scale of sounds and mode of spelling. As a descendant of Anglosaxon, English should have preserved its alphabet, with such additions as were required by the new Norman sounds. But insted of making a proper adaptation, the Anglosaxon alphabet was mutilated, two consonant letters common to Islandic, representing the lisped sounds of *thin* and *then*, were replaced with *th*, the uniform Roman *cay* was aided with a useless *k* and *q*, as in 'king' for *cing*, and 'queen' for *cven*, with true Latin *e* of *vein*. The letter used for

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the rare Celtic (i. e. Keltic) vowel of 'apple' (Angl. *æpl*) was rejected and the universal *a* (*ah* of 'arm') not only set in its place, but also perverted for the vowel in 'sane,' 'bait,' 'vein,' 'they.' The vowel letter *y*, legitimately representing Greek *v*, Danish *y*, German *ü*, French *u*, was assigned to the initial consonant in 'year' (Angl. *gear*, Ger. *jahr*) where the letter is in fact a form of *g*; in 'my' it is a combination of *ij*; in the name of the Scotch grouse 'capercailzie,' and in the proper names 'Menzies' and 'Dalzell,' it is shaped like a *z*, for which it might be mistaken. Finally, a pseudomorphic *y* sometimes appears in imitations of old English print, where one of the characters for *th* was used, but this supposed old English 'ye' will be read *the*, when the reader is acquainted with the art of spelling.

The Normans brought in new sounds, particularly *dzh* and *tsh*, of which we print the former as in '*g*em,' '*j*u*dg*e,' '*j*u*dg*ment,' and the latter as in 'whi*ch*,' 'hi*tch*,' but we omit the *d* in 'journal' from 'diurnal,' altho we retain the *d* of IUDEX in spelling 'judge.' For etymologic reasons we spell 'gem' with *g* and 'janitor' with *j*; nevertheless, we use both in the cognate words 'genteel' and 'jaunty'—forms of Latin *gentilis*; and we desert the etymologic idea in using *ge* in 'barge' from 'bark.' We spell the older word ('genteel') with the new modern *ee*, and pervert its proper spelling (*gentile*) to a later word with *i* as 'eye,' when in fact, the letters of 'gentile' formerly stood for *genteel*, for in old print of 1620 we read of a "Gentile [pronounced *genteel*] education." In all such cases, the new sound should have the new spelling.

The consonants *sh*, *zh*, being absent from Greek and Latin, nations having them were puzzled how to represent them, and insted of inventing or borrowing letters, they adopted excentric expedients. For *sh* the Germans used *sch*, which represent the sounds heard in Dutch *sch* (= *s-ch*, or *s* followed by guttural *ch*) as in Gr. σχολή, Dutch *school* (= *s·chōl*), Ger. *schule*, Eng. *school*. German *sch* also represents *sk*, as in *schrift*, Eng. *script*; and *s*, as in *schwalbe*, old Ger. *sualawa*, Eng. *swallow*. *Sch* was used in old English, but was soon shortened into *sh*, and now some reformers would have a character based on *sh*, which implies the absurdity of farther aspirating the aspirate *s* by adding *h*. Other examples of inconsistency in the use of letters were adduced.

It is time that the tyranny of the dictionaries should cease. The common people are at last aroused, demanding to be freed from nonsensical rules, and they will throw off the bonds which have hampered them so long.

Dr. J. Hammond Trumbull, of Hartford, Conn., then read a paper on "Cork, Orchel, and Jarrock."

Among the additions made to Johnson's Dictionary by its latest editor, Dr. Latham, is this:

"JARROCK [?] See Extract.
'Jarrock [is] a kind of cork or other ingredient, prohibited to be used in dyeing cloth.'—Jacob, Law Dictionary."

This vocable (in no sense, a *word*) has a curious history, which it may be worth while to trace. A statute of the first year of Richard III. (ch. 8) prohibited to dyers of cloth the use of "Orchell or Cork called *Jarecork*" ("Orchel ou Cork appellez *Jarecork*") except "the Cork made within the realm of England." In Rastell's and other early collections of English statutes,

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"Jarecork" was mis-printed as "Jarrock" (or "Iarrock") and this pseudomorph passed into the dictionaries and became a recognized "term of the law." It appears in Cowell's *Interpreter* (1607):

"*Iarrock:* anno 1 Ri. 3, ca. 8. is a kinde of cork so called."

Minsheu, in his *Guide in Tongues*, 1617, probably copied it from Cowell:

"**Iarrocke**, a kind of corke so called mentioned anno 1 R. 3, ca. 8."

In subsequent editions of Cowell, the definition became more indefinite: "a kind of cork *or other ingredient* which this statute prohibits dyers to use in dyeing cloth." Sheppard's *Grand Abridgement*, 1675, gives it as "*Jurrock*—a kind of cork." The law dictionaries of Blount (1670), Jacob, Cunningham, Tomlins, Granger, Whishaw—and perhaps others—take the term, with the definition, from Cowell or Minsheu. So did the lexicographers, Phillips, Coles, and Bailey (through more than thirty editions). Johnson *omitted* it, but Ash (1775) restored it to the vocabulary, with the remark that it was "*not much* used"! It is admitted by both Halliwell and Wright, in their dictionaries of obsolete and provincial English, on the authority of Minsheu. Skinner is the only lexicographer who seems to have troubled himself to ask—or guess—*what* "sort of cork" was "made within the realm of England," in the fifteenth century, to be used for dye stuff. In his *Etymologicon* (1671) he questioned the accuracy of Cowell's explanation of 'Jarrock,' as "*suberis* genus"; yet he quoted it—and wondered whether the English name of cork might not be derived "à *Carucio*, olim *Carace*, Corsicæ insulâ oppido, unde olim istiusmodi Suber *fortasse* advectum sit"! None of the dictionary-makers who have so carefully preserved the no-word "Jarrock" appears to have suspected that the "cork" which "Jarrock" was defined to be, was—in dictionary acceptation—no cork; that it was not "*suberis* genus," but *lichenis* genus, one of the popular names of several species of lichens or stone-mosses that yield a red or purple dye, including those which are now better known as "cudbear," "crottel," and "archil" or "orchel" lichens.

"*Corke* or *Archall*, Lichen petræus purpureus Derbiensis," is described by Parkinson (*Theatrum Botanicum*, 1640, p. 1315) as "a kind of herb much resembling Liverwort, growing in Darbyshire upon the free stones of the mountains," from which "the poor people make a kind of murrie or purplish dye." Skinner, in his *Etymologicon Botanicum*, has "*Cork*, Hepatica petræa," and "*Arcel* Hepatica petræa purpurea Darbiensis." Dr. Wallis (*Hist. of Northumberland*, i. 277) says: "Our northern people seem to call all stone-mosses that yield a red or purple dye, *Cork*." The name is, evidently, from the Gaelic *corcur*, *corcra*, 'purple' or 'crimson.' The wonder is, how it has escaped the notice of the northern provincial glossarists and the more comprehensive collections of Nares, Halliwell, and Wright. Jamieson's Scottish Dictionary gives: "*Corkes*, the ancient name of the *Lichen omphalodes*, now in Scotland called *Cudbear*. Gael. *Corcar*, the *L. tartareus*."

*Orchel*—now commonly written "*Archil*"—the name by which the coloring matter obtained from various species of lichens is known in commerce and to dyers, has been as much of a stumbling block as "Jarrock," in the way of the lexicographers. It is the Italian *oricello*, Spanish *orciglia* and *orchilla*, French *orcheil*, *ursell*, and *orseille*. The preamble to the statute of 1 Rich. 3, c. 8, mentions "*orchell* and *corke* brought from beyond the Sea, called *Jarecork*." At

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that period, the islands of the Mediterranean and the Canaries supplied most of the "orchel" used in Europe; though some, it appears, was "made in the realm of England." A statute of 3 and 4 Edw. 6, c. 2, permits the use of "good and sufficient corke or orchal," in dyeing some kinds of cloth. The *imported* orchel was, it appears, called 'Jarecork'—meaning, as I conjecture "*prepared* (A. S. *gearu*, Old Eng. *yare*) lichen," ready for the dyer's use.

*Argal* or *argol* was the popular name (still known in commerce) of the crude tartar deposited on the sides of wine casks. This was, probably, the "argoile" of Chaucer's Chanone's laboratory (C. T. 16281) which Tyrwhitt mistook for "potter's clay" (Fr. *argille*), and the "argaile" of Jonson's Alchemist (a. 1, sc. 1). Cotgrave translated Fr. *Tartre* by "Tartar or *Argall*." It was not easy to confound *orchel* with *argal*—but the lexicographers managed to do it. Cowell (*Interpreter*, 1607) gave, with reference to the statutes above-mentioned,—

"*Orchel*, *Orchall*, seemeth to be all one with cork";

and Minsheu repeats: "it seemeth to be all one with cork." Phillips (*New World of Words*) leaves it in this shape:

"*Archal;* otherwise called Darbyshire Liverwort," &c.
"*Corck*, or *Orchal;* a kind of blue colour used in painting."
"*Orchel* or *Orchal;* a certain stone like allum wherewith dyers use to colour red. In some old statutes it seemeth to be the same thing with Cork."

The editors of Cowell amended his definition. The edition of 1727 has: "*Orchel*, *Orchal*, seems to be a kind of cork, *or rather a kind of stone* like allum, which dyers use in their colours." Coles and Bailey have "*Archal*, Derbyshire Liverwort" and "*Orchal*, a stone like allum," etc.—to which Bailey adds "*Argal*, Tartar, or hard lees," etc., and "*Orgal*, the lees of wine, dried and prepared, used by dyers," etc. Johnson omitted *Archal*, retaining (on the authority of Ainsworth) *Orgal*, "lees of wine," and *Orchal*, "a stone from which a blue colour is made." Sheridan and Walker copied Johnson. Of the Law Dictionaries, Cunningham's adopted the absurd un-definition of Cowell's—"a kind of cork, or rather a kind of stone"; Jacob's omitted the "cork"; Tomlins followed Jacob; and Whishaw copied from Cowell and Blount.

To recapitulate: *Jarrock*, which since 1607 has been recognized as a term of English law and admitted to English dictionaries, solely on the ground of its occurrence in a statute of the first year of Richard III., does *not* occur in that statute, or in any other. *Jarecork*, the term for which "Jarrock" was substituted by a printer's or copyist's error, is not to be found in English dictionaries. The "Cork" which "Jarrock" has been uniformly defined to be is *no* cork that is known to English lexicography. The "Orchel," which seemeth to be all one with the cork, is not—as the law dictionaries make it—identical with *Orgal* or *Argol*, crude tartar, and the latter is not "a kind of stone" nor is it mentioned in any of the ancient statutes to which the law dictionaries refer, under *Orchel*.

A paper by Professor Lewis R. Packard, of Yale College, New Haven, Conn., entitled "Notes on certain passages in the Phaedo and the Gorgias of Plato," was read by Dr. C. R. Lanman.

The passages commented on in the Phaedo were as follows: 61 C (suggesting εὐηνῳ· instead of εὐηνῷ;), 71 D (the use of τὸ τεθνηκός where τὸ τεθνάναι would

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ordinarily be used), 78 A (*αὐτούς* taken as the object of *ζητεῖν*), 79 D (on the reading *ὥσπερ* for *περί*), 86 E (on *ἤ* followed by *δέ*), 89 C and 107 A (on the general meaning of *ἕως ἔτι φῶς ἐστίν* and *τὸν παρόντα καιρόν*, without reference to the death of Sokrates on that day at sunset), 97 D (*αὐτοῦ* in apposition with *ἐκείνου*), 99 C (that *τοῦ* belongs with *τεθῆναι* rather than with *κεῖσθαι*), 101 E (on the phrase *ἱκανοὶ δύνασθαι*).

Those in the Gorgias were 453 C (on Woolsey's suggestion of *ἢ οὔ* for *καὶ ποῦ*), 461 B (on the negatives in the sentence *ἢ οἴει—διδάξειν*;), 471 D and 486 C (on the probable irony in both places), 506 C–507 A (on the form of this mimic dialogue), 511 D (on the meaning of *καί* before *τὰ σώματα*), 512 A (to show that *οὐ* before *εἰ μέν* belongs not to *βιωτέον ἐστίν* alone, but to the whole statement in the two contrasted clauses taken together).

Professor W. W. Goodwin, of Harvard College, next presented a paper on "Greek Expressions of Future-perfect Time."

The object of this paper is to correct a common error, by which the Greek aorist subjunctive is said to express the same time as the Latin future-perfect indicative if it depends on a verb of future time. Thus *ἐὰν τοῦτο ποιῶσιν, φοβήσομαι* is said to be equivalent to *si hoc facient, timebo;* while *ἐὰν τοῦτο ποιήσωσιν, φοβήσομαι* = *si hoc fecerint, timebo.* The aorist subjunctive being thus made to some extent a *past* tense, so far at least that it denotes time past to the leading verb, it is said that when the leading verb is present the aorist subjunctive denotes time *actually* past, so that *ἐάν τι μάθωμεν, κέρδος ἐστίν* means *if we have (ever) learnt anything, it is a gain.* The same force *mutatis mutandis* is ascribed to the aorist optative. This view is distinctly stated by Krüger, *Griech. Spr.* § 53, 6, Anm. 5: "In rein und gemischt hypothetischen Sätzen haben der Conjunctiv und Optativ des Aorists regelmässig die Bedeutung der Vergangenheit, die aber, in sofern der Hauptsatz ein Zukünftiges bezeichnet, als erst bei dessen Verwirklichung eingetretene Vergangenheit vorgestellt wird, so dass diese Modi auch dem Lat. *futuro exacto* entsprechen." It is probably on the same principle that Professor Sewall (*Transactions* for 1874, p. 81) says of *ἢν προσμίξωσι* in THUC. ii. 39, that "it is in the past, not future."

It is well known that the Greek has a future-perfect tense, which is competent to do the work done by the Latin future-perfect; but it is found only in a few verbs, and in the active voice it must generally be expressed by a cumbrous periphrasis, so that practically the tense is of little account in Greek syntax. The Greek perfect subjunctive is the nearest equivalent of the Latin future-perfect in dependent sentences, as *ἐὰν τοῦτο πεποιηκὼς ὦ, si hoc fecero;* the perfect optative here is future-perfect in time, as *εἰ τοῦτο πεποιηκὼς εἴην, if I should* (in the future) *prove to have done this.* But these forms are cumbrous, and their use was avoided. The Greeks here sacrificed exactness to harmony, and thus made little use of a distinction which in Latin was constantly observed. It is now assumed in the theory above stated that the aorist subjunctive at least partly fills this vacant place in Greek and expresses future-perfect time. But if *ἐὰν τοῦτο ποιήσω* means *if I shall have done this,* how does it differ from *ἐὰν τοῦτο πεποιηκὼς ὦ,* which clearly has this meaning? The truth seems to be, that the aorist subjunctive *of itself* never expresses future-perfect or perfect time, so that *ἐὰν τοῦτο γένηται* can mean only *if this shall happen* or *if this ever happens.* This tense may, however,

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be introduced by a particle, like ἐπειδάν, *after that*, which throws the whole expression into time past relatively to the leading verb; so that ἐπειδὰν εἴπω means (according to the leading verb) *when I shall have spoken* or *when I have spoken*. But there is no more past force *in the verb* here than there is in μετὰ τὸ ἐμὲ εἰπεῖν, *after my speaking*. Further, as the leading verb on which a conditional subjunctive depends can never be strictly present, it follows that the subjunctive can never denote really past time. In fact we shall never find any evidence that such a subjunctive has by its own force what Krüger calls "die Bedeutung der Vergangenheit," which on all other grounds seems utterly opposed to the whole character of the Greek subjunctive.

It thus appears that the Greek often uses a less exact form, like ἐὰν τοῦτο ποιήσω, *if I shall do this*, where the Latin would use the more exact *si hoc fecero*, *if I shall have done this*. In many such cases it may be doubted whether the use of the English future-perfect in translating the Greek aorist subjunctive is objectionable, provided the context shows clearly that the subjunctive refers to time past relatively to the leading verb. But this is very different from maintaining that this past force is actually found in the aorist subjunctive itself. Instructive examples in this matter are ὃν μὲν ἴδῃ... κἂν... πεπόνθῃ, PLAT. Rep. ii. 376 A, and ὅταν μὲν τιθῆσθε... ἐπειδὰν δὲ θῆσθε, DEM. Mid. § 34, p. 525, 11.

To this paper was added a remark supplementary to the paper on "Shall and Should" printed in the *Transactions* for 1876.

A confirmation of the opinion that the optative in protasis, when it is not past and generic, is properly designated as one of the forms used for expressing a future condition, and that this futurity is its most marked characteristic, is found in the criticism of Dionysius of Halicarnassus on THUC. ii. 39: εἰ ῥᾳθυμίᾳ μᾶλλον ἢ πόνων μελέτῃ... ἐθέλοιμεν κινδυνεύειν, περιγίγνεται ἡμῖν, κ. τ. λ. Dionysius criticises ἐθέλοιμεν as an instance of ἡ παρὰ τοὺς χρόνους τῶν ῥημάτων ἐκβεβηκυῖα τὸ κατάλληλον φράσις, and makes the objection that ἐθέλοιμεν is a verb of future time while περιγίγνεται is one of present time. The difficulty would have been removed, he thinks, by substituting περιέσται for περιγίγνεται, thus making the apodosis also future. See DION. HAL. de Thucyd. Idiom. 12 (p. 801 R.). Most modern scholars would criticise this sentence in a different way; and most editors accept the more regular reading ἐθέλομεν on poor authority, in spite of the quotation in Dionysius.

The Association thereupon adjourned till Thursday morning.

### THIRD DAY—THURSDAY, JULY 12.

### MORNING SESSION.

The Association met at 9 o'clock, the President in the chair.

The Auditors of the Treasurer's Report reported that they found it correct; and it was, on motion, accepted.

A paper by Professor Edward S. Holden, of the Naval Observatory, Washington, D. C., on "The Vocabularies of Children under Two Years of Age," was read by Professor W. D. Whitney.

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The writer had been led, in the sequel of an inquiry earlier made into the extent of vocabulary of an educated adult, to examine further the subject of the extent of vocabulary commanded by young children. He chose the twenty-fourth month, the last of the second year, as the period for examination. All words actually used during that month were carefully noted down, with rigid exclusion of doubtful words, of nursery rhymes learned and repeated, and so on. The work was done by Professor Holden himself for two children, and he reports the results of a like work done by a friend upon a third child. The particular period was chosen because after the end of the second year words are learned so fast that the collection is attended with greatly increased difficulty and uncertainty. The collected vocabularies were presented in full, classified both as parts of speech and according to their initial letters, since it seems clearly apparent that early acquisition of words is determined in no small part by their comparative ease of pronunciation. The general results (omitting here the classification by initial letters) are as follows:

| Children. | I. | II. | III. |
|---|---|---|---|
| Nouns, etc., | 285 | 230 | 114 |
| Verbs, etc., | 107 | 90 | 29 |
| Adjectives, etc., | 34 | 37 | 13 |
| Adverbs, etc., | 29 | 17 | 6 |
| Miscellaneous, | 28 | 25 | 11 |
| Totals, | 483 | 399 | 173 |

The third child was one who showed considerable backwardness in utterance.

The result of Professor Holden's inquiry into the adult vocabulary (of which the method was fully explained) had been to show that an educated man has at command, out of those words which are admitted into Webster's Unabridged Dictionary (edition of 1852), about thirty-five thousand words, besides technical words, scientific and proper names, etc., not included in that work.

Professor William B. Carr, of Petersburg, Virginia, read the next paper, on "Virgil's Views of the Collective Noun."

The writer eulogized the poet's style, basing its superiority, in part, upon the philosophical accuracy of his powers of observation. He said that there was a single illustration of the manner in which Virgil employed these powers, on which he wished to remark. It was to be found in his peculiar method of contemplating and employing the collective noun. He then proceeded:

Other writers seem to take a photographic view of the object in nature before them, and reduce it immediately to a stereotype plate, which they lay away for any and every subsequent occasion. With Horace, the collective noun is always a unit; for example, *turba*, and *genus*, and *juventus* are each a unit; and, in the CARMEN SAECULARE, *chorus* is not only a unit, but a unit of the first grammatical person, an *ego: turba certat, genus haesit, juventus audiet, chorus reporto.* Of the thirty instances of the use of the collective noun in the lyric verses of Horace, every one corroborates this view. With Ovid, it is believed that the case is just the reverse; though it is hardly probable that he is as strict in the use of the plural as is Horace in the use of the singular.

Even nations seem to take this permanent view of some, at least, of the collective nouns. With English-speaking thinkers, *the people* has always been a many-sided

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monster of the plural number; exceptions may be found in the Authorized Version of, the Old Testament, and in the speeches of Kossuth. In France, on the other hand, *le peuple* is always singular; and so was the *Populus Romanus*, and even the *Senatus Populusque Romanus*.

But not so with our more philosophic author of the Aeneid. An enthusiastic admirer of nature, he lays bare his senses to her charms and receives his impressions fresh from their source. These impressions must, of course, vary according to standpoint and distance. Let a material object present itself to his philosophic eye, and though it be a group, a parcel, a collection, a multitude, he views it as a unit; but if he have occasion further to contemplate it and to take a second impression, his attentive gaze continues, the several objects that compose the group begin to assume a distinct individuality of their own; and there now come into view *men*, where before there was only a *crowd*, a *mob; soldiers*, where there had been but an *army; ships and sailors*, instead of a *fleet;* busy *bees* and industrious *ants*, instead of a teeming *swarm* or a black *battalion*.

Such being our poet's views of this matter, what are we naturally to expect, when such a noun is used as a subject, but that the first predicate verb, and accompanying adjective if there be one, shall be in the singular number, in accord with the noun as a unit; and that those verbs that follow in the same period, whether one or several, together with any accompanying adjectives or participles, shall all be plural? Now, in point of fact, *such is the case;* such is Virgil's usage, occasionally modified by the exigencies of the versification, but obviously his usage, whenever he is left free to exercise his preference.

Attention was then invited to a number of periods of the AENEID, as follows, in proof of this position:

I. 148–152, where *vulgus* is attended by *saevit* and *silent*, etc.

II. 31–32, where *pars* is followed by *stupet* and *mirantur*, although the poet was under no metrical constraint to write *mirantur;* for the quantities left him at the most complete liberty to choose between *miratur* and *mirantur*.

II. 62–63, where *juventus* is followed by *ruit* and *certant*.

II. 214–219, where *serpens uterque*, tantamount to a collective noun, is followed first by two singular verbs and then by several plural ones.

III. 675–676, where *genus* is followed by *ruit* and *complent*.

IV. 86–88, where *juventus* is followed by *exercet* and *parant*. Here, as elsewhere, the numbers might have been reversed, or both verbs might have been singular, or both plural, without other change in the hexameters; so that the poet, being perfectly unconstrained by the requirements of the meter, has followed his canon and indicated his choice.

Other passages of the AENEID, cited in proof and commented upon, are the following:

V. 33–34; VII. 573–576; VII. 687–689; IX. 74–75; XI. 608–610; XI. 880–883; XII. 122–124; XII. 562–564; and, on the best manuscript authority, though not according to the common editions, VI. 7–8; and IX. 174–175; (sixteen, in all.)

It was then shown that, in all those passages of this poem in which there were apparent exceptions to the rule, the departures arose from the fact that the requirements of the verse had obstructed the ready application of the rule, and in three instances modern editors had changed the text to suit their own notions of accord or of taste. These are:

I. 284–285; III. 233–234; V. 134–135; V. 862–863; VI. 218–219; VII. 813;



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IX. 607–608: and, in VI. 642–644, it was suggested that plurals were employed in acknowledgment of the immense numbers of spirits seen by the hero in the several groups of the blessed in Elysium; a sort of *pluralis excellentiae* in the form of the verb.

In the only passage of the GEORGICS bearing upon the question, III. 382–383, the common editions have *gens* followed by *tunditur* and *velatur;* but the better authorized reading, and doubtless the correct one, is *velantur.*

The writer would turn this well established peculiarity of style to more practical account, and endeavor by means of it to settle the question of genuineness among certain doubtful readings, e. g.: between *monstrat* and *monstrant*, AE VI. 8; between *exercet* and *exercent*, AE. VI. 642; and between *velatur* and *velantur*, G. III. 383; and also would decide, by this means, the question of the authenticity of the CULEX, CIRIS, COPA, CATALECTA, MORETUM,* and HORTULUS, all of which have been attributed to Virgil, but whose authenticity is still matter of doubt. But inasmuch as there is not in these pieces a single period bearing upon the canon, we are limited to the usual means, internal and historical evidence. It is worthy of remark that the BUCOLICS have not this criterion, and yet are, without question, genuine. As to the twenty other pieces of the doubtful list, sometimes attributed to Virgil, few scholars, if any, of the present day seriously believe that any part of it, unless it be the DIRAE, was written by Virgil.

The author feels at liberty, therefore, to assert that Virgil must have laid down a rule for his own guidance in the premises, and that that rule may be reproduced in the form of the following CANON:

*In connection with a collective noun as a subject, use first a singular verb; and then, if the predicate be compound, the verb or verbs that follow must be plural: never reverse this rule under any circumstances; but always adhere to it, unless when overridden by the stern exigencies of the meter.*

Professor W. D. Whitney, of Yale College, next read a paper on "Cockneyisms."

Professor Whitney said that the so-called "Cockney" peculiarities of pronunciation, in regard to *v* and *w*, and the use or omission of initial *h*, have generally been viewed as direct interchanges—the *h*, for example, being added to all words where it does not belong, and dropped from all words which have a right to it. As such interchanges, they would seem to have a certain analogy with the conversions of "Grimm's Law." He had himself formerly referred to them as possessing that value; and recently an English writer, Mr. Douse, has founded chiefly upon them an elaborate attempt at explaining the phenomena of Grimm's Law, assuming a "dissimilating sentiment," followed by a "cross compensation," and so on. But it appears extremely doubtful, on careful consideration, whether there is any real analogy between the two classes of phenomena.

The English author referred to, following Marsh, brings up as an analogue to the Cockney practices a habit well known in America: namely, the leaving off and putting on of a final *r;* the saying of *doah* for *door* and of *lor* for *law*, and so on. Every one must have noticed the frequency of the added *r* among uneducated people, and especially among children. It would certainly be wrong,

* In this little poem occurs the phrase "*e pluribus unus.*"

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however, to regard this as a case of interchange. The explanation seems to be plainly as follows. Among no small part of the English-speaking people, both in America and (by the testimony, for example, of Mr. A. J. Ellis) in England, no *r* is sounded at all unless it is directly followed by a vowel. So, in *for* and *color* the final *r* is entirely silent, and the words are sounded at the end precisely like *law* and (dialectic or vulgar) *holla* and *follow*. But the addition of another word or a derivative syllable beginning with a vowel brings the *r* to utterance, like the linked final consonant of a French word; and in *for and against* and in *coloring* the sound of *r* is plainly enough heard. The uneducated or unpracticed speaker, now, knows of no ground of difference between *for* and *law;* and so, misled by a false analogy, he avoids a hiatus by adding the same *r* to *law* in *lor and gospel* and the like; and he says *hollering* and *follering*, like *coloring*. There is nothing here that can be called an interchange; there is only a confusion in certain classes of cases, growing out of a general weakening and loss of the sound apparently interchanged.

As regards the two Cockneyisms referred to, there is a lamentable deficiency of authentic and trustworthy information. Neither of them is, so far as known, to be met with in this country; and our authorities on the subject are "Punch" and Dickens and their like—the humorists and caricaturists. Nor does Mr. Douse, though more favorably situated, appear to have any others to rely upon, or to have made any scientific study of the usages which he lays at the foundation of his theories. He contributes, however, one telling item of information as to the *h*-Cockneyism, which seems quite to deprive it of all value for his purposes, and to betray its true character. "With respect to the *h*," he says, "I have frequently observed a fact of some interest, viz., that when a group of deaspirators are talking together, an *h* is rarely heard at all; but when any of them begin to talk with people of superior education, they also begin to adorn their conversation with a frantic sprinkling of *h*'s in the wrong places, in order (as they suppose) to adapt their language to their listeners." It is readily seen what inferences we are to draw herefrom, and how far there is an analogy between this incorrectness of speech and the one already described, relating to the *r*. In both we have a widely extended loss of an element formerly uttered; but here, instead of a phonetic situation, causing a restoration of the lost letter in some cases, and its insertion by false analogy in others, we have a consciousness on the part of the speakers that in this particular they are talking wrongly and vulgarly; and, as a consequence of this, an unenlightened occasional attempt at bettering their practice. The necessary result is that their *h*'s are to no small extent "sprinkled in in the wrong places"; and also, of course, that a corresponding proportion of those which should be put in are still omitted. This naturally enough makes upon a correct speaker the general impression of an interchange; because his attention is more struck by the violations of correct speech, of either kind, than by the cases of conformity with his own practice: he distinctly hears every *h* wrongly added, and as distinctly misses every one omitted, while hardly noticing the others.

This is not put forward as a final or authoritative explanation; present information is too imperfect to permit that; but it is believed to satisfy better than any other the facts hitherto made known. And it is at least extremely probable that the asserted interchange of *w* and *v* is only a confusion of somewhat the same character, growing out of a dialectic rejection of the *v*-sound. If the case

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is not so, the London phonologists have it in their power to demonstrate the contrary. But until this is done, at any rate, we have a right to distrust the capacity of the Cockneyisms in question to contribute anything toward the solution of that greatest of phonetic mysteries, the *lautverschiebung*, or rotation of Germanic mutes.

Mr. William S. Liscomb, of Providence, R. I., then presented a paper on "Forms of Assibilation."

The object of the paper was to trace the development of the *sh* sound, which is not primitive, but of later growth in all languages. Its nature is that of a *Zwischenlaut*, or "intermediate sound," standing between two others and gradually coming to take their place. We find its analogy in the vowel scale in the replacement of *ai* by *e* (as Sanscrit *vêda* for ***vaida***), and of *au* by *o* (as Latin *Cl**au**dius* and *Cl**o**dius;* Latin *c**au**sa*, Italian *c**o**sa*).

Assibilated forms fall into two classes:

*First*, those in which the result of the assibilation is a simple sound, *sh* or *zh*, the two holding to each other the relation of *surd* and *sonant*. The former lies between and replaces (1) *sy*; e. g. *sure* ( = ***sh**oor*), originally = ***sy**oor* (Eng. *y* being *yoo*); *luxury* ( = *luc**sh**oory*), originally = *luc**sy**oory:* (2) *sχ* (*χ* representing the palatal heard in German ***ich***); e. g. A.-S. *fi**sc**, **sc**rift*, flattening out into *fi**sχ**, **sχ**rift*, give Eng. *fi**sh**, **sh**rift*. In like manner the sonant *zh* comes from *zy;* e. g. *azure* (= *a**zh**er*), originally = *a**zy**oor*. When *zh* comes from *sy*, *s* first passed into *z*.

The vowels *e* and *i*, standing between *s* or *z* and a following vowel, naturally sink to *y* and with the *s* or *z* form *sh* or *zh*. Thus A.-S. *blȳsiun*, becoming *blȳsyan*, gives Eng. *blu**sh**;* *nau**se**ous*, (originally = *nau-se-ous*), becoming *nau-s**y**ous*, gives *nau**sh**ous* (as pronounced). So *fusion*, *grazier* (once *fu-zi-on*, *gra-zi-er*), gave *fu**zy**on*, *gra**zy**er*, and finally *fu**zh**on*, *gra**zh**er*.

Similarly, *e* and *i* between *sc* or *sk* and a following vowel sink to *y*, the *c* and *k* flatten into *χ* and drop out, and *sy* forms *sh*. The steps are as follows: A.-S. *s**ce**acan*, *fi**sce**an;* *s**cy**acan*, *fi**scy**an;* *s**χy**acan*, *fi**sχy**an*; *s**y**acan*, *fi**sy**an;* *s**h**aken*, *fi**sh**en;* *s**h**ake*, *fi**sh**.* The *y* sound was parasitic and often absent from the spelling; e. g. A.-S. *s**c**atteran*, *s**c**ulder*, Eng. *s**h**atter*, *s**h**oulder*. This explains words like A.-S. *s**c**eddan* and *s**c**iftan*, Eng. *s**h**ed* and *s**h**ift*, in which *e* and *i* are not followed by a vowel. *Sh* has often come from *ti*, *ce*, *ci*, as in *na**ti**on*, *o**ce**an*, *spe**ci**al*. This occurs mostly in French words, in which these letters had already acquired the *sy* sound as in modern French.

*Second*, those in which the result of the assibilation is a compound sound, *tsh* and *dzh*, as heard in *bea**ch**, bri**dg**e*. These, which again are relatively surd and sonant, lie between and replace *ty* and *dy* respectively, the *t* and *d* being drawn backward into the mouth and the *y* being drawn forward to *sh*. It must be borne in mind that the *t* and *d* in the combinations *tsh* and *dzh* are not the ordinary *t* and *d*, being formed farther back toward the throat, instead of on the gums of the upper front teeth. Here, too, the vowels *e* and *i*, sinking to *y*, often produce the assibilation. Thus A.-S. *fe**t**ian*, becoming *fe**ty**an*, gives Eng. *fe**tch**;* *sol**di**er* (originally *sol-di-er*) passed through *sol**dy**er* to *sol**dzh**er* (as pronounced); Lat. *di**u**rnus*, sinking to *dy**u**rnus*, gives old Fr. *jorn* and *jor*, Eng. *jour*(*nal*) (old Fr. *j* and soft *g* = *dzh*).

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In Romance words *dzh* has often come from a primitive *y* sound which has taken before it a parasitic *d*. Thus Latin *juvenis, juxtare* (= *yuvenis, yuxtare*), became *dyuvenis, dyuxtare*, whence old Fr. *jone, juster* (the new Fr. *jeune, jouter*).

More frequently *tsh* and *dzh* have come from *cy* (or *ky*) and *gy* respectively, the *c* and *g* first passing into *t* and *d*, as in Gr. πράκγω, πράττω (through πράτγω), Lat. *suspicio, suspitio*. This change is due to the difficulty of forming *c* and *g* when the back of the mouth is set for *y*, the desired letter being made with the front of the tongue, which gives *t* and *d*. Thus A.-S. *bealcian, hegian*, pass through *bealcyan, hegyan*, to *bealtyan, hedyan*, then into Eng. *belch, hedge*. The *y*, which is parasitic, is often absent from the spelling; e. g. Lat. *carn-em*, becoming *cyarn-em*, gives old French *charn* and *char* (the new Fr. *chair*). This explains words with the vowels *e* and *i*, like A.-S. *cild, cese*, passing through *cyild, cyese*, and *tyild, tyese*, to Eng. *child, cheese*.

Besides these well defined classes, considerable irregularity exists in the origin of many assibilated forms. The different classes also cross into each other, as *cabbish* and *cabbage, partridge* and *partrich, debosh* and *debauch*. Italian shows assibilation of *di* to *dz*, as Lat. *radiom*, It. *razzo* (*zz* = *dz*), and Italian and German show assibilation of *ti* to *ts*, as Lat. *nation-em*, It. *nazione* (*z* = *ts*), Ger. *nation* (*ti* = *ts*). There is no sufficient evidence that either Latin or Greek anciently possessed true assibilation, though the former developed it at a later date.

In High German assibilation appeared in the eleventh century and has had a somewhat irregular development. Among the Low German dialects it is known only to English and Platt-Deutsch, and as a dialectic variation of the old Frisian. It is an important element in all the Romance and the Slavonic languages.

Professor C. H. Toy, of the Southern Baptist Theological Seminary, Louisville, Ky., read the next paper, on "The Nominal Basis of the Hebrew Verb."

As far as regards the form, the Hebrew verb is ultimately identical with the noun. Certain words, as *ayin, yesh*, are obviously substantives, the imperative is a noun without pronominal suffixes, the ground-form of the verb, *kataba*, has simple nominal inflection of gender and number, and the derived forms arise by prefixes (*ua, ta, ha*, etc.) and in other ways after the manner of nouns. In the imperfect the third masculine singular (*yi.ktob* = *ya.ktubu*) comes from the stem by prefix *ya*, which is by some regarded as the third personal pronoun; but this is against the analogy of the perfect (which has no pronominal addition in third singular masculine), and no such masculine personal pronoun as *ya* is found, while nouns are often made by this prefix and the imperfect resembles the noun in its endings (*a, i, u*) and otherwise. In the third singular feminine the prefix *ta* resembles the feminine sign, but its position is against identifying it with this, and rather points to the nominal prefix *t*. The second and first persons are made by prefixed pronouns, the second singular feminine has the feminine addition *i* or *i.n*, the plural of the third and second has the nominal plural terminations. The internal vowel-changes and the significance of the *ya* and *ta* are difficult to explain. The preference of the imperfect for prefixes springs perhaps from the fact that the ground-form (*ya.ktubu*) is thus made. The nominal base seems to be concrete.

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In the syntax also a prominence of the nominal conception is visible. Besides the preference for infinitive constructions, there is the peculiar use of the participle in circumstantial clauses, the occurrence of the verb after a noun in *stat. const.* and as qualifying a noun (imperfect), the descriptive imperfect with ו alongside of the infinitive with ל, the general development of the verb (without elaborated temporal and modal forms), the paucity of conjunctions, and the statuesque character of the syntax.

The Committee to nominate officers for the next year presented nominations as follow:

For *President*—Professor B. L. Gildersleeve, Johns Hopkins University, Baltimore, Md.

For *Vice-Presidents*—Professor J. B. Sewall, Thayer Academy, Braintree, Mass.; Professor C. H. Toy, Southern Baptist Theological Seminary, Louisville, Ky.

For *Secretary and Curator*—Professor Samuel Hart, Trinity College, Hartford, Conn.

For *Treasurer*—Mr. Charles J. Buckingham, Poughkeepsie, N. Y.

For additional members of the *Executive Committee*—

Professor Frederic D. Allen, University of Ohio, Cincinnati, O.

Professor W. W. Goodwin, Harvard College, Cambridge, Mass.

Professor F. A. March, Lafayette College, Easton, Penn.

Dr. J. Hammond Trumbull, Hartford, Conn.

Professor W. D. Whitney, Yale College, New Haven, Conn.

The report was accepted, and the persons therein named were declared elected to the offices to which they were respectively nominated.

On motion,

*Resolved*, That the thanks of the Association be given to the Secretary and Curator for his past services; and that, in consideration of the labors of his office, he be allowed the coming year the sum of $200 for clerical assistance.

On motion, the Executive Committee was desired to take into consideration the propriety of setting a limit to the time within which abstracts or papers may be sent to the Secretary for publication.

A recess was then taken till 2½ o'clock.

### AFTERNOON SESSION.

The Association met after the recess.

Professor W. D. Whitney presented a report from the Committee appointed in 1875 to take into consideration the whole matter of the Reform of English Spelling, and continued after its Report in 1876 for one year with Professor F. A. March as Chairman, as follows:

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The attempt to prepare an English alphabet according to the principles laid down in the Report of last year brings out the following facts.

1. There are eighteen Roman letters which commonly represent in English nearly the same elementary sounds which they represented in Latin: *a* (*fa*ther), *b*, *c* (*k*, *q*), *d*, *e* (m*e*t), *f*, *g* (*g*o), *h*, *i* (p*i*ck), *l*, *m*, *n*, *o* (*g*o), *p*, *r*, *s* (*s*o), *t*, *u* (*f*ull).

2. The consonant sounds represented in Latin by *i* and *u* are now represented by *y* and *w*, and the sonants corresponding to *f* and *s* are now represented by *v* and *z*.

3. There are three short vowels unknown to the early Romans which are without proper representatives in English, those in *fat*, *not*, *but*.

4. There are five elementary consonants represented by digraphs: *th* (*th*in), *th* = *dh* (*th*ine, *th*en), *sh* (*sh*e), *zh* (azure), *ng* (si*ng*); to which may be added *ch* (*ch*urch), *g* (*j*).

It seems best to follow the Latin and other languages written in Roman letters, in the use of a single sign for a short vowel and its long, distinguishing them, when great exactness is required, by a diacritical mark.

The alphabet would then have thirty two letters.

Twenty-two of these have their common form and power as described above in statements 1 and 2.

The three vowels in *fat*, *not*, *but* need new letters. Without laying any stress on the exact form, it is recommended to try some modifications of *a*, *o*, and *u*, such as ɑ, ɔ, ʊ.

For the consonants now represented by digraphs new letters would be desirable, but no particular forms are now recommended. The following are mentioned: *đ*, đ (then); þ, ƀ (thin); f, ſ (sh); ʒ (zh); ŋ (ng); č (ch).

The use of these letters with only these powers and the dropping of silent letters will so change the look of large numbers of words that they will not be recognized at sight. It seems necessary therefore that there should be a transition period, and for that the following suggestions are made:

1. Transition characters may be used resembling, if possible, two letters:

| | | | | | | |
|---|---|---|---|---|---|---|
| For *a* in *fate*, | ɑ | may be used in place of | | | | ɔ. |
| " *e* " *mete*, | ɐ | " | " | " | | I. |
| " *i* " *fine*, | ị | " | " | " | | ai. |
| " *u* " *pure*, | ü or ụ | " | " | " | | iu. |
| " *s* " *as*, | ȝ | " | " | " | | z. |
| " *g* " *gem*, | ɡ | " | " | " | | j. |
| " *c* " *cent*, | ç | " | " | " | | s. |

2. The digraphs now representing single consonants may be named and otherwise treated as single letters.

3. New letters can be easiest introduced by using them only for the old letters which they resemble in form.

4. Long words bear changes best, and vowels are more easily changed than consonants, which project more above and below the line. Dropping final silent *e* is the easiest change.

On motion, it was

*Resolved*, That the Committee on the Reform of English Spelling be continued for one year.

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The Committee on the place and the time of the next meeting recommended that the next meeting be held at Saratoga, N. Y., on Tuesday, July 9, 1878, unless the Executive Committee shall deem it advisable to fix upon some other place of meeting, in which event reasonable notice shall be given by the Secretary.

On motion, the report of the Committee was accepted, and the recommendation therein contained was adopted.

On recommendation of the Executive Committee, it was

*Resolved*, That the Association expects, in the interest of the timely appearance of its publications, that members who have read papers before it will be prompt in sending abstracts and copies of such papers at the call of the Secretary; and it authorizes and requests the Secretary to wait no longer than till October 15th for any abstract for the Proceedings, and no longer than till January 1st for the copy of any paper intended for the Transactions, but to close up and issue the Proceedings and the Transactions as if the delinquent papers had not been presented.

Dr. J. Hammond Trumbull presented a paper on "A Mistaken View of the Process of Word-making in the American Languages."

Major John W. Powell laid before the Association his Notes and Vocabularies of Indian Dialects.

On motion, it was

*Resolved*, That the thanks of this Association are hereby tendered to the Trustees and the Faculty of the Johns Hopkins University for the use of their buildings and their courteous attentions during this meeting, to the Local Committee for the completeness of their arrangements for our comfort, to those citizens of Baltimore who have contributed to our entertainment, and to the Athenaeum Club for the handsome reception given us in their rooms.

On motion, the Association adjourned.

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## 1877-78.

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SAMUEL HART.

TREASURER.

CHARLES J. BUCKINGHAM.

EXECUTIVE COMMITTEE.

The officers above named, and—

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W. W. GOODWIN,
F. A. MARCH,
J. HAMMOND TRUMBULL,
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